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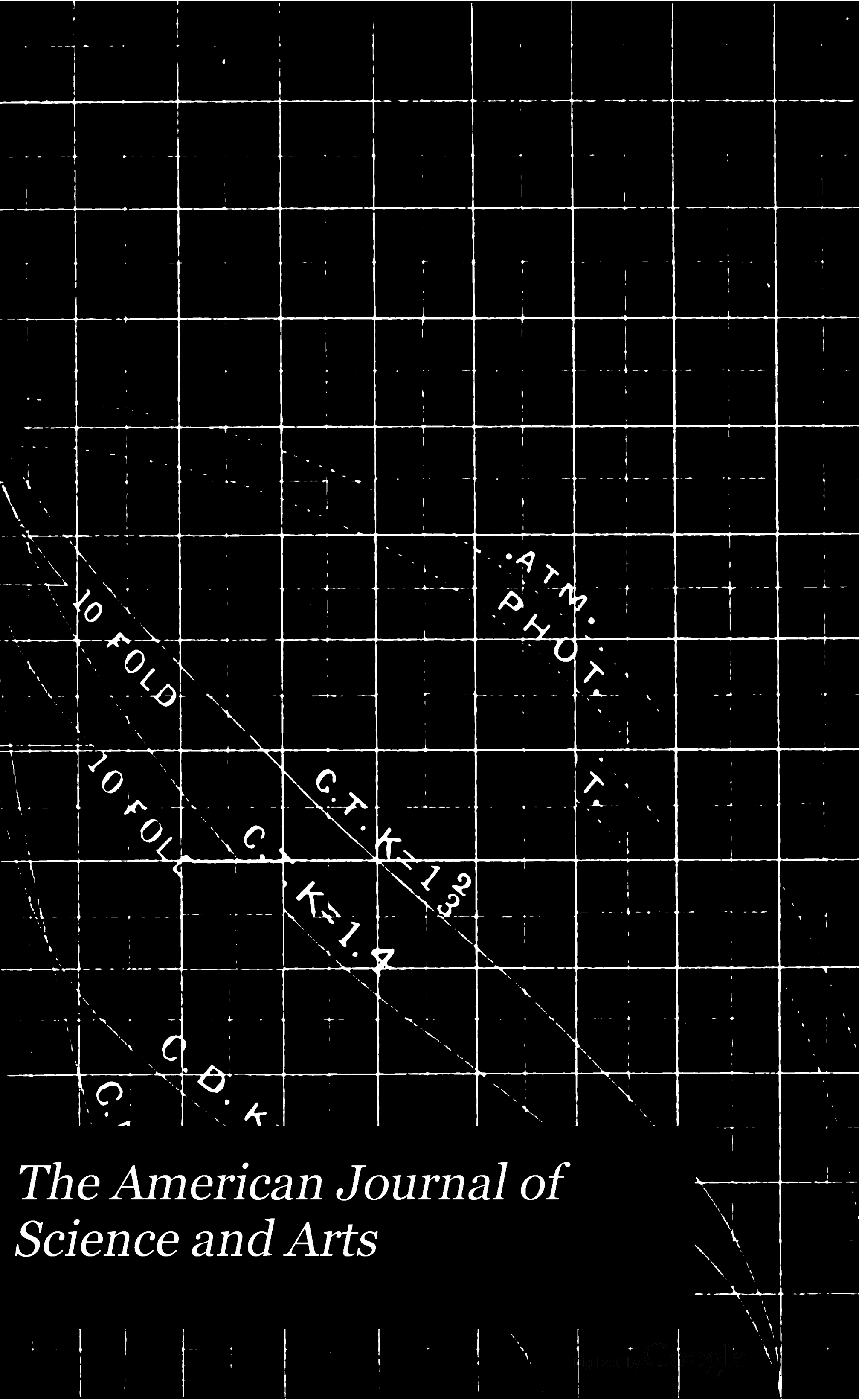

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[SECOND SERIES.]

ART. I.—*Alexander von Humboldt, his early Life, his Education, his Writings, and his Books;* by HENRY STEVENS of Vermont, F.S.A. etc., 4 Trafalgar Square, London.

THE higher the sun the shorter the shadow; even so the greater the eminence of a philosopher the briefer need be his eulogy by the unphilosophical. Nevertheless so exceptional was Humboldt in himself, his early training, his achievements, it may not be deemed out of place here, though perhaps at the expense of wearying the unlearned who need not know so much, or worrying the learned who know it already, to recapitulate a few of the well known points in the life, education, and character of this illustrious man.

If the indifferent reader will run his quick eye over the titles of the seventeen thousand volumes recorded in the Catalogue of his Library he will, no doubt, see at a glance that in very many respects, it is the most extraordinary collection of modern

---

[The above article has been contributed to the Journal at our request by Mr. Henry Stevens, a gentleman of well known bibliographical attainments, who purchased the library of Humboldt, not long after his decease, and who has arrived at many interesting conclusions in respect to the distinguished scientist, based chiefly upon the examination of his literary and scientific collections. It was originally written as a preface to a Catalogue of the Library prepared under the direction of Mr. Stevens. Owing to a change in Mr. Stevens's plan respecting its publication, some slight alterations have been made in it, but not in any way to affect the comments upon Humboldt.—EDS.]

scientific books, especially those pertaining to the physical sciences, ever brought together. It is true, many of the common every-day standard works of this class such as 'no library should be without,' are wanting here, particularly the long sets of scientific periodicals, the transactions of learned societies and other equally valuable and bulky books; but when it is remembered that the great philosopher, while in Paris, where for twenty years he was revising and finishing what he had done, and afterwards while at Berlin, where in his old age, in working up his master-pieces of thought, he brought back to the anvil that which he had hammered, always had at his elbow some of the best public and private libraries in France and Germany, as free to him at all times as his own, their absence from his own collection is accounted for. But the reader will observe that an unusually large proportion of Humboldt's books cannot be found in any other single library, public or private, in Europe or America, not excepting even the British Museum, the Imperial Library of Paris, the Royal Library of Berlin, or the Royal Society of London. Indeed it is a matter of fact, that on a careful examination in 1862–3, it was found that the library of the British Museum, was deficient in at least one-third of the titles entered in this catalogue, every one of which was deemed a desirable acquisition. These were mostly privately printed or unusual works. Almost every page of the catalogue contains books important in themselves, which no money can buy in the usual channels of commerce, but which in every tongue were drawn together from all parts of the world, by the irresistible gravitation of Humboldt's name.

About the year 1855 in this country an unfortunate public impression was achieved, for a purpose, and sedulously enforced by high authority, to the effect that the collection of large libraries of scientific and other books was not necessary to the *increase and diffusion of knowledge among men.* "There are two individuals," wrote a distinguished savant to the Committee of Congress, "who may, without qualification, be considered the most prominent scientific men of the nineteenth century—Cuvier and Humboldt. By what means have they given such powerful impulse to science? How have they succeeded not only in increasing the amount of knowledge of their age, but also in founding new branches of science? It is by their own publications and by aiding in the publications of others; by making large collections of specimens and other scientific apparatus, and not by the accumulation of large libraries. Humboldt never owned a book, *not even a copy of his own works*, as I know from his own lips. 'He was too poor,' he once said to me, 'to secure a copy of them'; and all the works he receives constantly from his scientific friends are distributed by him to needy students." The very historical truth was then and is now that the two best scientific private libraries in Europe belonged to Cuvier and Humboldt, the one lining the splendid salons in which the baron held those historic reunions; the other described in this catalogue which contains all the books Humboldt ever wrote, many of them thumbed, worn and annotated.

Humboldt was a fortunate child of nature. A lucky star seems to have presided at his birth. On the third of June, 1769, occurred the rarest and most important phenomenon within the whole range of mathematics and astronomy; an event more favorable of its kind than any that had been observed since the Christian era began, or would again occur for hundreds of years; an event which Rittenhouse in America, Euler in Germany and many other eminent astronomers of other countries were deputed to observe for the benefit of mankind. The last *Transit of Venus*,* the great opportunity for determining the sun's parallax, and consequently the dimensions of our planetary system and our globe, was a conjunction of planets coincident with the birth of twelve imperial men of nature, more renowned than the twelve Cæsars. No other single year probably, before or since, ever produced such men as Napoleon, Wellington, Soult and Ney; Brunel, Mehemet Aly, Turner, Sir Thomas Lawrence, Châteaubriand, and Castlereagh; CUVIER and HUMBOLDT; men who upturned the world and set it right again; who revolutionized science, art, politics, states, and the affairs of mankind. Humboldt, in many respects the most gifted of them all, outlived them all.

With his life ended, and became historic, the first grand period of modern science. The times that developed the French Revolution ripened Humboldt, and made conspicuous others, many of them his friends and fellow laborers, among whom may be named LaPlace, Lalande, Gay-Lussac, Kunth, Bonpland, Oltmans, Oersted, Bichat, Delambre, Bessel Berzelius, Davy, Robert Brown, Dalton, Herschel, DeCandolle, Latreille, Valenciennes, Audubon, David d'Angier, Arago, Gauss, Ritter, Müller, Leopold von Buch, Varnhagen von Ense, Compte, Biot, *et al.*, names themselves suggesting discoveries, inventions and unbounded knowledge.

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* *Transits of Venus* are as rare as they are important. They occur in couples, in June and December, about eight years apart, and then not again for several generations. Kepler was aware of the phenomenon and as early as 1604 announced that one would take place in 1761, but young Horrocks of Liverpool with better tables, and additional data, calculated that there would be a *transit* on the 4th of Dec., 1639. He let a friend into his secret, and they two on the day named, for Venus was punctual, were the first ever known to observe it. It was soon calculated that one must have taken place on the 6th of Dec., 1631, and another in June, 1526, and that the next would not occur till the 5th of June, 1761. But of all the transits, past and to come, the climax would be, that of the 3d of June, 1769, when Venus passed across the sun's disc very near the center. The next one, but not visible in this country, will take place five years hence on the 8th of Dec., 1874, which will be a grand one for science, considering the great advance in scientific instruments, but far inferior to the last. If, however, it produces only half a dozen Cæsars it will be a godsend to this rapid century. *Let young folks take note of the date* 1874. Another will occur on the 6th of Dec., 1882, but not again till nearly five quarters of a century later, on the 7th of June, 2004; to be followed eight years after, on the 5th day of June, 2012; to be repeated in Dec., 2117, and so on.

ALEXANDER VON HUMBOLDT was born in Berlin on the 14th of September, 1769, and died there on the 6th of May, 1859, in the ninetieth year of his age. A rapid sketch of his youth and early manhood will serve to show how well he prepared himself, single-handed, to accomplish so much; more even than most of the learned academies of which he was a member, and certainly not less than any of the great Voyages of Discovery, and celebrated Exploring Expeditions conducted at the public expense. The course pursued with him was so peculiar, and contrasts so completely with the usual course of training in the colleges of this country, even in the scientific departments, that no pains have been spared to render this account as complete and exact as possible. Unroll the scrolls of biography and no name will be found a brighter example to stimulate, encourage and direct the youthful student than that of Humboldt.

He was fortunate in his father, Major von Humboldt, who gave him wealth and position; favored in his elder brother William, who gave him the best of companionship, whom he and the world alike loved and honored through a long life; and blessed in his noble mother, to whose virtues, devotion to her boys, energy of purpose, and common sense he owed a right start both in the political world, which he disliked, and in the physical world, which he adored. The earliest tutor chosen by the mother to teach and play with the two boys was Campe the educationist, who among other children's books, edited in German, Robinson Crusoe, a work which no doubt had its early influence in bending the youthful twig. When Alexander was eight years old his father died. The mother and her two boys then lived at Tegel the old homestead a few miles from Berlin. Alexander's constitution was not very strong. In the year 1779 the widow was fortunate enough to secure for their companion and tutor, G. J. Christian Kunth, a youth of twenty, a rare and noble fellow, in whom the mother could and did place implicit confidence. He took charge of the brothers, and became their guide, companion and friend, at the same time being in constant consultation with their mother. How admirably he guided them in their boyish games and studies, and how faithfully he labored for and with them in after life, both the brothers bear ample testimony. The next year, when Alexander was eleven, he heard at Tegel private lectures on botany by Heim. Two years later Kunth used to take the boys to Berlin to pursue their studies with private masters. They studied together, but each was allowed to follow his own bent. Though Alexander differed widely from William in his inclination for some studies, yet both were alike ardent, and each sympathized with the taste of the other. Here Löffler taught them Greek, and young Willdenow, a rising botanist, with perhaps the best herbarium in Europe, instructed them in botany, while Dohn, Engel, Klein and other distinguished professors were engaged by their ever faithful tutor, to give them private lectures in law, politics, philosophy, mathematics, and the physical sciences. Kunth too heard all these lectures, and a little boy of nine, four years younger than Alexander, heard some of them, Leopold von Buch his name.

Thus they passed two years partly at the capitol and partly at Tegel, always under the watchful care of Kunth, and all three under the eye of the mother, with no temporal cares to retard their progress, and no family obscurity to embarrass their position. Two years later Kunth and his charge, late in 1785, took their first leave of the mother for a time, and went to join the University at Frankfort on the Oder, where they resided two years or more, in the family of their Greek professor Löffler, who had removed thither from Berlin. At Easter, 1788, having exhausted the resources of Frankfort and grounded for a higher course, Kunth accompanied the brothers to Göttingen, at that time the most celebrated University of Germany. Here Alexander at the age of eighteen found ample scope for all his aspirations in nature and natural science, and both brothers had more ample opportunities afforded them to follow out the diverse branches of research to which each felt a strong innate tendency.

The University of Göttingen was then at its zenith, with the best selected library of modern books in Europe. Here they met and cemented lasting friendships with those world-renowned Professors, Blumenbach, Eichhorn, Gmelin, Heeren, Heyne and George Forster, all of whom in after life the brothers were never tired of naming with love and gratitude. The wives of Heeren and Foster were the accomplished daughters of Heyne, he of *the* Homer, he of *the* Virgil, who was himself both Professor of languages and librarian of the University. Into this learned family circle the brothers were cordially admitted in the closest terms of intimacy. Alexander soon became the favorite pupil of the great naturalist Blumenbach, and was proud to call himself the scholar of Gmelin.

But it was to George Forster that Alexander never ceased to acknowledge his indebtedness. Forster was then only thirty-four. He had accompanied Capt. Cooke in his second voyage round the world in 1773–1775, with his father J. Reinhold Forster, and in the following year 1777 gave to the world his marvelous book, *Reise nach der Südsee*, which afterwards made a profound impression on the mind of young Humboldt, and was perhaps the first great incitement in him to the scientific study of nature. How rejoiced then must he have been to find at Göttingen, Forster whose voyages he knew by heart, and whom he delighted long after to call 'my celebrated teacher and friend.' 'If I might,' wrote he in *Cosmos* in the late evening of his life, 'be permitted to instance my own experience, and recal to mind the source from whence sprang my early and fixed desire to visit the land of the tropics, I should name George Forster's *Delineations of the South Sea Islands*,' etc.

The same year, 1788, and while enjoying the society of Forster, there appeared another little book which seems to have still further aroused his love of nature and strengthened his resolution, already formed, for great voyages. This was Bernardin de St. Pierre's master work, *Paul and Virginia*, a copy of which he says in *Cosmos* 'accompanied me to the climes whence it took its origin. For many years it was the constant companion of myself and my valued friend and fellow traveler Bonpland, and often in the calm brilliancy of a southern sky, or when in the rainy season the thunder re-echoed, and the lightning gleamed through the forests that skirt the shores of the Orinoco, we felt ourselves penetrated by the marvelous truth with which tropical nature is described, with all its peculiarity of character, in this little book.'

After a residence of nearly two years at Göttingen, studying languages, botany, zoology, geography, chemistry, mathematics, geology, mineralogy, etc., in the spring of 1790, Humboldt joined his friend Forster at Mayence, whither he had removed, and they two set out on a private scientific Exploring Expedition down the Rhine. At that time the great question that devided geologists, had reference to the Plutonian and Neptunian origin of rocks. The Basalt of that noble river was before him, and accoutred as he was he plunged into the controversy with mind impartial and fresh from the university. The result of his investigations appeared the same year in his first book, at the age of twenty, entitled, *Mineralogische Beobachtungen über einige Basalte am Rhein. Braunschweig*, 1790, 12°. It is a very neat little volume arrranged with taste and judgment, and in a scientific point of view is said to be creditable to a much older head. The book was published anonymously and is now but little known, being very scarce. The copy described in the Catalogue (No. 4563), and now belonging to General Frémont, possesses peculiar interest. It bears the author's autograph signature, at the end of the dedication, and was presented to Professor Gmelin in 1790, with an affectionate inscription 'by his scholar, A. von Humboldt.' More than sixty years after, on his eighty-fifth birth-day, this precious little volume was re-presented by Theodor Wagener to the great philosopher, who on the 14th of Sept., 1854, inscribed in it a graceful memento of his youth and of his old age.

From the Rhine the travelers passed through Holland and Belgium, and thence to England, where Forster introduced Humboldt to the President of the Royal Society, Sir Joseph Banks, his fellow voyager fifteen years before with Capt. Cooke round the world. He was warmly received by many of the scientific men of London. At the house of Warren Hastings he was shown some pictures by Hodge representing the shores of the Ganges, which made a lasting impression upon his youthful mind, and still further increased his longing for travel. On returning to Germany he published his book, and then the first great question of his life was asked by his mother. What next? There is some pretty strong evidence that she desired him to take to political life and become a diplomatist or statesman. At all events it was determined that he should enter upon some public employment, and the better to qualify him for this, he went to Hamburgh, and in the winter of 1790–1791 attended the celebrated Commercial Academy of Ebeling, studying business, accounts, trade, banking, and commerce, as much as his ardent love of the physical sciences would permit. But even here, of all places in the world, he was unexpectedly encouraged in his ruling passion. Ebeling became his friend and opened to him the treasures of his own private library, at that time one of the largest collections of voyages and travels with maps and geographical works relating to America, then in Europe. Ebeling was about to go to press with the first volume of his great work on America and hence his mind was full of the subject. That rich collection was afterward purchased by Mr. Thorndike of Boston, and presented to the library of Harvard College, of which to this day it forms a prominent feature.

After a struggle of a few months between business and science, Humboldt's inordinate love of the latter finally triumphed, and in 1791 he turned his back on Ebeling his friend, and commerce his foe, and soon after found himself in Werner's house in Freiberg, with that dear boy for his chum whom he had met at lectures eight years before in Berlin, Leopold von Buch, then a youth of seventeen. Werner, at that time the greatest geologist of the age, was Director of the School of Mines at Freiberg. From sight these two young men became friends for life. More than three score years later, wrote Humboldt, 'That I should be destined—I, an old man of eighty-three—to announce to you, dear Sir Roderick, the saddest news that I could have to convey:' . . . 'Leopold von Buch was taken from us this morning' . . . . 'without him I am desolate.' . . . 'His mind left a track of light wherever it passed.' . . . 'We were together in Italy, in Switzerland, in France,—four months in Saltzburg.' At Freiberg Humboldt devoted himself with humboldtian energy to the study of mining and metallurgy. His mind was ever open and ready for impressions, which it received as surely as wax, and as speedily as photography. No bee could exhaust the wild flowers of the woods quicker than he could extract from his masters all they had to impart. Scarcely a year then sufficed to accomplish his aim at Werner's, whom he left in March, 1792, and returned to his mother at Tegel.

Humboldt had now arrived at the end of his pupilage; and such a pupilage! unparalled in biography. Who before him was ever so favored by fortune, so mentally gifted, so lovingly *led*, and so intellectually prepared, for the brilliant career upon which he was about to enter? Yet he took no royal road to his acquisitions, but that hard paved one open to all, with work, self reliance, energy and love of nature for mile stones. At this early age we find him in scholarship ripe beyond his years, a linguist, an archaeologist, a botanist, a geographer, a geologist, a mineralogist, a metallurgist, a chemist, and an author. His travels for the period had been considerable, and few had so many learned friends. What a contrast to the youthful struggles of the immortal Franklin, and yet these two are perhaps the brightest examples for youth in record. Thus prepared Humboldt was launched into the wide world at the age of twenty-two, burning with an irresistible desire, as he repeatedly tells us in after life, to travel in distant lands unexplored by Europeans.

The next five years, from 1792 to 1797, the young aspirant is tracked with some difficulty through a combination of circumstances well calculated to elevate, strengthen and mature him for the execution of some grand project. Born and educated in central Germany, remote from salt water, with a love of nature ingrain and strengthening with his knowledge, he longed for the sea, as he tells us, and the tropics, and had already resolved, as soon as the opportunity presented itself, to go round the world, and gratify his enthusiasm for the savage beauties of tropical countries guarded by mountains and volcanoes, shaded by primeval forests and watered by vast unexplored rivers; and going or coming, explore that New World where man and his handiworks of ancient and modern civilization had not intervened to dwarf the stupendous display of gigantic nature. All his studies now tended to qualify him for a scientific traveler. As his journey was to be the circle of the globe, so his study was the circle of the sciences. He worked hard and observed closely; and, what in a young observer of nature is of the highest importance, he reduced to order his observations and wrote them out. During his short year with Werner, the parent of the Neptunian theory, he found time to collect and describe the cryptogamous plants he found growing far down in the mines of Freiberg. He made drawings of them, wrote out their natural history in good Latin, and sent his manuscript to Göttingen to his old friend and teacher, Blumenbach, who soon after returned it, edited with his own notes, and backed with the seal of his approbation. The work,* a handsome quarto volume, saw the light the next year, at Berlin, it being his *second* book, at the age of twenty-three. The second part of it, *Aphorisms on the chemical physiology of vegetables*, he found of great use to him in his observations in America.

This same year he accepted an official position under government in order that he might have influence and opportunities the better to pursue his investigations. He became the Superintendent of Mines in Franconia, and during the short two years he held that position, is said to have remodeled the systems of development and management. He inspected every department himself, near and remote, and became both an executive officer and a business man; while at the same time he was a student, an observer, an explorer and an author. His mental activity was perfectly marvelous, and his scientific and literary labors prodigious. He made experiments and contributed many articles both in his own and in the French language, to the chief scientific journals of Germany and France. Humboldt thus early achieved a reputation throughout Europe as a rising naturalist. In 1795 he resigned this official position, which, although favorable to the cultivation of his favorite pursuits, still did not fill his active mind, now more and more thirsting for explorations in the equatorial regions. This passion led him to devour and analyze the relations of voyages and travels to India, America, Africa and the Islands, but generally to regret the want of variety of knowledge in insulated branches of natural history. The great Expeditions of Fleurieu in 1768–69, of Bougainville in 1766–69, of Cooke 1768–1780 were familiar to him as household words, as were also afterward those of Vancouver, La Pérouse and d'Entrecasteaux; but all these, though they gave ample accounts of the oceans, their islands and their coasts, yet left him unsatisfied as to the vast interiors of countries and continents. They developed marine geography and nautical astronomy, but left comparatively untouched, physical geography, botany, zoology, the relations of the vegetable world, the migrations of the *social* plants, and the geological structure of mountains and volcanoes. All these he found set forth more to his liking in M. de Saussure's scientific explorations of the Alps and Vesuvius, which interested him profoundly and caused him to study carefully both the results and the use of the instruments by which they were attained.

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* FLORÆ FRIBERGENSIS Specimen Plantas cryptogamicas præsertim subterraneas exhibens. Accedunt Aphorismi ex Doctrina Physiologiæ chemicæ Plantarum. iv Plates, 4°, Berolini, 1793. The *Aphorisms* the next year, 1794, were translated into German by G. Fischer, with additions by J. Hedwig, and a Preface by G. F. Ludwig, and published at Liepzig in 8vo.

In this same year, 1795, freed from official care, Humboldt traveled much through Germany and visited Vienna, where he renewed his studies in botany and physical geography, studied and traveled with Freiesleben the celebrated geognost, and with von Haften visited northern Italy, but was for political reasons deterred from going to the volcanic regions of Naples and Sicily. At Vienna he became acquainted with the recent discoveries of Galvani which interested him deeply, and henceforth galvanism became one of his special studies, if indeed a mind of such general grasp can attend on specialties. Many months of most useful preparatory study he passed there examining the exotic plants, and enjoying the friendship, of M. de Jacquier and of M. Vander Schott. Already familiar with the experiments of Franklin and others in electricity, he began there his famous experiments in chemistry, galvanism, electricity and other matters pertaining to organic life, which in importance and originality rivalled the celebrated but subsequent investigations of Bichat.

About this time, at the instigation of Baron von Zach, he found time to acquire a practical knowledge of astronomy, surveying, geometry and mathematics, all so essential to travelers, and became familiar with the use of the various scientific instruments for ascertaining latitudes and longitudes, heights and distances, etc. Next, in the winter of 1796–97, we find him at Jena studying anatomy and physiology under Loder. Here he continued his investigations into animal life in connection with chemistry, galvanism and electricity, and especially experimented on the irritability of the muscular fibre, the vital force in animals and plants, and almost hoped that he had found the clue to the phenomena of life. In close connection with these studies he found it necessary to obtain a practical knowledge of zoology, ichthyology and ornithology. All these investigations and experiments, containing the germs of a new science, which, especially in America, is to-day exerting itself with vigor, were embodied in his *third* book* published in Posen in 1797, in two volumes in octavo.

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* Versuche über die gereizten Muskel- und Nervenfasern oder Galvanismus, nebst Vermuthungen über den chemischen Process des Lebens in der Thier- und Pflanzen-

Humboldt now began to think seriously of leaving Europe for a long journey, but regretted to do so without first having seen Vesuvius, Stromboli and Etna, to enable him by comparison to form a 'proper judgment of a great number of geological phenomena; especially of the nature of the rocks of trap formation, it became necessary to have examined strictly the phenomena offered by burning volcanoes.' At Jena he revived his friendship with Goethe and Schiller, the one twenty, the other ten, years his senior, both of whom he terrified by his tremendous energy, and inspired with his own love of nature. 'My natural historic studies have been roused from their winter sleep by his presence' wrote Goethe to Schiller; and wrote Schiller to Goethe 'Although the whole family of Humboldt lie ill of the ague they speak only of great journies.' He therefore determined to return to Italy and with his friend Leopold von Buch set out in November, 1797. They spent some time in Vienna, four months in the several cantons of Salzburg and Styria, pursuing to great advantage their geological investigations; but as they were about to pass the Tyrolian Alps the wars of Italy compelled them to turn back, and, to Humboldt's great regret, to abandon the volcanoes. They then proceeded through France home to Berlin.*

The time had now arrived for immediate preparation for his great voyage. But whither go? He was undecided, the impediments of wars and politics being so great it was impossible to determine. However, having buried his mother, and settled his worldly affairs for a long absence, he set out for Paris in 1798. His fame had already preceded him, and he soon made the acquaintance of many savans, and set about earnestly to collect the instruments and all things necessary for a long scientific exploring expedition. The very list of this apparatus filling pages in his *Personal Narrative* is enough to overpower the mind of an unscientific traveller. There were chronometers, telescopes, (achromatic and simple), lunettes, sextants, reflecting and repeating circles, theodolites, artificial horizons, quadrants, compasses, graphometers, dipping and other needles, magnetometers, pendulums, barometers, thermometers, hygrometers, electrometers, cyanometers, eudiometers, phosphoric eudiometers, boiling water meters, thermometrical leads, areometers, compound microscopes, meters, gauges, chains, tubes, vases, evaporators, Leyden vials, galvanic apparatus, etc., not one of each only, but

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welt mit Blumenbachs Anmerkungen 2 vols., viii Plates. 8° Posen, 1797–1799. This work was immediately translated into French with the following title: Expériences sur le Galvanisme et en général sur l'irritation des fibres musculaires et nerveuses: trad. de l'allem. [par Gravel] avec des additions par J. F. N. Jadelot, medicin. 8° Paris, 1799.

* About this time he must have prepared his *fourth* and *fifth* books, which were printed at Braunschweig in octavo in 1799, the one *über die chemische Zerlegung des Luftkreises und über einige andere Gegenstände der Naturlehre*, and the other *über die unterirdischen Gasarten und die Mittel ihren Nachtheil zu vermindern.*

in many instances duplicates and even triplicates. Most of the instruments he had already tested in kind in his various travels and explorations the past two years, and had therefore confidence in his own judgment in selecting them.

The first opportunity that presented itself he accepted, though not much to his taste. Lord Bristol asked him to accompany him to Upper Egypt on an archæological exploring expedition of eight months. He accepted this proposal and for some time directed his studies in conformity with this new project; and though it was abandoned in consequence of the temporary insecurity to travelers, he found that the archæological information then acquired proved in Mexico to be of no inconsiderable service to him. Meanwhile he had made the acquaintance in Paris of two young naturalists, Aimè Bonpland of La Rochelle, and Michaux of Versailles, who had been appointed to the proposed Exploring Expedition to be commanded by Captain Baudin, round the world by Cape Horn, skirting South America from the La Plata to Quito and Panama, and thence across the Pacific to New Holland, Van Diemen's Land, Madagascar (the scenes of his friends Paul and Virginia), and so home by the Cape of Good Hope. Though Humboldt had little confidence in Capt. Baudin, he obtained permission to embark with all his instruments, reserving to himself, however, the liberty to leave the expedition whenever he thought proper. For several months he worked with an eye single to this great enterprise, with his whole heart and soul in it, when, on a sudden, news came that war had broken out in Germany and Italy, and Napolean had determined to postpone the expedition indefinitely. The disappointment was cruel, but the knowledge he had gained was not dissipated. His determination now was to quit Europe at once, by engaging in any enterprise that might tend to console him.

He had made the acquaintance of a Swedish Consul, appointed by his government to carry presents to the Dey of Algiers, passing through Paris, who offered to give to Humboldt, if he would accompany him, the advantage of his long acquaintance in that part of Africa, to facilitate him in visiting the Atlas Mountains of Morocco. No mineralogist had yet examined this lofty chain of mountains which rose to the limit of perpetual snow. He jumped at this proposal, and his friend Bonpland jumped with him. The Swedish frigate was to reach Marseilles towards the end of October, 1798, and therefore all three hastened thither. Two long months they waited there, and no frigate came, but finally news reached them that she had met with accidents and could not be expected at Marseilles till spring. Disappointed again, almost disheartened, but not discouraged, they resolved to spend the winter in Spain, and in the spring if possible embark at Carthagena or Cadiz for the East. Their instruments they took with them, leaving only the duplicates at Marseilles to follow. Their object was still to work to the East, to India if possible. They crossed Catalonia, Valencia and Old Castile to Madrid, making on their way many astronomical and geographical observations, and ascertained the inclination of the needle and the intensity of the magnetic forces, the results of which were never published.

Immediately on their arrival in Madrid they had reason to rejoice at the wind that wafted them to Spain. Baron de Forell the Saxon minister, himself a mineralogist, at once interested himself in their behalf, thought they might obtain through the enlightened minister Urquijo, permission to visit the interior of Spanish America. The friends hesitated not a moment to adopt this suggestion. In March, 1799, Humboldt was presented at Court, and himself presented to the King a memoir on the motives which led him to undertake a voyage to the new continent and the Phillippine Islands. The result was without delay two passports for himself and Bonpland, one from the Secretary of State, and the other from the Council of the Indies. That he might not be obstructed by narrow-minded viceroys, priests or remote officials of church or state, he had it set down plainly in the bond that he was authorized to make free use of his instruments of all kinds, might make astronomical observations, measure heights and weigh mountains, examine the soil, explore rivers, inspect mines, and in short execute all operations deemed useful for the progress of the sciences, throughout the whole of the Spanish dominions. No passport from the Spanish government, before or probably since, was ever so liberal. Thus after many disappointments, and changes of plans, consequent upon the wars and the generally unsettled state of the political world, Humboldt had great reason to congratulate himself, as indeed the whole world with all the sciences at this day has to congratulate itself, that he at last drifted into Spain, all the tougher, the wiser and the better for his many disappointments. The travelers proceeded immediately to Coruña, secured a passage in the Sloop Pizarro, a companion of the monthly packet boat, and in June, 1799, embarked their instruments and impedimenta. But the port of Coruña was at that time blockaded by an English squadron. However, under the protection of a friendly storm which obliged the English to stand out to sea, and the cover of a dark night, the Pizarro ran the blockade, and on the sixteenth of July, landed at Cumana in Venezuela, within sight of that beautiful Paria which Columbus discovered in April, 1498, and believed to be Paradise, whence our first parents were expelled. Thus three hundred years after Columbus, an Italian, had sailed from a port in Old Spain to discover a new world, Humboldt, a German, and his friend Bonpland, a Frenchman, availed themselves of the same alien courtesy to go and discover what it contained.

[To be continued.]

ART. I*.—*Livingstone's African Explorations.** *Despatch from Dr. Livingstone to the Earl of Clarendon*, dated 'near Lake Bangweolo, South Central Africa, July 8th, 1868.'

My Lord—When I had the honor of writing to you in February, 1867, I had the impression that I was then on the watershed between the Zambesi and either the Congo or the Nile. More extended observation has since convinced me of the essential correctness of that impression; and from what I have seen, together with what I have learned from intelligent natives, I think that I may safely assert that the chief sources of the Nile, arise between 10° and 12° south latitude, or nearly in the position assigned to them by Ptolemy, whose River Rhaptus is probably the Rovuma. Aware that others have been mistaken, and laying no claim to infallibility, I do not yet speak very positively, particularly of the parts west and northwest of Tanganyika, because these have not yet come under my observation; but if your Lordship will read the following short sketch of my discoveries, you will perceive that the springs of the Nile have hitherto been searched for very much too far to the north. They rise some 400 miles south of the most southerly portion of the Victoria Nyanza, and, indeed, south of all the lakes except Bangweolo.

Leaving the valley of the Loangwa, which enters the Zambesi at Zumbo, we climbed up what seemed to be a great mountain mass, but it turned out to be only the southern edge of an elevated region, which is from 3000 to 6000 feet above the level of the sea. This upland may roughly be said to cover a space south of Lake Tanganyika, of some 350 miles square. It is generally covered with dense or open forest, has an undulating, sometimes hilly, surface; a rich soil; is well watered by numerous rivulets, and, for Africa, is cold. It slopes toward the north and west, but I have found no part of it under 3000 feet of altitude. The country of Usango, situated east of the space indicated, is also an upland, and affords pasturage to the immense herds of cattle of the Basango, a remarkably light colored race, very friendly to strangers. Usango forms the eastern side of a great but still elevated valley. The other or western side is formed by what are called the Kone Mountains, beyond the copper mines of Katanga. Still further west, and beyond the Kone range or plateau, our old acquaintance the Zambesi, under the name of Jambaji, is said to rise. The southern end of the great valley inclosed between Usango and the Kone range is between 11° and 12° south. It was rarely possible there to see a star, but accidentally awaking one morning between 2 and 3 o'clock, I found one which showed latitude 11° 56′ south, and we were then fairly on the upland. Next day we passed two rivulets running north. As we advanced, brooks, evidently perennial, became numerous. Some went eastward to fall into the Loangwa; others went northwest to join the River Chambeze. Misled by a map calling this river in an off-hand manner 'Zambezi, eastern branch,' I took it to be the southern river of that name; but the Chambeze, with all its branches, flows from the eastern side into the center of the great upland valley mentioned, which is probably the valley of the Nile. It is an interesting river, as helping to form three lakes, and changing its name three times in the 500 or 600 miles of its course. It was first crossed by the Portuguese, who always inquired for ivory and slaves, and heard of nothing else. A person who collected all, even the hearsay geography of the Portuguese, knew so little actually of the country that he put a large river here running 3000 feet up-hill, and called it New Zambesi.

I crossed the Chambeze in 10° 34′ south, and several of its confluents south and north, quite as large as the Isis at Oxford, but running faster, and having hippopotami in them. I mention these animals because in navigating the ambezi I could always steer the steamer boldly to where they lay, sure of finding not less than 8 feet of water. The Chambeze runs into Lake Bangweolo, and on coming out of it assumes the name Luapula. The Luapula flows down north past the town of Cazembe, and 12 miles below it enters Lake Moero. On leaving Moero at its northern end by a rent in the mountains of Rua, it takes the name Lualaba, and passing on N.N.W., forms Ulenge in the country west of Tanganyika. I have seen it only where it leaves Moero, and where it comes out of the crack in the mountains of Rua, but am quite satisfied that even before it receives the river Sofunso from Marungu, and the Soburi from the Baloba country, it is quite sufficient to form Ulenge, whether that is a lake with many islands, as some assert, or a sort of Punjaub —a division into several branches, as is maintained by others. These branches are all gathered up by the Lufira—a large river, which by many confluents drains the western side of the great valley. I have not seen the Lufira, but pointed out west of 11° south, it is there asserted always to require canoes. This is purely native information. Some intelligent men assert that when the Lufira takes up the water of Ulenge, it flows N.N.W., into Lake Chowambe, which I conjecture to be that discovered by Mr. Baker. Others think that it goes into Lake Tanganyika at Uvira, and still passes northward into Chowambe by a river named Loanda. These are the parts regarding which I suspend my judgment. If I am in error there and live through it, I shall correct myself. My opinion at present is if the large amount of water I have seen going north does not flow past Tanganyika on the west, it must have an exit from the Lake, and in all likelihood by the Loanda.

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* From the Minutes of the Royal Geographical Soc., for the Meeting of Nov. 8th

Looking back again to the upland, it is well divided into districts, Lobisa, Lobemba, Ubengu, Itawa, Lopere, Kabuire, Marungu, Lunda or Londa, and Rua; the people are known by the initial 'Ba' instead of the initial 'Lo' or 'U' for country. The Arabs soften 'Ba' into 'Wa, in accordance with their Suaheli dialect; the natives never do. On the northern slope of the upland, and on the 2d of April, 1867, I discovered Lake Liemba; it lies in a hollow, with precipitous sides 2000 feet down; it is extremely beautiful, sides, top, and bottom being covered with trees and other vegetation. Elephants, buffaloes, and antelopes feed on the steep slopes, while hippopotami, crocodiles, and fish swarm in the waters. Guns being unknown, the elephants, unless sometimes deceived into a pitfall, have it all their own way. It is as perfect a natural paradise as Xenophon could have desired. On two rocky islands men till the land, rear goats, and catch fish; the villages ashore are embowered in the palm-oil palms of the West Coast of Africa. Four considerable streams flow into Liemba, and a number of brooks (*Scotticè*, "trout *burns*"), from 12 to 15 feet broad, leap down the steep bright red clay schist rocks, and form splendid cascades, that made the dullest of my attendants pause and remark with wonder. I measured one of the streams, the Lofu, 50 miles from its confluence, and found it at a ford 294 feet, say a 100 yards broad, thigh and waist deep and flowing fast over hardened sandstone flag in September—the last rain had fallen on the 12th of May. Elsewhere the Lofu requires canoes. The Louzua drives a large body of smooth water into Liemba, bearing on its surface duckweed and grassy islands; this body of water was 10 fathoms deep. Another of the four streams is said to be larger than the Lofu, but an over-officious headman prevented my seeing more of it and another than their mouths. The lake is not large, from 18 to 20 miles broad, and from 35 to 40 long; it goes off N N.W. in a river-like prolongation two miles wide, it is said, to Tanganyika: I would have set it down as an arm of that lake, but that its surface is 2800 feet above the level of the sea, while Speke makes that 1844 feet only. I tried to follow the river-like portion, but was prevented by a war which had broken out between the Chief of Itawa and a party of ivory traders from Zanzibar. I then set off to go 150 miles south, then west, till past the disturbed district, and explore the west of Tanganyika; but on going 80 miles I found the Arab party, showed them a letter from the Sultan of Zanzibar, which I owe to the kind offices of his Excellency Sir Bartle Frere, Governor of Bombay and was at once supplied with provisions, cloth, and beads; they showed the greatest kindness and anxiety for my safety and success. The heads of the party readily perceived that a continuance of hostilities meant shutting up the ivory market, but the peace-making was a tedious process, requiring 3½ months. I was glad to see the mode of ivory and slave-trading of these men, it formed such a perfect contrast to that of the ruffians from Kilwa, and to the ways of the atrocious Portuguese from Tette, who were connived at in their murders by the Governor D'Almeida.

After peace was made I visited Msama, the chief of Itawa; and, having left the Arabs, went on to Lake Moero, which I reached on the 8th September, 1867. In the northern part Moero is from 20 to 33 miles broad. Further south it is at least 60 miles wide, and it is 50 miles long. Ranges of tree-covered mountains flank it on both sides, but at the broad part the western mountains dwindled out of sight. Passing up the eastern side of Moero we came to Cazembe, whose predecessors have been three times visited by Portuguese. His town stands on the northeast bank of the lakelet Mofwe; this is from two to three miles broad and nearly four long. It has several low, reedy islets, and yields plenty of fish—a species of perch. It is not connected with either the Luapula or Moero. I was forty days at Cazembe's, and might then have gone on to Bangweolo, which is larger than either of the other lakes; but the rains had set in, and this lake was reported to be very unhealthy. Not having a grain of any kind of medicine, and, as fever, without treatment, produced very disagreeable symptoms, I thought that it would be unwise to venture where swelled thyroid gland, known among us as Derbyshire-neck, and elephantiasis (scroti) prevail. I then went north for Ujiji, where I have goods, and, I hope, letters; for I have heard nothing from the world for more than two years: but when I got within 13 days of Tanganyika, I was brought to a stand-still by the superabundance of water in the country in front. A native party came through, and described the country as inundated so as often to be thigh and waist-deep, with dry sleeping places difficult to find. This flood lasts till May or June. At last I became so tired of inactivity that I doubled back on my course to Cazembe.

To give an idea of the inundation which, in a small way, enacts the part of the Nile lower down, I had to cross two rivulets which flow into the north end of Lake Moero; one was 30, the other 40 yards broad, crossed by bridges; one had a quarter, the other half a mile of flood on each side. Moreover, one, the Luao, had covered a plain abreast of Moero, so that the water on a great part reached from the knees to the upper part of the chest. The plain was of black mud, with grass higher than our heads. We had to follow the path which, in places, the feet of passengers had worn into deep ruts. Into these we every now and then plunged and fell, over the ancles in soft mud, while hundreds of bubbles rushed up, and, bursting, emitted a frightful odor. We had four hours of this wading and plunging—the last mile was the worst; and right glad we were to get out of it to the sandy beach of Moero and bathe in the clear tepid waters. In going up the bank of the lake we first of all forded four torrents, thigh deep; then a river 80 yards wide, with 300 yards of flood on its west bank, so deep we had to keep to the canoes till within 50 yards of the higher ground; then four brooks, from 5 to 15 yards broad. One of them, the Chungu, possesses a somewhat melancoholy interest, as that on which poor Dr. Lacerda died. He was the only Portuguese visitor who had any scientific education, and his latitude of Cazembe's town on the Chungu being 50 miles wrong, probably reveals that his mind was clouded with fever when he last observed, and any one who knows what that implies will look on his error with compassion. The Chungu went high on the chest, and one had to walk on tiptoe to avoid swimming. As I crossed all these brooks at both high and low water, I observed the difference to be from 15 to 18 inches, and from all the perennial streams the flood is a clear water. * * * * * * * * * *

ART. II.—*On the relation between the Intensity of Light produced from the combustion of Illuminating Gas and the volume of Gas consumed;* by B. SILLIMAN.*

(Read at the Salem meeting of the Am. Assoc. for Adv. of Science, Aug. 1869.)

IN photometric observations made to determine the illuminating power or intensity of street gas, it is the practice of observers to compute their observations upon the assumed standard of five cubic feet of gas, consumed for one hour, and in the constantly occurring case, of a variation from this standard, whether in the volume of the gas consumed or in the weight of spermaceti burned, the observed data are computed by the "rule of three," up or down, to the stated terms. The standard spermaceti candle is assumed to consume 120 grains of sperm in one hour, a rate which is rarely found exactly in actual experience.

For example, a given gas, too rich to burn in a standard argand burner at the rate of five cubic feet per hour without smoking, is consumed at the rate of 3½ cubic feet to the hour, with an observed effect of 20 candles power. This result, previously corrected by the same rule for the sperm consumed, is then brought to the standard of five cubic feet by the ratio $3{\cdot}5 : 20 = 5 : 28{\cdot}57$

The candle power of the gas is therefore stated as 28·57 candles, and this result has been universally accepted as a true expression of the intensity of the gas in question, or the relative value of the two consumptions.

In common with other observers, I have long suspected that this mode of computation was seriously in error, as an expression of the true intensity of illuminating flames, and that there were other conditions besides the volume of gas or weight of sperm consumed which must influence, and greatly modify the results. As most of these conditions are considered somewhat at length in a paper on "*Flame Temperatures,*" prepared chiefly from researches conducted by Prof. Wurtz and myself, and presented at the Salem Meeting of the Association, they need not be discussed in this connection.

The results of many trials, made with the purpose of determining the value of these photometric ratios, indicate clearly that the true ratio of increase in intensity in illuminating flames is, within certain limits, expressed by the following theorem, viz :—

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* The main points of this paper were made the subject of a verbal communication to the Conn. Academy of Arts and Sciences at their session, June 17th, 1869.

The intensity of gas flames, i. e., *illuminating power*, *increases* (within the ordinary limits of consumption) *as the square of the volume of the gas consumed.*

As the first experimental demonstration of this theorem was made by Mr. William Farmer, the photometric observer at the Manhattan Gas Co's. works in New York, I propose to speak of it as "Farmer's theorem." I am also indebted to Mr. Farmer and to Mr. Sabbaton, the well known and courteous Engineer of the Manhattan Gas Light Company, for the free use of their experimental data and the permission to employ them in illustration of Farmer's theorem.

The fundamental importance of this new mode of computation will at once appear, if, assuming it for the sake of illustration to be true, we apply it to the case already given above, which then becomes—

$$3{\cdot}5^2 : 20 = 5^2 : 40,$$

showing an increase of forty per cent over the old rule of correction. Let us see how far this theorem is sustained by the test of experiment.

Experiment 1*st.*—Two similar gas flames, one at each end of the photometer bar, were made to give exactly the same intensity of illumination. This was accomplished of course by placing the Bunsen disc midway between the two burners, and regulating the combustion until the disc was perfectly neutral ; the consumption being noted equal by two wet gas meters under the same pressure. The screen was then moved upon the bar to a point just four times as far from one flame as it was from the other, i. e., the bar being 100 inches, the screen stood at 80, i. e., as 1 : 4. The light from the distant burner was then increased until the disc again showed an equality of illumination. On reading the rate of the gas consumed by the two burners respectively, one gave 3·66 cubic feet and the other 7·32 cubic feet, or exactly double, or in other words, the lights were as the squares of the volumes of gas consumed, thus: $3{\cdot}66^2 : 7{\cdot}32^2 = 1 : 4.$

By the old rule the intensity would have been estimated directly as the volume of the gas consumed, thus :

$$3{\cdot}66 : 7{\cdot}32 = 1 : 2.$$

Experiment 2*d.*—The following results were obtained with the use of a standard argand burner. The readings of the index meter, the gas consumed in cubic feet, and the ratio of the lights produced, are given in three columns, viz :—

Index,	·0550	=	3·30 feet.	=	1	light.
"	·0725	=	4·35 "	=	2·1	"
"	·0856	=	5·13 "	=	3 2	"
"	·0926	=	5·55 "	=	4·	"

In this series the lights increase in considerably higher ratio than is required by Farmer's theorem, which demands 6·60 cubic feet, corresponding to a four-fold consumption, while the actual consumption was 1·05 cubic feet less than the quantity required by the theorem.

Experiment 3d.—The following series was obtained by another argand burner.

Index, ·062 = 3·72 cub. feet. = 1 light.
" ·0814 = 4·88 " = 2 "
" ·1000 = 6·00 " = 3 "
" .1203 = 7·219 " = 4 "

In this series the ratio is more nearly in accordance with the demands of the theorem, the intensity being still a little in excess of the squares of consumption (3·72 × 2 = 7·44 in place of 7·219).

The gas employed in these comparisons had a candle power of about 14 candles.

Experiment 4th.—Results obtained by a comparison of fish-tail burners, ratio as 4 and 9 feet respectively.

A. Index, ·0750 = 4·500 cub. feet. = 1 light.
B. " ·1586 = 9·519 " = 4 "

In this comparison the ratio falls but little short of the demands of the theorem.

Experiment 5th.—Comparison of fish-tail burners.

A. Index, ·086 = 5·16 feet. = 1·85
B. " ·1677 = 10·06 " = 4·

In this trial the departure from the requirements of the theorem is considerably greater than in any of the preceding experiments. But it appears that from some cause the ratio of the squares does not hold with gas of the power used in these trials (14 candles), where the consumption rises above 9 or falls much below 3 cubic feet. This is undoubtedly connected with the well recognized fact, that there is for each gas a kind of burner and a volume of gas better calculated than any other to develop its maximum intensity.

Experiment 6th.—This series was designed by Mr. Farmer to test by a direct comparison the value of the new as contrasted with the old method of correction. Both trials were made upon the same gas, the second observation following immediately after the first and with the same candle, and therefore should give about the same candle power.

1st Trial.—Consumption of sperm, 32·7 grains.
" of gas, 5·004 cubic feet.
Mean candle power (15 observations), 13·93 candles.

2d Trial.—Consumption of sperm,	32·2 grains.
" " gas,	4·58 cubic feet.
Mean candle power of 15 observations,	11·8 candles.

The above data calculated by Farmer's Theorem.

5·004	cubic feet, and	32·7	grains give	15·15	candles.
4·58	" "	32·2	" "	15·09	"
			Difference,	·06	"

Calculated by the old rule.

5·004	cubic feet and	32·7	grains give	15·16	candles
4·58	" "	32·2	" "	13·82	"
			Difference	1·34	"

It is obvious from the study of these results, that within the limits named the increase of intensity in gas flames, whether naked or argand, is at a ratio certainly as great as the squares of the volumes of gas consumed; and hence it follows that all the photometric determinations, which have been obtained by computation from volumes greater or less than the assumed standard of five cubic feet per hour, in the simple ratio of the volumes consumed must be considered as *absolutely worthless,* provided the theorem of Farmer here announced is confirmed.

It is evident also that this theorem applies with equal force to the weight of sperm consumed by the standard candle as to the volumes of the gas burned in equal times.

With a view to test the theorem of Farmer, I at once sought to apply it to the case of certain observations I had made upon very rich gas obtained from cannel and other rich coals. The photometric power of these gases had been measured in the usual way heretofore practiced by gas engineers, by burning a less quantity than five cubic feet in the standard argand and then computing up to a standard of five cubic feet by direct ratio. The results of this comparison appear to go far to confirm Farmer's theorem.

Peytona Gas.—This gas was made from a coal of West Virginia, known as Peytona Cannel Coal. It was much too rich to permit the flow of five cubic feet from the 15 hole argand burner, with a perfect combustion. The gas was therefore reduced by mixture with a measured volume of street gas of known value, and the illuminating power of the mixture having been carefully determined, the value of the Peytona gas alone was readily calculated and fixed at 42·79 candles. The following trials exhibit the result obtained by burning smaller volumes of Peytona gas, and the values obtained by the two methods of calculation.

No. 1	Argand	burner	consuming	5	cubic feet	per hour,	mixed gas	=42·79	candles
2	"		"	3.24	"	"	"	18·95	"
3	"		"	3·48	"	"	"	20·94	"

Here No. 1 represents very nearly the true illuminating power of the gas, and may be assumed as a fair criterion of the law under consideration.

By Farmer's Theorem.

No. 2 becomes 3.24^2 : 18·95 = 5^2 : 45·12 candles.
3 " 3·48^2 : 20·94 = 5^2 : 43·22 "

By direct ratio (old rule).

No. 2 becomes 3·24 : 18·95 = 5 : 29·24 candles.
3 " 3·48 : 20·94 = 5 : 30·09 "

By this it appears that by the old rule, assuming the true candle power of the gas to be 42·79 candles, the two observations Nos. 2 and 3 are in error by about 30 per cent, while by Farmer's theorem the error is reduced to 3 per cent, the former being too small and the latter too large.

Albert Gas.—The well known *Albertite* of New Brunswick furnishes a gas of remarkable richness. Its true candle power can be measured only by diluting largely with street gas of known value, and calculating it from the determined intensity of the mixture. In this way the gas from Albertite is shown to have an intensity equal to 70·38 candles. The following results were obtained by consuming different volumes in the burners named.

No. 1	argand	burner	consuming	5	cubic feet	= 70·38	candles.
2	"	"	"	2·25	"	= 16·39	"
3	'Scotch tip'	"	"	3·	"	= 25·25	"

By Farmer's Theorem.

No. 2 becomes 2·5^2 : 16·39 = 5^2 : 65·56 candles.
3 " 3^2 : 25·25 = 5^2 : 70·14 "

By simple ratio.

No. 2 becomes 2·5 : 16·39=5 : 32·78 candles.
" 3 " 3 : 25·25=5 : 42·08 "

The differences from the assumed standard of 70·38 candles are as follows:

By the old rule,	No. 2	falls short	37·6	candles	or 115	pr. ct.
" Farmer's theorem,	"	"	4·72	"	7·1	"
" the old rule,	No. 3	"	28·30	"	67·25	"
" Farmer's theorem,	"	"	0·24	"	0·34	"

It will be observed that No. 2 in this series represents a consumption considerably below the minimum which in most cases experiment has shown to be the limit of the proposed theorem, namely, 3 cubic feet, while No. 3, which represents exactly this limit, brings the result within the range of experimental error—it being impossible to make two series of 15 photometric observations which will accord more closely than these.

Wollongong Gas.—This gas was obtained from *wollongonite,* a new carbohydrogen described by me in a late number of this Journal (July, 1869), as coming from Australia. Its illuminating power was determined by mixing 10 per cent of the gas with 90 per cent of street gas. But this mixture was still too rich to burn 5 cubic feet in the Argand standard without smoking, and even when burned at this rate in a fish-tail burner the flame was somewhat smoky and inclined to "tail off." I have therefore little doubt that its true candle power is more nearly 142 candles than to 132 as stated in the article referred to. We quote, however, the observations made as follows:

1 fish-tail burner consuming 5 cubic feet gave 132·94 candle power.
2 " " 1·5 " " 12·89 "

Computing the second observation we have:

By Farmer's theorem for No. 2 143·22 candle power
" direct ratio " " 42·96 " "

This is an extreme case in which the volume of gas consumed in the second observation is far too low, but it is clear that by the old rule the result coming from the consumption of so small a volume of gas is perfectly worthless, while by Farmer's theorem the difference of 10·28 candles is within 7·7 per cent, while if the true intensity of this remarkable gas is placed, as there is good reason to believe it should be, at 142 candles, the agreement in the two observations is absolute.

Every photometric observer can confirm the results here given by reference to his own records of former observations, or by direct experiment designed to test the accuracy of the theorem here announced.

In Sugg's "Gas Manipulation" (London, 1867), page 64, is a tabular statement of the results of an experiment designed to illustrate the unfitness of the "Birmingham" burner, (a special form of argand) to develop the highest intensity of which a gas of 14 candle power is capable when it is used upon any other standard than that of a consumption of five cubic feet per hour. By this statement the burner in question produced from five cubic feet of gas exactly 15 candle power. But when reduced to 4·5 cubic feet consumption the candle power when "corrected to the standard quality of gas by proportion," was only 11·93 candles. The values of the 'correction' referred to can only be conjectured, but assuming that the observation made the uncorrected rendering 11·32 candles (a very probable quantity) we find that the law of the squares of consumption then makes the ratio as follows :—

$$4{\cdot}5^2 : 11{\cdot}32 = 5^2 : 14,$$

a result which in view of the facts before given cannot be regarded as accidental. The theorem applied to this case as it stands reported (including the correction) gives for the value of the fourth term of the ratio 14·7 candles.

I have endeavored to apply this theorem to some of the results recorded in the well known researches of Messrs. Audouin and Bérard, but I find these results stated in a manner which renders it difficult to fix clearly the terms of comparison. I venture, however, to append a few comparisons drawn from two of the tabular records of experiments with butterfly or bat's wing burners of the "fifth series" which so far as they go lend confirmation to the views here presented.

Burner of the fifth series—slit $\frac{1}{89}$ inch wide.

Consumption of the Burners under trial.	Consumption of the Bengel Argand standard burner without cone ; 8 in. chimney.	Comparative intensities. The Bengle burner—100.	Intensities by law of the squares of consumption.	Pressures.
Cubic feet.	Cubic feet.			
3·1079	3·6024	50	103	·23622
2·4015	3·5318	40	90·9	·19685
2·0131	3·6024	30	96·	·11811
Burners of same series—slit $\frac{1}{63}$ inch wide.				
3·9555	3·6730	80	92·6	·078474
3·1786	3·6730	60	80·7	·07480
2·6487	3·6730	50	96·7	·07480
2·3309	3·6730	40	97·5	·03937
1·5186	3·6730	20	115·6	·01968

The comparison of their results by this theorem, which gives reasonably exact results for consumptions which are not greater than that of the standard Bengle burner employed by them, fails when the consumptions become greater than that of the standard.

A comparison of the foregoing results will show that the coincidences with the requirements of the theorem of Farmer are, within the limits assigned, too numerous, and too closely accordant, to be considered as otherwise than pointing clearly to its general truth. A rigorous demonstration cannot be expected, as there are too many variable functions of unknown value involved in the best methods at present known for photometric measurements to permit more than an approximate proof of its general accuracy. Every photometric observer must recognize its importance and the necessity in his observations of bringing the consumptions of gas and sperm to the agreed standard.

To the consumer of gas the evident inference from the data here presented is that, where it is important to obtain a maximum of economical effect from the consumption of a given volume of illuminating gas, this result is best obtained by the use of burners of ample flow.

Where a moderate light of equal diffusion is required over a large space, as in public rooms, it may be expedient to use numerous small jets; but when the maximum intensity obtainable from a given volume of illuminating gas is desired, intensity burners of large consumption are plainly indicated.

In the discussion following this paper, Mr. F. Stimpson, State Inspector of gas for Massachusetts, brought forward some results of observations he had made upon Farmer's theorem (having been in correspondence with Prof. Silliman on the subject), and considered them in comparison with those herewith given. His conclusion was that while in many cases the theorem was closely applicable, in others it was not so. Mr. Stimpson's discussion of the matter will very likely appear in an early number of this Journal.

ART. III.—*Principles of Molecular and Cosmical Physics*; by Prof. W. A. NORTON.

IN a memoir on Molecular Physics, published in 1864–66, in this Journal, and republished in the London Philosophical Magazine, I showed that, by the introduction of a new hypothesis not in itself improbable, the principle of gravitation might be explained. I now find that no additional hypothesis is necessary but that this principle is essentially involved in one of the fundamental principles of the general theory; viz., that of the *interception of force by matter*. This fundamental principle was brought out in treating of the subject of the electric condition of molecules, and formed an essential element in the special theory of electricity developed in the memoir. This interception was regarded as no arbitrary assumption, but as a necessary consequence of the fact that a certain portion of the propagated force is instantaneously expended in imparting motion to the molecule, or atom, which it encounters, and is therefore abstracted from this force. In every instance of a propagated force this conclusion is inevitable. Now I find that if we admit that the primary forces of nature are propagated forces, that is, do not act instantaneously at all distances, the principle of universal gravitation, as well as the doctrine of the molecular forces and agencies set forth in my paper, may be directly deduced from *a single force of repulsion exerted by every primary atom upon every other atom.*

Admitted Principles.—It is now universally conceded: (1st,) that matter exists in at least two different fundamental forms, or conditions, viz: that of universal or luminiferous ether which pervades all space, and that of ordinary matter directly recognizable by our senses.

(2d.) That all masses of matter of sensible extent are made up of distinct atoms.

(3d.) That every atom is essentially inert or incapable of itself of altering its own state, whether of rest or motion; and that in every act of motion, or change of motion of an atom, an amount of force is expended proportionate to the mass of the atom and the velocity, or change of velocity, produced in the direction in which the force acts.

It is also the general conception with physicists, that every atom has a definite form, and a definite size dependent upon the quantity of matter which it contains, and it will serve to fix our ideas to adopt this conception; at the same time it should be understood that in order to arrive at our conclusions the only essential supposition to be made, with regard to the state of an atom, is that it occupies a certain space, proportionate to its quantity of matter, in such a manner as to receive and intercept a certain portion of the force propagated along every line, or a number of lines proportional to its mass, traversing this space.

Cosmical Force of Repulsion.—The fundamental notion of the propagation of force involves with it the conception that the force acts, or is transmitted in a series of recurring impulses. We may also assume that, like all known propagated actions, the force varies according to the law of inverse squares. In fact, if the impulses are transmitted along definite lines, and the atom occupies, as a cause of interception, a definite space, it is obvious that this law must of necessity hold good; or, if they are propagated by the intervention of wave pulses in a more subtile ether, whether the atom be regarded as a mere point or of definite size, so be that it has a definite degree of inertia, the same law should obtain. Now, let us leave out of view, for the present, all the bodies of ordinary matter in existence, and confine our attention to the luminiferous ether uniformly disseminated through space. Every atom of this ether exercises a repulsive action upon every other atom of the same at all distances; and this action consists in a series of impulses perpetually renewed, at an immensely more rapid rate, we must suppose, than those of light or radiant heat. Each effective impulse is but an excessively minute fraction of each individual impulse propagated in the force; and this is expended in giving motion, or virtual motion to the atom. That is, as in all known instances of propagated action, each impulse produces its effect, in the shape of velocity instantaneously imparted, and is expended in so doing, just as if no other impulse or series of impulses were in operation. Since the ether is uniformly disseminated, each atom must be subjected, in an appreciable interval of time, to the same amount of force transmitted from every direction, and therefore cannot experience any sensible displacement. But, although no sensible progressive motion of the ethereal atoms can result, under the circumstances supposed, still it is to be observed that the united effective impulses that come into operation upon the atom in any minute interval of time, *from any one direction*, must be enormously great in comparison with the force of repulsion subsisting between two contiguous atoms of the ether: and this latter force must be vastly greater than the elastic force of the ether called into play in the propagation of a wave of light or heat; since this elastic force results from a slight inequality in the repulsive actions of the contiguous atoms on different sides, attendant upon a slight relative displacement of the atoms. It is, as I conceive, by the coming into operation under certain circumstances, of a portion of this vast cosmical, ethereal force, received from definite directions, that the known effective forces of nature are brought into play.

Immediate consequences of the Interception of the Cosmical Force.—Universal Gravitation.—Let us next conceive that a single atom of ordinary matter is posited at a certain point in the universal sea of ether, and consider what forces would be brought into operation by its interceptive action. In the first place it is obvious that the entire force transmitted from all the ethereal atoms lying on any line intersecting the atom supposed, will be partially intercepted in passing the atom, and that the residual force propagated on will be of less intensity than the force coming toward it in a directly opposite direction. If then any point in space be taken, at any distance from this atom, and right lines be conceived passing through this point and the various points of the atom, and extending indefinitely in both directions, they will form a cone circumscribing the atom and having the point supposed for a vertex; and the force transmitted to the point along every line within the surface of this cone that encounters the supposed atom of ordinary matter, will be less than that received in the directly opposite direction. Accordingly there will be a greater amount of repulsive force transmitted through the vertex of the cone, from the ethereal atoms lying within the outer nappe or indefinite portion of the cone, than from those lying within the other nappe which circumscribes the atom of ordinary matter. The excess will operate as an effective force to urge any atom that may be posited at the vertex toward the ordinary atom. It is obvious that except at minute distances, this effective force would vary inversely as the square of the distance.

All this being understood, let us consider what will be the result with regard to the ether in the immediate vicinity of the supposed atom of ordinary matter. It is plain that each ethereal atom so situated will be urged toward the atom considered, until the effective force directed inward is neutralized by the outward repulsion of the ordinary atom and the increased repulsion of the ethereal atoms lying nearer to this. The ultimate result then would be the condensation of an ethereal atmosphere around the atom of ordinary matter, just as if this exerted a direct attractive action upon the adjacent ether. Now what will be the effect of the supposed atom (*a*), and its condensed atmosphere of ether, upon any other atom (*b*)? In the first place the entire interception of the cosmical repulsion produced by *a* and its atmosphere would give rise to a gravitating tendency of *b* toward *a*. But *a* and its atmosphere exert a direct repulsive action upon *b*. This direct force of repulsion is the sum of all the repulsive impulses exerted by *a* and the atoms of its atmosphere. But for the interception of individual pulses, produced by *a* and the atmospheric atoms encountered on lines of propagation, it would be the same as if all the repellant matter considered were concentrated at the center of *a*. As a matter of fact, in consequence of this interception, the center of repulsion will be displaced toward the exterior atom *b* acted on. Beyond a certain minute distance the direct repulsion thus exerted upon *b*, will vary inversely as the square of the distance from *a*; and it will at the same time be less, at all distances, than the gravitating tendency originating in the manner above explained.

The excess of this tendency toward the atom (*a*) *of ordinary matter above the repulsion exerted by the atom and its atmosphere, constitutes the effective force of gravitation due to the atom.* It will vary, at all measurable distances, according to the law of inverse squares.

Before inquiring into the dependence of this force upon the quantities of matter in the active atom and in that acted upon, we will remark that nothing precludes us from supposing that *the atom of ordinary matter, so called, is actually a mass of condensed ether.* It will readily be seen that if a certain portion of the equally diffused ether lying around any point were to be condensed upon this point, the increased interception of the cosmical force within the space that it would occupy, would develop, as above explained, an inward acting force that might be in equilibrio with the augmented mutual repulsion of the atoms of the condensed ether. If this conception be adopted, the interception of the cosmical force, effected by an atom of ordinary matter, will take place at each of the ethereal atoms of which it is composed, and the entire effect will be proportional to the number of such atoms, or the entire mass of the compound atom. If then a second such atom of ordinary matter (B) be considered, at a distance from the first (A), the effective gravitating tendency of each of its constituent ethereal atoms toward A will be proportional to the mass of A. The entire gravitating tendency of B toward A, will then be proportional to the mass of A multiplied into the mass of B. Also, since the tendency of each ethereal atom of B toward any ethereal atom of A is inversely proportional to the square of the distance, the same law will hold for the entire gravitating tendency of B toward A. It is to be borne in mind that the effective force of gravitation here considered, is the excess of the gravitating tendency due to the partial interception of the general cosmical force by the atom A and its ethereo-electric atmosphere, over the repulsion directly exerted by the same.

The Newtonian principle of gravitation being thus made out for individual atoms of ordinary matter, it is also made out for cosmical masses. The Newtonian laws of the mutual gravitation of masses, in all their precision, are inevitable consequences of the principle of gravitation as now deduced from the one cosmical force of repulsion; if we admit that the portion of every impulse of this force that is intercepted by each atom is an excessively minute fraction of the whole intensity of the propagated impulse.

Molecular Forces.—The Molecular Forces, so called, consist of a force called the attraction of cohesion, and one or more forces of mutual repulsion. The force of heat is recognized as one force of repulsion, and has generally been regarded as the only repulsion in operation. But this notion must certainly be discarded by all physicists who maintain that the heat-repulsion is due to vibrations of the atoms of bodies; since such vibrations can only be maintained by the operation of existing antagonistic forces of attraction (or virtual attraction) and repulsion. And even if we admit, as I cannot do, the mechanical possibility of molecular motions of revolution, or rotation, so constituted as to originate in some way a force of heat-repulsion exerted in all directions outward from each atom, and to be always augmented in rapidity by waves of radiant heat from whatever direction received, it is wholly inconceivable that in the collision of bodies such motions of revolution or rotation of the impinging atoms, or molecules, should give rise to a mutual repulsion, whatever might be the relative direction of the motions. In fact, in whatever mode of molecular motion heat may be supposed to consist, if heat be the only force of repulsion, a certain amount of the living force of heat belonging to each of two impinging bodies would be expended in diminishing the velocity of the one body and augmenting that of the other, and it would be impossible that they should become heated by the impact. It is here assumed that the impact of two bodies must develop a force of mutual repulsion between the impinging molecules, which determines the equality of the action and reaction. This obvious fact seems now-a-days to be in a good degree ignored, and the exchange of momenta to be supposed to be brought about by some unimaginable process, in which the idea of force is wholly lost sight of.

We must then conclude that there is a primary force of molecular repulsion, in addition to that of heat. We might perhaps ascribe this force to the repulsion of the ethereal molecular atmospheres when brought into contact; but to what can we ascribe the heat-repulsion? It has come to be generally believed that it must consist in some mode of motion of the atoms, or molecules of bodies, as a whole; either of vibration, revolution, or rotation. But it might be almost demonstrated, did space permit, that this cannot be the true nature or origin of heat. I will only allude here to one or two arguments in support of this statement, which may be briefly given. It is well known that whenever any body is by collision with another body, or in any other way, permanently compressed, heat is given out. Now the fact that a force of pressure, or percussion, produces a permanent compression, increasing with its intensity, leads to the almost inevitable inference that in the act of compression the atoms, or molecules, experience some change of physical condition, by reason of which the molecular attraction is augmented, and the condensation maintained, and that this change must be proportionate to the degree of condensation, and so to the amount of heat evolved. Can such a change proportionate to the heat evolved, be connected by any admissible supposition with the augmented velocities of vibration, revolution, or rotation of ordinary atoms, in which the increase of heat is conceived to consist? In fact the entire phenomena of inelastic, as well of elastic reaction, point to the conclusion that they are attended with changes in the physical condition of the atoms, ordinarily so called, and that the evolution or absorption of heat that takes place is the direct result of these changes.

Again it is not conceivable that the phenomena of electricity can be ascribed to any imagined motion of ordinary atoms, or molecules, whether these are surrounded with ethereal atmospheres or not. We are accordingly constrained to look in some other direction for the origin of the molecular forces and agencies; and it appears to me that it is to be found in the general conception of an electric ether less subtile than the luminiferous, and gravitating like this toward the atoms of ordinary matter, so as to form envelopes to these atoms, permeated and underlaid by the condensed luminiferous ether. Each atom would thus be surrounded by an ethereo-electric atmosphere. The atoms of electric ether may be conceived to be masses of condensed luminiferous ether, but far more minute than those of ordinary matter. Adopting this conception of the existence of an electric ether, we are led, by legitimate inferences from the fundamental principles I have laid down in this paper, not only to the physical constitution of a primitive molecule, but also to the theory of molecular forces set forth in my memoir on Molecular Physics. The force of molecular attraction was conceived to consist in a contractile action exerted by the central atom upon its electric envelope, and originating waves propagated outward through the electric ether to contiguous atoms. This contractile action is now seen to consist in the gravitating tendency of the electric envelope toward the central atom; resulting from the partial interception of the cosmical force by this atom. This action also originates waves in the luminifereous ether posited between the central atom and its electric envelope, that are propagated outward by this ether, and constitute the primary force of heat-repulsion. All the diverse methods of developing heat are but different methods of compressing or forcing inward the atomic or molecular envelopes, and so originating ethereal heat-waves. These waves at the outset pulsate in the line of propagation; but, by passing around the ethereal atmospheres of molecules, become converted into waves attended with transverse vibrations. The other force of molecular repulsion originates in the direct repulsion subsisting between the diverse atoms of the electric envelope. This force originates waves that proceed outward through the electric ether from different depths in the envelope. These waves must increase in their intensity at the outset from the lowest depth upward, to a certain height in the envelope, by reason of the increase in the quantity of ether that is effectively repellant. On the other hand the attractive waves, that issue from various points of the envelope, must decrease outward in their intensity at their origin. It thus happens that the resultant waves, which may be taken to represent the entire actions of these two systems of waves, must be conceived to proceed from different depths; which depths may therefore be taken as the upper and lower limits of the effective envelope. This is equivalent to one of the principles adopted in the mathematical theory of the molecular forces developed in my former memoir; viz. that the attractive waves proceed from the lower limit of the electric atmosphere, or envelope, and the waves of electric repulsion from the upper limit. In the memoir referred to it was shown that the entire action of one atom, or rather primitive molecule, on another, amounts to a repulsion at the more minute distances, and an attraction at greater distances up to a certain limit, beyond which it becomes a repulsion to an indefinitely great distance. The first mentioned, or inner repulsion, and the attraction, are the forces which determine and maintain the solid and liquid condition of matter, and the outer repulsion constitutes the expansive force of vapors and gases. The extraneous heat that a body may receive is a distinct force of repulsion, modifying the natural curve of molecular action.

Origin of Chemical Attraction, and of Electric and Magnetic Forces.—The electric envelopes of atoms, besides being the source of the molecular forces, including the primary heat-repulsion, invests the molecules of each substance with the property of chemical attraction for the molecules of other substances to which they bear a certain physical relation. This consists in the disturbance of the equal distribution of the ether in the envelopes of two contiguous heterogeneous molecules; under their natural molecular action, by reason of which the molecules become oppositely polarized on their adjacent surfaces—the one having an excess of the electric fluid and the other a deficiency. The attraction thus originating may prevail over the natural molecular repulsion that comes into play when the two liquids come into contact, and bring the molecules within the range of their natural attraction.

The state of molecular electric polarization, superinduced under certain varied circumstances, originates the galvanic current, and determines the phenomena of electro-static, electro-dynamic, and magneto-electric induction. The attraction and repulsion of electric, and of magnetic currents, may be ascribed to the origination and maintenance of currents in the adjacent body of luminiferous ether, running in the same direction as the electric or magnetic currents. These currents determine inequalities of elastic force in this moving mass of ether, from which result *transverse currents*. These transverse currents take effect upon the line of the electric or magnetic current because they are partially intercepted by it. The induction of one electric or magnetic current by another, is effected by the intervention of currents directly induced in the adjacent body of luminiferous ether.

The key to the explanation of the excitation of electricity by friction lies in the fact made out in my former paper, that the electric ether is condensed between the component molecules of each compound molecule of a substance; and that every expansion of the compound molecule sets free a certain portion of this condensed ether, and every condensation withdraws a certain portion from adjacent molecules that are undisturbed. If two dissimilar surfaces are rubbed, the one over the other, the consequent disturbance of the compound molecules of the two surfaces, will be either unlike or unequal in amount; and a certain quantity of electricity will in consequence pass from the one surface to the other. As soon as the friction ceases the disturbed molecules will recover their original form and size, and the positive state of the one surface and the negative state of the other will manifest themselves. For example, if the molecules of the one surface are compressed by the rubbing, and those of the other expanded, electricity will flow from the latter to the former while the condensation and expansion are going on, but as soon as two rubbing molecules are freed from each other's influence they recover their former dimensions, and the excess of the electric fluid in one of the molecules and deficiency in the other become positive and negative electric states. Non-conduction consists in a comparative deficiency of electric ether in the interval between contiguous compound molecules, by reason of which a transmission of electric movement does not readily occur from one to the other. I have shown in my paper on Molecular Physics that in such cases an electric polarization of the molecules is induced, which develops a resistance to the electro-motive force.

The excitation of electricity by heat is conceived to be principally due to the expansive action of heat on the electric envelopes of primitive molecules, and on the compound molecules, which either sets free a certain portion of electric ether, or establishes a chain of electro-polarized molecules.*

General Considerations.—The Theory of Cosmical and Molecular Physics, of which I have now given a brief outline, rests essentially upon the following principles.

(1.) *The doctrine of inertia* applicable to all matter.

(2.) *The existence of a single primary force of repulsion* exerted by every atom upon every other atom. This force is universally admitted to be in operation between the atoms of the luminiferous ether, and between the atoms of ordinary matter and this ether at the most minute distances.

(3.) *The existence of but one primary form of elementary matter*, viz: the universal or luminiferous ether;—the atoms, so called, of ordinary matter and of the electric ether being but different masses of condensed luminiferous ether. The theory of the origin of universal gravitation that has been propounded might be reconciled with the ordinary notion of matter, viz: that its atoms are essentially different from those of the ether, if we could admit that the resistance of an ordinary atom to a force giving it motion was proportional to its surface instead of its mass.

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* For the more complete exposition of my theoretical views I must refer the reader to the memoir on Molecular Physics.

(4). *The doctrine of the interception of force*, as already set forth.

(5). *The primary force of repulsion is made up of impulses recurring with an immeasurable rapidity.* This is no new hypothesis. In all treatises on Mechanics, gravity, and all incessant forces, are conceived to consist of an indefinitely great number of impulses taking effect in a finite interval of time.

If we conceive the propagation of the primary force of repulsion to be by the intervention of a medium, this medium must be an ether more subtile, and endued with a more intense elastic force than the luminiferous. This elastic force must consist in a mutual repulsion between the atoms. Thus, upon the idea of a material propagation of force, we must ultimately rest upon the conception of a force exerted between two atoms separated by a finite though excessively minute interval of space. There is no tenable position between this and that of a plenum.

Let us here devoutly acknowledge that in thus following the chain of cause and effect into the precincts of that most deeply hidden of all mysteries, the origin of force, we have come into the presence of the Infinite Spirit who puts forth unceasingly, from every point in the realms of space, His creative and sustaining power upon the subtile matter that fills all space, and is the essential substance of all worlds.

In addition to the principles just stated, we recognize the existence of matter in the three states of *the luminiferous ether*, *the electric ether*, and *ordinary matter.*

It hardly need be stated that among the consequences of these fundamental principles is included the doctrine of the *Conservation of Energy*, actual and potential. It is obvious that at any point in the boundless sea of ether the same amount of cosmical force would be received from every direction, but for the existence of the innumerable worlds dispersed through it. It is also true that in the mutual gravitation of each pair of cosmical bodies, resulting from the interception of the cosmical force, the amount of this force expended in giving motion to the one body will be expended in precisely opposite directions in giving motion to the other body; so that on the whole the cosmical force in operation in any one direction is equal in amount to that in operation in the opposite direction. Also the amount of energy taken up in the half revolution during which the one revolving body is approaching the other, is given out during the remaining half revolution in which it is receding and returning to the original position.*

The kindred principle of the mutual convertibility, or "Correlation of Physical Forces," including among these the living force of moving bodies and of the atoms of all bodies, is obviously an essential feature of our general theory. For heat is regarded as being in its origin a wave of translation in the luminiferous ether, and an electric current is a similar wave in the electric ether, and the conversion of one of these into the other, or into the motions of ordinary matter, is merely the transfer of the living force of moving matter from one of the states or forms of matter to another. The heat developed in chemical attraction, is the amount of living force in the waves of luminiferous ether originated by the compression of the electric envelopes of the uniting molecules; and the force of attraction is expended in the act of originating these waves, and must be equal to the living force propagated in them. In the chemical union of two atoms, or primitive molecules, the "clash of atoms" that takes place cannot be succeeded by a perfectly elastic mutual repulsion, since such a repulsion would completely separate them, and restore them to their condition before the attraction came into operation. Now the loss of elasticity, which renders the union possible, consists in the outward movements imparted to a certain portion of the luminiferous ether lying between the uniting atoms and their electric envelopes, by the forcible compression of the envelopes, resulting from the clash of the atoms; and these movements constitute also the living force of heat developed.

The heat produced by extraneous pressure, or by impact, is the work done by the force of pressure in compressing the molecular envelopes, in opposition to the resistance of the luminiferous ether posited below them, transformed into the accumulated work of the ethereal waves set in motion by this compression. The deficiency of elasticity, essential to the development of the heat, consists in this escape of the ether in waves of translation, and the attendant loss of living force; and the permanent compression of the impinging bodies results from the change in the molecular forces, consequent upon the permanent compression, or forcing inward of the molecular envelopes.

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* The ever-recurring pulses of the primary cosmical force, emanating from all the atoms of the one, primary matter, are directly consumed in communicating opposite movements, or virtual movements, to every atom in the universe. It is, as I conceive, because in the existing condition of things the distribution of matter is unequal in different directions around a point, and therefore the partial interception of the impulses of the cosmical force along the different lines of direction unequal, that an effective gravitating force exists. The entire amount of the cosmical force consumed in any interval of time is the amount intercepted by all the atoms of matter, and is independent of the motions that result from the inequalities just noticed. Gravitation, and molecular and chemical attractions which originate in the gravitation of electric ether toward atoms of ordinary matter, are then derivative forces, incidental to the direct actions exerted by the cosmical force upon the atoms. The heat which escapes from the sun, or any cosmical body, into space, represents only a certain amount of the general gravitating force previously expended in imparting movements, in waves of translation, to the luminiferous ether. The electric current, wherever it exists, represents a certain amount of the same force, expended in imparting similar movements to a certain quantity of electric ether at the source of the current. In other words, the escaping heat and the electric current each represents a certain amount of work done, either by the gravitating tendency of electric ether toward ordinary atoms, or by that of ordinary atoms toward each other.

The principle of the "Correlation of Physical Forces," involves (as above implied) with it that of the "Conservation of Force," maintained by modern physicists, viz: that all transformations of one physical force into another take place without any loss, and therefore the store of living force in existence in nature is invariable.

The idea seems now to be commonly entertained that the entire force in operation in the universe is the result of certain motions imparted to all matter at the creation, and so being ever equivalent to the living force embodied in these primordial motions, must be invariable. But a gravitating force is always operating to deflect each revolving body from the tangent to its orbit, and a certain amount is continually being expended in this act, so that if the force of gravitation were due to preëstablished motions, it would be continually wasting away, and the entire amount of force in the universe would be perpetually diminishing. The sum total of the individual forces taking effect throughout all nature may indeed be constant, but if so it is because the flux of the primary force of repulsion is uniform, and is consumed primarily by interception from all atoms, incidentally in maintaining the motions of revolution of all the cosmical bodies (in the universe), and all the molecular forces perpetually neutralizing each other within these bodies.

From our present point of view we may also discern that the physical forces, ordinarily so called, have an entirely different origin from that above mentioned. We may perceive that they are all either the direct or indirect result of the operation of the general force of gravitation, which is itself a consequence of the operation of the cosmical force of repulsion. Gravitation is the direct agent in two general classes of phenomena, viz: the revolution of one cosmical body around another, and the act of condensation of every such body upon its center of gravity. In the former the motion is curvilinear, and all the living force imparted by gravity to either body, while the two are approaching each other, is taken out, by the operation of the same force in opposite directions, while they are receding from each other; and this alternation has continued from the first without loss of living force, from one revolution to another. But, in the act of formation of the existing cosmical masses by condensation, the motions of their elementary parts have been directed toward the center of gravity of the mass, and the mutual destruction of such opposing motions has developed an equivalent amount of living force in the form of heat, that is in the form originally of waves of translation proceeding from the ethereal atmospheres of the condensed molecules. By the continued operation of the gravitating force, in the sun and earth, and probably in innumerable other worlds, the transformation of the work of condensation effected by this force into an equivalent store of heat-work is constantly going on. The heat of chemical combination, as well as that of liquefaction and solidification, are attributable to the gravitation of the electric ether toward the atoms of ordinary matter. All the heat and attendant light evolved since the creation form a store of accumulated work equivalent to the work done by the force of gravitation in effecting all the condensations that have hitherto taken place. This heat and light force has been in its natural operations opposed to the force of gravitation, and so essentially a separating and decomposing force. It has been the great physical agent in all the processes of vegetable and animal life. It has ever been passing through cycles of transformation into other physical forces, but in all its transformations the entire amount of energy, actual and potential, has remained invariably the same. Chemical combination, the electric current, liquefaction, and solidification, are instances of motion directly due to the general gravitating force operating on the electric ether. All motions of translation or rotation of bodies at the earth's surface are traceable directly to the same force, or to that of heat repulsion and therefore indirectly to that of gravitation. We may accordingly lay down the postulate, that the entire circle of what are termed physical forces have originated in the living force of the motions of condensation that have resulted from the natural operation of the general gravitating force, initiated at the dawn of creation and continued through all time. The very process of formation, by gradual condensation, of the worlds that people immensity, has developed the natural forces which have presided and continue to preside over all the processes of change that diversify and beautify their surfaces, and fit them to be the abodes of living beings. These diverse natural agents are but the offspring of the one overshadowing force, which, in the progress of countless ages has formed all worlds out of the primordial material fashioned by the Hand of the Creator. We may indeed, as we have seen, rise to a still greater height of conception, and refer this universal force to a primal force of cosmical repulsion, associated by the Infinite Spirit, the Source of all Power, with every atom of the one primeval matter that fills immensity, and is the elementary substance of all worlds.

ART. IV.—*The Photo-mapper;* by HENRY M. PARKHURST.

IT is a remarkable fact that while vision is by far the most accurate of our senses, aiding us almost exclusively in obtaining accurate measures, we have as yet discovered no method of measuring the intensity of light. The method of measuring the brightness of a star by extinction, for example, is in fact merely a method of assisting the eye in estimation. If we were to measure the diameter of a sphere by removing it to such a distance that it should cease to be visible and multiplying that distance by a certain constant; or by subdividing it until we could divide it no longer, or until the atoms should have no appreciable weight, and multiplying by a constant; it would be analogous to determining the brightness of a star by ascertaining what proportion of its light is too small to affect the retina. The true mode of measurement is by substraction of certain known quantities. To illustrate the advantage of this, were the difference of brightness of Arcturus and Capella but .01^m, that difference would be visible to the naked eye; and were it but .0001^m, it would be easily visible with telescopic aid. Could we construct a glass which would transmit, not a certain proportion of the whole light of a star, but all its light exceeding a certain absolute quantity, it would afford us a perfect photometer. Or could we construct a glass which should transmit polarized light, but which would not transmit common light, that would accomplish the same result. But in the mean time we may be allowed to designate estimations assisted by mechanical means as measures of the magnitude of a star.

My first experiments in determining by mechanical means the magnitude of stars observed in zones, and recording that magnitude at the time of observation on star-maps taken by the instrument described in the Journal for September, were made in 1865. I then constructed a hexagonal diaphragm so arranged that a series of six plates over the object glass, moved simultaneously by a lever extending the length of the telescope, gradually diminished the aperture until the star ceased to be visible. I connected this lever with the system of levers operated by the star-key, and recorded upon the map, in the same right-ascension with each star, a point whose position indicated the aperture at the moment of extinction, and therefore gave by a prepared scale the magnitude of the star.

My next step was to avoid the long and cumbrous lever by adopting the principle of the apparatus invented by me in 1860, and which I term a Bar Photometer, reducing the cone of light from a star at a point intermediate between the object-glass and its focus, instead of directly reducing the aperture at the object-glass.

At a convenient point, say one-eighth of the distance from the focus to the object-glass, I place in the meridian a straight bar one-eighth of the diameter of the object-glass in width. Clamping the telescope a little to the west of a star, the cone of light is gradually intercepted by the bar, and gradually re-appears. The time during which the star is extinguished, or during which it remains of less brightness than a standard artificial star in the field, indicates the brightness of the star by the proportion of the light cut off by the bar at the moments of occultation and reappearance.

A modification of this plan, is to make the bar wider, so that it shall become a diaphragm, with a central aperture of the width of the cone of rays, observing the duration of apparition instead of the duration of occultation of the star.

By either of these methods the scale may be made one of equal parts by a device similar to that which will presently be explained. With or without such a device, this method is well adapted for observations of small variable stars, requiring no apparatus but the bar, whose distance from the focus shall be so determined that photometric accuracy shall not be sacrificed, while there shall be no unnecessary consumption of time. Indeed a plain Bar Photometer can at any time be extemporized in a few minutes. Its adjustment in width or distance is not essential, because its position and width can be determined and the proper connections made without difficulty after the observations.

A further modification of this plan, is to revolve the diaphragm 90°, and make it movable in declination, occultating the star on both sides of the center of the field, the magnitude being indicated by the extent of the motion, which may be conveniently and accurately measured, with a dim light, by a pair of dividers.

By a still further modification, the motion of the diaphragm may be produced, by means of intermediate levers, by the star-key of my star-mapper; and this constitutes the Photo-mapper, which I will now more particularly describe.

The diaphragm is moved parallel to itself in an arc corresponding to that described by the star-point and star-key, being supported and guided by two parallel bars whose bearings are screwed to the east side of the telescope tube. The diaphragm being seven-eighths of the distance from the object-glass to its focus, the length of the supporting bars is seven-eighths of the radius of the star-point. As I use a camera-prism, which slightly shortens the focus, I have, in my instrument, made the proper corrections, but need not here refer to details of that description.

On the same center with the star-point, and star-point-bar, shown in fig. 1 of my article on the Star-mapper, is another bar placed parallel to the axis of the telescope, pointing toward the object-glass. The further end of this magnitude-bar is connected at right angles by a connecting-rod with universal joints, with the upright arm of a lever, the lower arm of which is the lower supporting-bar. The length of this upright arm and of the magnitude-bar must be equal. In photo-mapping I place the prism always in the meridian, to avoid the complicated adjustments which would be necessary if its position were to be varied.

As the diaphragm will move seven-eighths as fast as the star-point, it is evident that if it is so adjusted that it will not intercept any of the light of a star in the center of the field when the star-point is brought to the declination of that star, then in mapping a northern or southern star in the field its light will be unobstructed. But moving the star-key to the north or south of a star will intercept part of its rays. The light of the star being thus equalized with that of a standard artificial star, or extinguished, as the case may be, a magnitude mark is impressed on the paper. The star is then mapped in its proper position, and the distance upon the map, of the magnitude mark from the corresponding star, will be the measure of the magnitude of that star.

A perpendicular plate, with a circular hole through which the connecting-rod passes, furnishes a convenient point from which to measure with dividers when the instrument is used without the mapper.

Thus far I have spoken of the "cone" of rays, as if the aperture were circular. If it were so, the scale of magnitudes would not be one of equal parts. It may be made one of equal parts by placing over the object-glass an outer diaphragm with an aperture of suitable form, and making the aperture of the inner diaphragm of corresponding form.

Let $x=2{\cdot}5[y]$ be the equation of a logarithmic curve. Then, the area between any two ordinates of that curve will be $2{\cdot}5\mathrm{M}\triangle y$; M being Modulus. Constructing for the object-glass of 6 inches aperture a diaphragm bounded by four such curves, with values of x ranging from $-1{\cdot}5$ inches to $+1{\cdot}5$ inches, laying off x vertically, above and below the center, and y to the right and left, and a similar inner diaphragm of one-eighth the lineal dimensions, neglecting for the present the portion of the curve which will pass outside of the circle, it will be seen that the motion of each one-fourth of an inch of the inner diaphragm will leave an area corresponding to stars exactly one magnitude smaller. The motion of the star-point will be one-seventh greater, and of the star-key still greater, according to the scale of the map.

But while the object-glass is limited, the inner aperture may extend beyond the corresponding circle of one-eighth the diameter. If therefore an area equal to twice the omitted portion of the curves above and below is added to the inner diaphragm, it will be almost immediately available, and the omission from the outer aperture will cause an error of less than ·1^{m}, and that only at the commencement of the scale. The proper form of this added portion is the inversion of the last 1$^{\cdot m}$2 of the curve within the circle, the final value of y being exactly three times its value at the margin of the disk.

With the diaphragms thus constructed, the scale runs through 3$^{\cdot m}$0 as a scale of equal parts. Beyond that point the star may still be extinguished, if not too near the center of the field; but the scale will be condensed.

The stars occurring in any zone may be divided into three classes: those above the 6$^{\cdot m}$5 being approximately measured by the condensed scale, and being so few in number that they may be conveniently, as well as more accurately, measured by a different method, to be explained below; those between the 6$^{\cdot m}$5 and the 9$^{\cdot m}$5, which may be equalized in brightness with an artificial star in the field previously brought to an equality with a 9$^{\cdot m}$5 star; and those fainter than the 9$^{\cdot m}$5, which may be extinguished. The outer diaphragm will so diminish the aperture that 12$^{\cdot m}$5 stars will be the smallest which can be seen.

The Disk Photometer.—For the brighter stars I employ an entirely different apparatus, based upon a more accurate method. I expand the stars into disks by drawing out the eye-piece beyond the focus, until a portion of the disk shining through an aperture in the field of view shall either be exactly equal in brightness with an adjacent luminous disk, or be imperceptible. The extinction of a disk by expansion is only practicable with a small aperture or with telescopic stars, and is not much, if any, more accurate than extinction by reducing the aperture. But comparison of disks, especially for the stars visible to the naked eye, is the most accurate means of measurement known to me, the error of the comparison being in my experience less than the inequalities of the artificial disk, and the latter much less than those of the sky.

The star and the disk can be compared, if desired, by the aid of polarization, by the use of a double-refracting prism to combine the two disks into one, and a selenite plate and another double-refracting prism to form new colored disks; but instead of producing the final adjustment by further polarization, the bundle of plates may be dispensed with, and the disks rendered colorless by the motion of the eye-piece.

For convenience I have made a slide with six equal openings, to shade the disk with neutral shades, and a slide to shade the star. I ascertained that one shade was equivalent to 1$^{\cdot m}$03, and that, although I admit the rule would not be accurate with colored shades, five neutral shades were equivalent to $5^{\cdot m}15$. It is therefore convenient to compare directly stars differing as much as α and λ Ursæ Minoris. In order to neutralize the yellow color of the artificial disk, and to make it appear indistinguishable from the visible portion of the disk of a star, I have found it necessary to use a blue shade. I have extended the telescope by various tubes, sometimes nearly two feet; but with the use of the slides I find that a tube five inches long to hold the eye-piece, usually gives sufficient extension to the telescope and sufficient range of brightness. The amount of the extension I measure with dividers, using a prepared scale which gives directly, in magnitudes, the difference in brightness between the observed star and a standard depending on the brightness of the disk.

The disk photometer is especially useful for observing the brighter variable stars. It is inapplicable for determining the brightness of stars so close together that the space between them is not sufficient for a disk; but in this case it may be used to determine the aggregate brightness of the two stars. It may be used for measuring the brightness of planets, small nebulæ and comets, probably of the moon, and possibly of the sun.

The same apparatus may be used to determine the comparative brightness of different portions of the sky, but requires a different process.

The error of the determination of the magnitude of a star by the method of equalizing disks, may be divided into four parts:

I. The error of the assumed magnitude of comparison stars. In my series of observations, instituted to determine the relative amount of the several errors, this error hardly exceeds $^{m}\cdot01$.

II. The error of observation, or inaccuracy of judgment as to the exact point where the disks are equalized. Not only according to the statements of Arago, Silliman, and Crookes, reduced by myself, but according to my own results, the error of observation—I refer to the *mean* error—does not exceed $^{m}\cdot02$, in a series of stellar observations averaging forty to the hour. For fainter stars than I observed in my series, the error of observation is greater. There are three limitations:

1. One disk cannot be distinguished in brightness from another, unless one-sixtieth brighter or fainter, according to Arago.

2. One disk cannot be distinguished as brighter than another, if the subtraction of the light of the fainter disk from the other would leave a disk too faint to be visible by itself. The same principle is applicable to points of light. Unless, therefore, the disk is at least sixty times as bright as is necessary for visibility, the error of observation will be greater.

3. If the disk is small, the accuracy of comparison is impaired. Hence points of light cannot be as accurately compared as disks. I have used a disk of an apparent magnitude of at least 1°, and have not investigated the amount of error with a smaller disk.

III. The error from the variation of the artificial disk during observation. I have made many and careful experiments to render this error as small as possible, and to avoid ascribing to the irregularity of the sky discrepancies which might be owing to variations of the artificial disk. The brightness of the artificial disk may vary from four causes:

1. The illuminating quality of the gas may vary. Hence observations on different evenings cannot be compared directly. But the change of the gas will be so slow, excepting probably from the heating of the apparatus when it is first lighted, as not to appreciably affect the results.

2. From variation of the pressure the flame may be made larger and brighter. I first partially corrected this by forming upon the screen an image of a circle of the flame ·05 in. in diameter. I afterwards made a gas regulator, admitting the gas into an inverted receiver suspended at one end of a lever, the other end of which gradually shut off the gas as the receiver rose; so that whatever the pressure of gas in the mains there should be no variation in the flame.

3. The flame varies in different parts. Hence, if on turning the telescope upon a new star the lantern is not accurately adjusted to a horizontal position, a brighter or fainter portion of the flame will come opposite the aperture. The flame as a whole having been made uniform by the regulator, I have introduced a smoothly ground glass at the aperture nearest the flame, and reduced the flame and moved it further back, so as to use the whole flame.

4. The light from the star is a sharp cone, wholly entering the eye even if withdrawn several inches from the eye-piece. The light from the illuminated screen is a hemisphere. Unless therefore the eye-piece renders the rays parallel, which it will not if the observer is either near-sighted or far-sighted, a difference in the distance of the eye will greatly affect the results. I have therefore turned the plane glass in the eye-piece so as to throw the light directly into the eye, reducing the aperture of the lenses so that the cone of rays from the artificial disk nearly corresponds with that from the star. It is necessary that the eye should be held so that both cones shall completely enter it at once, which can be easily accomplished but requires care.

The error from the variation of the artificial disc, without the gas regulator, I ascertained not to exceed ^{m}·04. How much this has been reduced by introducing the regulator and ground glass screen, I have no means of estimating.

IV. After allowing for the previous errors, amounting in the aggregate to less than ^{m}·05, the remaining discrepancies of observation of invariable stars must arise from variations of the apparent brightness of the stars from atmospheric obscuration. I divide this into two parts:

1. The general and permanent obscuration depending on the altitude of the star. I have ascertained that this follows the law of refraction; but the coefficient varies in amount on different evenings, sometimes equalling ^{m}·50 for an altitude of 20°, and at other times not exceeding one-third that amount. I apply the correction for obscuration to each star, in reducing the observations, so as to eliminate this; and I therefore regard it as no part of the error of observation. The correction may be easily applied without directly determining the altitudes, by a prepared scale.

2. The temporary and local obscuration, from various atmospheric causes. By more than 400 observations of 18 stars of the 2nd to the 4th magnitude, α Persei and γ Arietis being the extremes, I found that the average mean error of an observation, from all causes, amounted to ^{m}·11; so that the mean error from the temporary and local obscuration alone is on the average ^{m}·10. There is a great difference between different days; the average mean error of all the stars being sometimes as low as ^{m}·07 for a whole evening, and at other times for an evening apparently equally clear, as high as ^{m}·20. The difference between the stars is no less conspicuous. The stars ε and γ Persei are near each other, and nearly of the same brightness. Yet the mean error of an observation of γ Persei, assuming it to be an invariable star, is nearly three times as great as of ε Persei.

It is evident therefore that if the results I have reached are even approximately correct, the errors of observation and of the instrument, if ordinary care is used, are of no material consequence; and accurate results can only be obtained by multiplying observations on different evenings, so as to eliminate as far as possible the errors arising from the variations of the sky.

ART. V.—*On the Crater of Haleakala, Island of Maui, Hawaiian Group;* by Prof. W. D. ALEXANDER. (From a letter to one of the Editors.)

I HAVE just been spending a summer vacation on Maui, and in the course of it made a careful survey of the great crater of Haleakala. During the vacation I went three times to the summit. The first time I rode up from Makawao before sunrise, and spent about seven hours in collecting mineral specimens and plants, and forming a plan for the survey of the crater. The flora of this region has been described before. I will only say that the principal plants that survive on the bleak summit are the *Argyroxiphium*, *Raillardia montana*, *Vittadinia*, and some stunted ferns, viz: *Pteris aquilina* and *Trichomanes.* In the belt between one and four miles from the summit, in addition to the above, are found the beautiful *Geranium cuneatum*, *Sophora chrysophylla*, Sandalwood, *Coprosma*, and a few ohelos (*Vaccinium reticulatum*). Water boiled at 193° Fahrenheit when the atmosphere was at 46° Fahrenheit. At sunrise we enjoyed the grand sight of the vast triangular shadow of the mountain projected on the clouds in the western sky.

On the morning of August 4th, I ascended the mountain again from Makawao, with five natives, and furnished with a superior theodolite, a dozen large bamboos for signal poles, a good tent, and provisions for a week. We spent seven days on the mountain, and enjoyed almost uninterrupted fine weather.

We commenced operations by setting signals on the prominent points along the western side of the crater. The western cliffs are very steep, though it is possible to descend in many places, and are from 2,000 to 2,500 feet high. We pitched our tent on the lee side of a hill near the southwest corner, called by the natives "Pakaoao" or the "fortress of Kaoao." This hill is composed of a light gray solid clinkstone, which splits into laminæ like slate.

It has been much shattered, probably by the terrible convulsions that attended the opening of the Koolau gap, and for a quarter of a mile toward the northwest the ground is strewed with fragments of rock that have been hurled in that direction. These rocks form a striking contrast with the darker and more basaltic rock to the northward. The same formation crops out on the east side of the Koolau gap, at the southern foot of Hanakauhi in the oasis surrounded by recent lava, at which place it projects from the hill side in the form of huge perpendicular slabs or lamellar masses, not more than ten feet thick, and from thirty to fifty feet high, standing out of the grassy slope like immense grave stones. The patches of gravel on the summit contain numerous crystals of augite. The hill called the fortress of "Kaoao" is partially terraced on the lee side, and is covered with hundreds of little inclosures built of stone, three or four feet high, and paved with thin flat pieces of clinkstone. I noticed that a few had been covered over with a kind of slate roof. Here, according to tradition, encamped the army of Kaao, a chieftain who had been driven out of Kaupo by his rival, some time in the early part of the last century. We found a couple of ancient sling stones in the camp. Many silver-swords (*Argyroxiphium*) and other plants have grown up, flourished and died in the *pas* where his army once encamped. I was told that they drew their supplies from *Kula*, the district on the west side. Other natives relate that the sandalwood cutters used to encamp there forty or fifty years ago, when collecting sandal wood for their chiefs. The chief, Kaao, was finally defeated and killed in a battle fought on the west side of the mountain.

Our supply of water was brought from a water-hole two or three miles west of *Kalahaku*, which is only known by a few of the natives. We spent three nights on the spot.

About a quarter of a mile farther south is a red scoria hill, which is the highest point of the mountain, 2,750 feet above the bottom of the crater. It is the first of a long line of scoria hills which extends to the sea on the southwest, passing through Capt. Makee's plantation, and is called *Kolekole*.

The true meridian was obtained by careful observations on the pole star when on the meridian taken at this point and also at the east end of the base line in the crater. The fact had been noticed before that there is a considerable local attraction in the walls of this crater. I found that certain rocks of a compact bluish lava on the highest point were decidedly magnetic. Pieces of this rock have distinct polarity. There was evidently a large daily variation of the needle; between sunrise and noon the needle moved at least half a degree westward, and returned to its former position at night. On the morning of the 5th there was a heavy frost around our camp, the thermometer at sunrise standing at 38° Fahr. The average temperature at noon was 72° in the shade; the temperature in the crater averaged about 4° higher. After selecting stations and setting up signal flags along the western and southern sides of the crater, we descended into it at the southwest corner, along an immense sloping bank of reddish sand and scoria. We then marked out a base line more than a mile and a half long over a level plain at the foot of the southern wall of the crater.

This plain was sprinkled with myriads of silver-swords (*Argyroxiphium*), many of which were in full bloom, presenting a magnificent sight. These plants are most abundant over the volcanic sand in the central part of the crater. Both in descending into the crater and on our return, we followed a beaten path, which leads through the southeast gap to Kaupo. Our party were somewhat affected by the rarity of the air; we all experienced shortness of breadth, and some complained of headache, but after spending a week in this region, we became accustomed to the air, and our "wind" improved wonderfully.

The natives have many local names for different parts of the crater. The term *Haleakala* properly belongs to the southeastern part of the mountain, extending two miles west from the

Kaupo gap to the low place in the wall of the crater, called the "Puali" or pass of "*Koanui o Kane.*"

On the eastern side is a famous rock called *Pohaku Palaha*, which is the "hub" of East Maui, from which all the boundaries between lands are believed to radiate.

The next day, the 6th, we moved our tent and baggage four or five miles into the crater, and encamped near a cave called "*Ke ana ma ka uahi*," i, e, the smoky cave, half a mile from a trickling stream on the southern wall called "the water of Palaoa." This place we made our head-quarters for three days, during which we enjoyed almost cloudless weather, and our survey made rapid progress.

There is much more vegetation in the eastern end of the crater than in the western. On the sloping walls of this part of the crater, and on the three easternmost cones we noticed a good many mountain plants, such as *Dodonæa*, *Sophora*, *Raillardia* and *Geranium*, and on the summit of the southeastern walls of the crater, we observed clumps of large trees. This vegetation gave this region a very different aspect from the stern desolation of the western end. Near the commencement of the Kaupo gap, close to the road, there is a famous water-hole called "*Ka wai pani.*" Here, as I am told, there is a subterranean stream of pure cold water, two or three feet below the surface, which is reached through a small hole in the rock, which was formerly kept closed by a large stone, whence its name, "the closed water."

The scenery around us was entirely unique, and as grand in its way as that of the Yosemite valley. The vast plain surrounded by black precipices nearly twenty miles in circumference, and from 1,000 to 2,500 feet in height, with the two wide breaks through which we had glimpses of the distant ocean, the sixteen crater cones, of different colors, gray, red and black, the profound solitude and silence broken only by the flocks of wild goats, formed a scene to which it would not be easy to find a parallel elsewhere in the world. I ascended several of these cones in the course of my triangulation, and found the highest to be over 750 feet in height above its base. Another measured 390 feet, and several are over 200 feet in height. They are composed of light scoria and cinders, generally of a deep reddish color, but two are of a peculiar gray tint, and others nearly black. I found no traces of sulphur and very little pumice. The cones all have their ancient names, which are known only by the mountaineers and old natives. For instance, the highest cone is called "*Ka lua o ka Oo*," the crater of the Oo, and was the former residence of Kamohoalii, Pele's younger brother, "the king of vapor," of whom many legends are told. Another was called "*Ka iwi o Pele*," Pele's bone, and close by it on the north side is the *Pa puaa o Pele*, or "Pele's pig-pen." This is a singular natural formation of lava, about two rods square and ten feet high, with an opening on the northwest side. Some of my men believed that if any one should *kaha*, i. e., scratch the sand in this place or otherwise desecrate the sacred precinct, the evil spirits of the mountain would bring on fog and rain. A similar superstition attaches to the *Argyroxiphium*, or silver-sword plant. The natives also report a bottomless hole on the northwest part of the crater, which I did not visit, but which has been seen by my father and others.

After returning to our first camp on the summit, we spent another day in visiting the stations around the south and west sides, and measuring the remaining angles, and returned to Makawao late in the evening of the 11th. After having worked up my notes and drawn a map of the crater, I visited the summit once more on the 25th, to compare my map with the original on the spot, and to make the details more exact.

According to the results of my survey, the crater is of an oval shape, its longest diameter being nearly east and west, unlike most of the Hawaiian volcanoes, the major axes of which generally range north and south. The greatest length of the crater was found to be about seven and a half miles from east to west, the width being from two and a quarter to three miles. The area is about sixteen square miles.

The highest point was determined by the United States Exploring Expedition to be 10,217 feet above the level of the sea, and 2,783 feet above the bottom of the crater. I made its height by triangulation 2,750 feet above the east end of my base line in the crater. The boiling water experiment gave an altitude of 10,165 by Regnault's rule, which is certainly a close approximation.

The northern or Koolau break is about three miles wide, and over 2,000 feet deep, where it first leaves the crater. The western brink of it bears about N. 64° E. The bottom of it is floored with streams of lava of different ages, and is extremely rugged.

The southeast or Kaupo break is about a mile and a quarter wide, and its direction for the first three or four miles is about S. 58° E. The lava flow then turns to the southward, and continues in the direction S. 34° E. to the sea, spreading out in the form of a delta, and filling up the lower part of several large ravines.

The appearance of the lava fields and cones in the eastern end, and the vegetation on them seem to indicate that they are far more ancient than those in the western end. The latest action in the crater seems to have been the formation of the cinder cones in the southwestern part.

To conclude, the survey has impressed me with the conviction that this is a real terminal crater, and not merely "a deep gorge open at the north and east," or a *caldera.* I have indeed heard the theory proposed that the mountain is but the wreck of a complete dome with a small terminal crater, the whole top of which has fallen in and been carried away, as is supposed to have been the case with some of the volcanoes of Java, and the Caldera of Palma. Such questions I leave for geologists to settle, and if I can furnish them any new data on the subject, I shall be quite content.

Oahu College, Oct. 12, 1869.

ART. VI.—*On a new method of separating Tin from Arsenic, Antimony, and Molybdenum;* by FRANK WIGGLESWORTH CLARKE, S.B.

SOME time since, happening to notice that the remarkable highly crystalline precipitate formed by oxalic acid in a solution of stannous chlorid was not blackened or otherwise affected by sulphuretted hydrogen, I was led to a series of experiments concerning the action of the above named acid upon certain metallic sulphids, and obtained the following results.

Both sulphids of tin, if moist, and freshly precipitated, are readily decomposed by moderately long boiling with an excess of oxalic acid, H_2S being given off. The monosulphid is converted into the insoluble, crystalline stannous oxalate, while the yellow disulphid is completely dissolved. The commercial "mosaic gold," however, seems to be unacted upon by the reagent. In presence of an excess of oxalic acid, tin cannot be precipitated by H_2S.

The sulphids of arsenic, even upon very long boiling with the acid, are almost unattacked. Very minute traces of the metal sometimes go into solution, but may be reprecipitated by a bubble or two of H_2S. Accordingly, the presence even of an enormous excess of oxalic acid does not hinder the precipitation of arsenic as sulphid.

The sulphid of antimony behaves in a somewhat different manner. Although upon long boiling with oxalic acid considable quantities of the metal are taken into solution, yet every trace of it may be reprecipitated by H_2S.

Molybdic trisulphid appears to be wholly unattacked by oxalic acid, even upon very long boiling.

With the sulphids of tungsten I have obtained discordant results. Under certain circumstances they seem to be wholly insoluble in the acid, while at other times they are decomposed completely, and partly taken into solution.

By availing myself of the solubility of the sulphids of tin in oxalic acid, I have been enabled to separate tin perfectly from arsenic and molydenum, and almost perfectly from antimony. When only arsenic and antimony are to be separated from tin, I find it best to proceed as follows: to the solution containing the three metals, this solution being prepared in the usual manner for the precipitation of the sulphids, I add oxalic acid in the proportion of about twenty grams of the reagent for every gram of tin, taking care to have the whole so concentrated that the acid will crystallize out in the cold. I then heat to boiling, and pass in sulphuretted hydrogen for about twenty minutes. No precipitate appears at first, but as soon as the liquid is saturated with the gas, the sulphids of arsenic and antimony begin to fall, and in a very few moments are completely thrown down. Then, as usual, the whole should be allowed to stand about half an hour in a warm place before filtering. Every trace of arsenic and antimony is precipitated, so that in the filtrate from the sulphids neither of these metals can be discovered by Marsh's test, nor can any antimony stain be produced with zinc upon platinum. I have carefully experimented to learn whether oxalic acid could interfere with either of these tests, and find that it has not the slightest influence upon them. The sulphid of arsenic is absolutely free from tin, but the antimony always carries down a minute trace of that metal with it. This trace, however, if the operation has been carefully performed, can scarcely be detected, and generally may be ignored with safety. If, however, the greatest accuracy is desired, it may be well to redissolve the sulphid of antimony in an alkaline sulphid, decompose the solution with an excess of oxalic acid, boil with a little strong sulphydric acid water, filter, and add the filtrate to the tin solution previously obtained.

To separate tin from molybdenum, owing to the difficulty of precipitating the latter metal with H_2S, I have been obliged to slightly vary my process. I find that by adding an alkaline sulphid in excess to a solution containing a molybdate, then decomposing the sulphur salt formed with a considerable quantity of dilute chlorhydric acid, and allowing the whole to stand over night in a warm place, every trace of molybdenum is precipitated. The sulphid thus obtained can be easily washed with a mixture of dilute chlorhydric acid and ammonic chlorid. If now, by this process we throw down tin and molybdenum together, every trace of the former metal may be dissolved out by boiling the mixed sulphids for about three fourths of an hour with oxalic acid in the proportions which I have already given.

It is best to have present in the solution, while boiling, a little dilute chlorhydric acid.

If antimony also is contained in the mixture, it is necessary, just before ceasing to boil, to add to the solution an equal volume of strong sulphydric acid water, to reprecipitate any of that metal which may have gone into solution. Upon filtering no molybdenum can be detected in the filtrate by any ordinary tests, and the molybdic sulphid is absolutely free from tin. In all these cases it is assumed that the tin is in the form of a stannic compound. It must be borne in mind that the lower sulphid of this metal is converted by the acid into an oxalate insoluble in water. But as the latter dissolves to an almost unlimited extent in dilute HCl, its formation need not interfere with an analysis.

Since the presence of oxalic acid interferes somewhat with the complete precipitation of tin by ordinary methods, I was subjected to some trouble in finding a process for determining that metal after the separation. At last I found it could be thrown down as follows; the solution, after being rendered slightly alkaline with ammonia, is mixed with enough ammonic sulphid to redissolve the precipitate at first formed, an excess of *acetic* acid is added, and the whole allowed to rest several hours in a warm place. *Acetic* acid must be used, for stronger acids would be liable to set free some of the oxalic to redissolve the tin. The precipitate which at first varies from white to pale yellow, rapidly darkens in color, and seemingly consists of a mixture of oxyd and sulphid of tin. It should be washed with a solution of ammonic nitrate, and, after ignition, is weighed as SnO_2. In two successive experiments, in which I mixed a weighed quantity of tin with unknown proportions of arsenic and antimony, I received of the tin, after making my separation, respectively 99·93 and 99·57 per cents. The loss in the second case was due to my not having allowed the tin precipitate to settle sufficiently long before filtering, in other words, to incomplete precipitation.

The arsenic and antimony, being in the form of sulphids, may be estimated by any of the ordinary methods. They may be best separated by Bunsen's process with sulphurous acid, which, though far from perfect, is superior to all others. Lenssen's method, in which the arsenic is precipitated from the sulphur solution of the two metals as ammonia-magnesian arsenate, is worthless.

A couple of years ago I made a few experiments upon indirectly determining the proportions of tin and antimony in alloys of the two metals. I oxydized a weighed quantity of the alloy with nitric acid in a porcelain crucible, heated the resulting oxyds with ammonic nitrate, and then, regarding the tin as converted into SnO_2 and the antimony into Sb_2O_4, calculated the proportions of the metals from the increase in weight. This method, although by no means giving me accurate results, served very well for rough approximate determinations. I cite it here simply as an easy and convenient process for obtaining a close idea of the constitution of any alloy composed of the two metals. Possibly the method might be so modified as to give accurate determinations.

ART. VII.—*Notes on the structure of the Crinoidea, Cystidea, and Blastoidea;* by E. BILLINGS, F.G.S., Palæontologist of the Geological Survey of Canada.

[Continued from this Journal, II, vol. xlviii, p. 83.]

5. *On the homologies of the respiratory organs of the Palæozoic and recent Echinoderms, and on the "Convoluted Plate" of the Crinoidea.*

IN a former note I have advanced the opinion that:—"The grooves on the ventral disc of *Cyathocrinus* and, also, the internal "*convoluted plate*" of the Palæozoic Crinoids, with the tubes radiating therefrom, belong to the respiratory and, perhaps, in part, to the circulatory systems—not to the digestive system. The convoluted plate with its thickened border seems to foreshadow the "œsophageal circular canal" with a pendant madreporic apparatus as in the Holothuridea." (This Journal, II, vol. xlviii, p. 73). I should have referred it to the madreporic system of the existing Echinodermata in general, instead of to that of the Holothuridea in particular. At the time the note was written I had in view the madreporic sack of *Holothuria* which, as will be shown further on, most resembles in form that of *Actinocrinus.* The figures and descriptions, which follow, are intended to show the gradual passage or conversion of the respiratory organs of the *Cystidea*, *Blastoidea* and *Palæocrinoidea* into the ambulacral canal system of the recent echinoderms, and that as the convoluted plates of the former have the same structure and connections as the madreporic sacks and tubes or sand canals of the latter, they are, most probably, all the homologues of each other.

Among the Cystideans we find several genera, such as *Cryptocrinites*, *Malocystites*, *Trochocystites*, and apparently some others, whose test is totally destitute of respiratory pores, being composed of simple, solid plates like those of the ordinary Crinoidea. In a second group of genera, among which may be enumerated *Caryocystites*, *Echinósphærites*, *Palæocystites and Protocystites*,

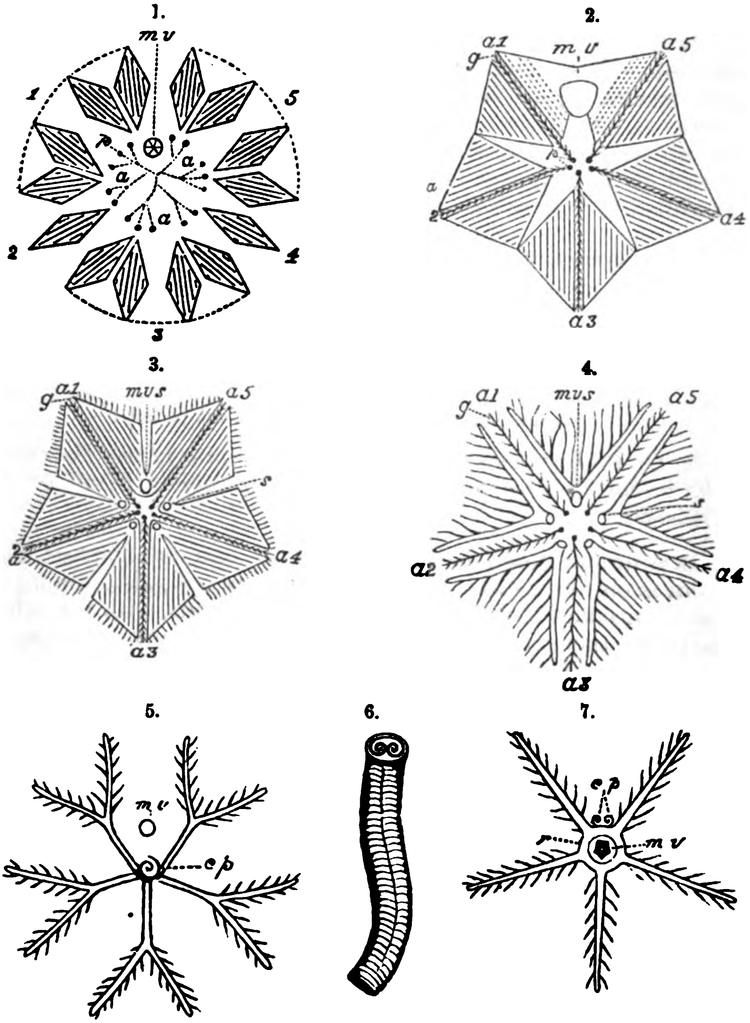

Fig. 1. The upper part of *Caryocrinus ornatus*, the test being removed in order to show the internal structure of the fourteen hydrospires that surround the summit. The parallel lines represent the flat tubes. The other figures exhibit the modifications which the hydrospires undergo in passing through:— 2. *Codaster*. 3. *Pentremites* with broad ambulacra. 4. *Pentremites* with single tubes. 5. *Palæozoic Crinoids* with a convoluted plate attached to the centre of radiation. 6. Sand canal or madreporic tube of a starfish inclosing a doubly convoluted plate. 7. Ambulacral canals of a starfish with the doubly convoluted plate of the sand canal attached to the œsophageal ring. The following letters have the same reference in all the figures in which they occur: *a*, an arm or ambulacrum; *m v*, mouth and vent combined in a single aperture; *m v s*, mouth, vent and spiracle; *g*, ambulacral groove; *p*, ovarian pore; *s*, spiracle; *c p*, convoluted plate; *r*, œsophageal ring.

the whole of the external integument seems to have been respiratory, as all, or nearly all, of the plates of which it is composed, are more or less occupied by variously arranged, poriferous or tubular structures. The Cystideans of these two groups hold the lowest rank of all those known. In their general structure they are mere sacks of a globular, ovate or, (as in the case of *Trochocystites*) flattened form. Their test consists of an indefinite number of plates without any radiated arrangement. They were also, according to our present knowledge, the first to make their appearance, two of the genera, *Trochocystites* and *Eocystites*, having been discovered in the primordial zone. No other echinoderms have been found in rocks of so ancient a date.

Next in order may be placed those genera whose test is composed of a definite number of plates, which have, to some extent, a quinary arrangement. Thus, *Glyptocystites*, *Echinoencrinites*, *Apiocystites* and several others, have each four series of calycine plates, of which there are four plates in the basal and five in each of the other three series. The respiratory areas or hydrospires are reduced in number—ten to thirteen in *Glyptocystites* and three in most of the other genera of the group. Neither in the plates nor in the hydrospires is there exhibited any tendency to a radiated arrangement. The most ancient genus of this family is *Glyptocystites*, which first appears in the Chazy limestone and seems to have become extinct in the Trenton. The other genera occur in various horizons between the Chazy and the Devonian.

In the genera *Hemicosmites* and *Caryocrinus* the hydrospires in the upper part of the test converge toward, but do not reach, the central point of the apex, thus forming the commencement of that concentration and complete radiation which is exhibited in the ambulacral canal system of the higher echinoderms. In a former note (this Jour., II, xlviii, p. 77,) it is pointed out that *Caryocrinus* has thirty hydrospires,—ten at the base with their longer diagonals vertical,—a zone of six round the middle with their diagonals horizontal and a third band of fourteen around the upper part of the fossil. These latter are represented in fig. 1, as if spread out on a plane surface. On consulting this figure it will be seen that the flat tubes of the hydrospires, represented by the parallel lines, all converge toward the central point from which the dotted lines radiate. This point is the position of the mouth in the recent echinoderms, but in *Caryocrinus* it is occupied by a large solid imperforate plate. The hydrospires are arranged in five groups. Commencing at *m v* and going round by 1, 2, &c., there are four in the first group; one in the second; four in the third; one in the fourth and four in the fifth. These five groups represent the five ambulacral canals of the recent echinoderms. In the specimen from which this diagram was constructed there are the bases of fifteen free arms to be seen situated at the outer extremities of the dotted lines. At the base of each arm there is a small pore, *p*, which I believe to have been exclusively ovarian in its function. The hydrospires have no connection whatever with the arms and are, moreover, all of them entirely separated from each other. If then they represent the ambulacral system of the recent echinoderms, it is quite certain that that system was at first, (or in the undeveloped stage in which it existed in the Cystidea,) destitute of the œsophageal ring.

In *Codaster* a further concentration of the respiratory organs is exhibited. There are here only five hydrospires and they are all confined to the circle around the apex. Two of them are incomplete in order to make room for the large mouth and vent (*m v*, fig. 2.) They are each divided into two halves by an arm, *a*1, *a*2, &c. They are only connected with the arms to this extent, that these latter lie back upon them. The arms are provided with pinnulæ but it is not at all certain that they (the pinnulæ) were in any direct communication with the hydrospires. It is evident that *in all the Cystidea*, (and in none is it more obvious than in *Caryocrinus*), there was no connection between the hydrospires and the pinnulæ. The main difference (so far as regards the evidence of the presence or absence of such a connection) between *Caryocrinus* and *Codaster*, consists in this, that in the former the arms are erect and do not touch the hydrospires, whereas in the latter they are recumbent and lie back upon them. Each of the arms of *Codaster* has a fine ambulacral groove and all of the grooves terminate in a single central aperture. But as this aperture was covered over by a thin plated integument, as in the Blastoidea, I have not shown it in the diagram, but only the five pores, *p*.

No one who compares a *Codaster* with a *Pentremites* (the internal structure of the latter being visible) can doubt that the hydrospires of the two genera are perfectly homologous organs. If we grind off the test of a species of the latter genus, selecting one for the purpose which has broad petaloid ambulacra such as those of *P. Schultzii*, the structure exposed will be that represented in the diagram, fig. 3. In *Pentremites* as in *Codaster*, the five hydrospires are divided into ten equal parts by the five rays, *a*1, *a*2, &c. In *Codaster* these ten parts remain entirely separate from each other, but in *Pentremites* they are re-united in pairs, the two in each interradial space, being so connected, at their inner angles, that their internal cavities open out to the exterior through a single orifice or spiracle (*s*, figs. 3 and 4). This is best shown in fig. 4, intended to represent the structure of *P. ellepticus* (Sowerby) as described by Mr. Rofe, Geol. Mag., vol. ii, p. 249. In this species the hydrospires instead of being formed of broad sacks, with a number of folds on one side, consist of ten simple cylindrical tubes connected together in five pairs. The only difference between the structure of fig. 3 and fig. 4 is in the width of the tubes and in the absence of folds in the latter. These two forms are moreover connected by intermediate grades. Species with 11, 10, 8, 6, 5, 4 and 2 folds being known, there is thus established a gradual transition from the broad petaloid form to the single cylindrical tube.

Between the *Cystidea* and the *Blastoidea* the most important changes are, that in the latter the hydrospires become connected in pairs, and, also, are brought into direct communication with the pinnulæ. In the Palæozoic Crinoidea (or at least in many of them) concentration is carried one step further forward, the five pairs of hydrospires being here all connected together at the centre as in fig. 5. There is as yet no œsophageal ring, (as I understand it) but in its place the convoluted plate described in the excellent papers of Messrs. Meek and Worthen This organ, according to the authors, consists of a convoluted plate, resembling in form the shell of a *Bulla* or *Scaphander*. It is situated within the body of the Crinoid with its longer axis vertical and the upper end just under the centre of the ventral disc. Its lower extremity approaches but does not quite touch the bottom of the visceral cavity. Its walls are composed of minute polygonal plates or of an extremely delicate network of anastomosing fibres. The five ambulacral canals are attached to the upper extremity, radiate outward to the walls of the cup and are seen to pass through the ambulacral orifices outward into the grooves of the arms. (Ante, vol. xlviii, p. 31.)

The ambulacral canals of the Crinoidea are, for the greater part, respiratory in their function. They are, however, as most naturalists who have studied their structure will admit, truly the homologues of those of the Echinodermata in general. In the higher orders of this class the canals are usually more specialized than they are in the lower; being provided with prehensive or locomotive organs. In all of the existing orders, including the recent Crinoidea, we find an œsophageal ring. To this organ, which is only a continuation of the canals, are attached the madreporic appendages. These consist of small sacks or slender tubes varying greatly in form and number in the different genera. That of the Starfish *Asteracanthion rubens* is thus described by Prof. E. Forbes. "On the dorsal surface is seen a wart-like striated body placed laterally between two of the rays: this is the *madreporiform tubercle or nucleus*. When the animal is cut open, there is seen a curved calcareous column running obliquely from the tubercle to the plates surrounding the mouth; Dr. Sharpey says it opens by a narrow orifice into the circular vessel. It is connected by a membrane with one side of the animal, and is itself invested with a pretty strong skin, which is covered with vibratile cilia. Its form is that of a plate rolled in at the margins till they meet. It feels gritty as if full of sand. When we examine it with the microscope we find it to consist of minute calcareous plates, which are united into plates or joints, so that when the investing membrane is removed it has the appearance of a jointed column. Professor Ehrenberg remarked the former structure, Dr. Sharpey the latter: they are both right. Both structures may be seen in the column of the common cross-fish." (Forbes, British Starfishes, p. 73.)

In Prof. Joh. Müller's work, "Uber den bau der Echinodermen," several forms of the madreporic appendages of the different groups of the recent Echinodermata are described. In general they are composed of a soft or moderately hard skin consisting of a minute tissue of calcareous fibres, or of small polygonal plates. The walls are also, sometimes, minutely poriferous. In all the Holothurians the madreporic organ is a sack attached by one of its ends to the œsophageal canal, the other extremity hanging freely down into the perivisceral cavity, not connected with the opposite body wall as is the sand canal of the starfishes. (Op. cit., p. 84.) In its consisting of a convoluted plate the madreporic organ of *Actinocrinus*, therefore, agrees with that of the starfishes, while in its being only attached at one extremity it resembles that of the Holothurians.

The convoluted plate of the Palæozoic Crinoids and the madreporic sacks and tubes (or sand canals) of the recent Echinoderms, therefore, all agree in the following respects:—

1. They have the same general structure.
2. They are all appendages of the ambulacral system.
3. They are all attached to the same part of the system, that is to say, to the central point from which the canals radiate.

The above seems to me sufficient to make out at least a good *prima facie* case for the position I have assumed. When among the petrified remains of an extinct animal, we find an organ which has the same general form and structure, as has one that occurs in an existing species of the same zoological group, we may, with much probability of being correct in our opinion, conclude that the two are homologous, even although we may not be able positively to see how that of the fossil is connected with any other part. But when, as in this instance, we can actually see that it is an appendage of another organ, or system of organs rather, which is known to be the homologue of the part with which that of the existing species is always correlated, we have evidence of a very high order on which to ground a conclusion. By no other mode of reasoning can we prove that the column of an *Actinocrinus* is the homologue of that of *Pentacrinus caput Medusæ.*

In an important paper entitled "Remarks on the Blastoidea, with descriptions of New Species" which Meek and Worthen have kindly sent me, the authors, in their comments upon my views, state that:—

> "In regard to the internal convoluted organ seen in so many of the *Actinocridæ* belonging to the respiratory instead of the digestive system, we would remark that its large size seems to us a strong objection to such a conclusion. In many instances it so nearly fills the whole internal cavity that there would appear to be entirely inadequate space left for an organ like a digestive sack, outside of it, while the volutions within would preclude the presence of an independent digestive sack there. In addition to this, the entire absence, so far as we can ascertain, of any analogous, internal respiratory organ in the whole range of the recent *Echinodermata*, including the existing Crinoids, would appear to be against the conclusion that this is such, unless we adopt the conclusion of Dujardin and Hupé, that the Palæozoic Crinoids had no internal digestive organs, and were nourished by absorption over the whole surface. We should certainly think it far more probable that this spiral organ is the digestive sack, than a part of a respiratory apparatus."

The objection here advanced does not appear to me to be a strong one. In many of the lower animals the digestive organs are of inconsiderable size in proportion to the whole bulk. In the Brachiopoda, for instance, the spiral ciliated arms fill nearly the whole of the internal cavity, the digestive sack being very small and occupying only a limited space near the hinge. These arms, although not the homologues of the convoluted plates of the Palæozoic Crinoids, have a strong resemblance to them, and are, moreover, at least to some extent, subservient to respiration. They are certainly not digestive sacks. In the recent echinoderms the intestine is usually a slender tube with one or more curves between the mouth and the anus. It fills only a small part of the cavity of the body, the remainder being occupied mostly by the chylaqueous fluid, which is constantly in motion and undergoing æration, through the agency of various organs, such as the respiratory tree and branchial cirrhi of the Holothuridea, the dorsal tubuli of the Asteridæ and the ambulacral systems of canals of the class generally. In no division of the animal kingdom do the respiratory organs occupy a larger proportion of the whole bulk than they do in the Echinodermata. The great size which the convoluted plate attains in some of the Crinoids is, therefore, rather more in favor of its being a respiratory than a digestive organ.

Professor Wyville Thomson says that inside of the cavity of the stomach of the recent Crinoid, *Antedon rosaceus*, there is a spiral series of glandular folds which he supposes to be a rudimentary liver. (Phil. Trans. R. S., 1865, p. 525). It is barely possible that the convoluted plate may represent this organ. At present I think it does not.

I believe that the reason why the convoluted plate attained a greater proportional size in the Palæozoic Crinoids, than do the sand canals of the recent echinoderms, is that the function of the system of canals (of which they are all appendages,) was at first mostly respiratory, whereas in the greater number of the existing groups, it is more or less prehensive or locomotive, or both.

[To be continued.]

ART. VIII.—*Upon a new Spectroscope, with Contributions to the Spectral Analysis of the Stars;* by DR. J. C. F. ZÖLLNER.*

STELLAR spectrum analysis, in addition to its revelations concerning the physical constitution of the heavenly bodies, has begun most recently to claim attention in an increasing degree in another no less interesting direction. With the aid of this method the prospect is presented of proving, and under favorable circumstances also of measuring, what influence is exerted on the lines of the spectrum of a star by the components of the relative motion of the earth and star along the line uniting the two bodies.

A single consideration shows that effects which two separated bodies exert upon one another by means of a periodical impulse of a limited rapidity of propagation must be modified by a continual change of the distance separating them. It is Doppler's merit to have first, in the year 1841, recognized the necessity of this influence,† although the conclusions which he derived from it with respect to the color of the stars must be acknowledged as incorrect by reason of a disregard of the invisible parts of the spectrum.

With reference to sound this influence was proved to be conformable to the demands of theory by numerous experiments of Ballot, Mach and others.

On the contrary with reference to light it has not been possible hitherto to establish by observation a trustworthy value (sicher nachweisbare Grösser) of this influence, because even the cosmical movements, which are the greatest at our disposal for this object, are very small in comparison with the rapidity of the propagation of light.

The great improvement however, which optical instruments for the observ tion of spectra have experienced since the discovery of spectrum analysis, presents the prospect of demonstrating this influence by the spectra of the stars. According to theory this influence must show itself in a small displacement of the lines of the spectrum. For example, for a mean velocity of the earth of four German miles per second, this displacement would amount to the tenth part of the distance separating the two sodium lines. This value which is obtained in a very simple way from the velocity of light and the undulation-time of the rays corresponding to the sodium lines, has only quite lately been derived again by J. C. Maxwell, in agreement with earlier computations by F. Eisenlohr* and others.

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* From the Proceedings of the Royal Society of Sciences of Saxony at Leipzig, Session of Feb. 6, 1869. Translated by A. N. Skinner, assistant at the Dearborn Observatory, Chicago, Illinois.

† Doppler, "On the colored light of double stars and of some other stars of the heavens." Transactions of the Bohemian Society of Sciences, vol. ii, (1841–42) p. 465–482.

The amount to be observed of the displacement appeared, however, to Maxwell to be so small, that he closed his considerations concerning this (having reference to the spectroscope hitherto constructed and the method of determining the position of the lines), with the remark: "It cannot be determined by spectroscopic observations with our present instruments, and need not be considered in the discussion of our observations."†

Huggins nevertheless in his most recent memoir,‡ of which the above mentioned investigations of Maxwell are an integral part, attempted the solution of the problem in question by the use of a spectroscope with no less than five prisms, of which two are Amici's, with two flint and three crown-glass prisms.

The diminution of the light caused by so great a number of prisms permits, however, the observation of only the brightest stars. Huggins indeed even confined himself to the communication of his results from observations on Sirius, and he believed here that he found a small displacement of the line F in comparison with the bright hydrogen line produced by a Geissler's tube. The direction and amount of the displacement would indicate an increase in the distance between the earth and Sirius, and this with a velocity of 41·1 English miles per second.

If we eliminate the component of the earth's movement, which at the time of observation amounted to 12 English miles, the resulting velocity with which the sun and Sirius are moving from one another would be 29·4 English miles, or about 6·5 German miles.

Huggins himself regards this result as one affected by great probable error, an error caused partly by the great weakening of the light, already mentioned, from numerous prisms, and partly by the difficulty of comparing the coincidences of the bright lines from terrestrial sources of light with the analogous dark lines of stellar spectra. The latter have sometimes a different appearance; for example, they are blurred on the edge and of different breadth, as is precisely the case with the line F in the spectrum of Sirius.

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* Heidelberg Transactions of the Phys. Med. Soc., vol. iii, p. 190.

† Phil. Trans., 1868, p. 532.

‡ Ibid., p. 535.

The most essential of these difficulties, which have hitherto opposed a definite solution of the problem in question, I believe that I have successfully overcome, by a new construction of the spectroscope, the first example of which I have the honor to exhibit here to the Royal Society.

The arrangement is in essentials the following. The line of light produced by a slit, or a cylindrical lens, lies in the focus of a lens which as in all spectroscopes renders parallel the rays to be dispersed. Then the rays pass through two Amici's direct-vision prism-systems of excellent quality, which I obtained from the optical establishment of Merz in Munich.

These are fastened to one another in such a manner that though each passes one half of the pencil of rays proceeding from the collimator object-glass, and also so that the refracting angles lie on opposite sides. In this way the collected pencil of rays will be dispersed in the two spectra in an opposite direction. The object-glass of the observing telescope, which unites the rays again to an image, is perpendicular to the refracting angles of the prisms placed horizontally, and as in the heliometer, is divided; each of the two halves can be moved micrometrically both parallel to the line of section and perpendicular to it. By means of this we can bring the lines of one spectrum into coincidence with those of the other, and also place the spectra in immediate juxtaposition instead of superposing them, so that one spectrum moves by the other like a vernier, or we can superpose them only partially. By means of this construction not only is the delicate principle of double-images rendered available for the determination of any change whatever in the position of the lines of the spectrum, but *any such change is also doubled*, since its influence appears in the two spectra in an opposite sense.

The principal of the reversion of spectra which lies at the foundation of the instrument described, on account of which I venture to propose for it the name "REVERSION SPECTROSCOPE," can be introduced without using Amici's systems of prisms. It is only needed to reverse one part of the pencil of rays proceeding from a common prism by reflection on a mirror or prism, and then to observe the united pencil of rays exactly as above with a telescope furnished with a divided object-glass. Furthermore, this principle renders the simultaneous introduction of artificial sources of light for the investigation of small changes of refrangibility wholly unnecessary, and permits the perception and measurement of these changes, by means of the changes in position of objects completely similar in kind.

The series of measurements which was carried out both on the dark D line of the solar spectrum, and also on the bright sodium lines of a candle flame impregnated with common salt, (and these I venture to add here to show the working power of the instrument), authorizes the hope, that with the aid of this spectroscope we shall succeed not only in perceiving the influence of the earth's movement, but also in determining it quantitatively with such accuracy as appears desirable for an approximate (vorlaüfig) control of theoretical conclusions.

The numbers cited denote the parts of the micrometer-screw, and refer to the distance between the two sodium lines:

Sodium flame.	Sun.
49·5	49·5
50·5	51·5
53·0	48·1
49·5	48·9
50·6±0·6	49·6±0·5

In the following series of observations the reversion spectroscope was furnished, not only with another micrometer-screw with a somewhat coarser thread, but also with two other systems of prisms whose dispersion in the region of the sodium line is 1·77 times greater than that of the systems used for the above measurements. Likewise the old achromatic object-glasses of the collimator and the observing telescope were replaced by unachromatic ones, by which not only nothing was lost in sharpness of the images, but, as was designed, an advantage was gained in clearness and distinctness by increasing the intensity of light.

SUN.

	Screw divisions.	Deviation from mean.
	67·1	—0·8
	69·4	+1·5
	68·4	+0·5
	67·9	0·0
	66·6	−1·3
	66·1	—1·8
	68·2	+0·3.
	68·0	+0·1
	69·6	+1·7
Mean	67·9±0·3	

According to this, the interval between the two D lines was accurately determined, with a probable error of $\frac{1}{226}$ of its value. But in accordance with facts previously presented, a change of the distance separating the source of light and the spectroscope, with a velocity of four German miles per second, will effect a corresponding displacement of the lines of the two spectra, to the amount of $\frac{1}{8}$ of the interval of the D lines, a quantity which is also about forty times greater than the probable error found above for the mean of nine readings.

If, therefore, in the observation of stellar spectra, a sufficient amount of light can be used, it can be definitely determined in the way stated, whether the expected displacement of the lines of the spectrum occurs or not. In reference to the intensity of light required, I venture to remark, that, for these observations, an unachromatic lens* of one Paris foot in aperture and six feet focal length is at our disposal; its cone of rays one inch from the focus is acted upon by a suitable concave meniscus of flint-glass, and, freed thus as much as possible from spherical and chromatic aberration, is directed to the slit of the spectroscope. I feel that I should here especially point out the fact, that, in the use of a slit, the achromatism of the optical image, for the observation of its spectrum, (especially of individual parts of it), is unessential, and consequently the construction indicated here must claim the important preference of great cheapness in comparison with those with achromatics of strong light. Evidently this advantage must be given up in those cases, where, as in double stars, the desideratum is the sharpest possible separation of the objects under investigation.

I may be permitted perhaps to make some remarks upon problems and methods which refer to spectrum observations on the sun and with which I am at present employed.

The sun possesses a velocity of rotation, by virtue of which a point on its equator moves with a velocity of about 0·25 German mile. If, therefore, with the aid of a heliometer, or in any other way, we produce a double image of the sun, and by a suitable arrangement bring into contact two points of the equatorial limb, then at the point of contact, parts of the sun's surface border upon one another, one of which approaches us with a velocity of the given amount, and the other recedes from us with the same velocity. From this arises a difference of velocity of the parts in contact, in the direction of the line of sight, of about half a German mile. According to previous statements such an amount of movement would cause a change of position of the sodium lines corresponding to the 80th part of the interval between them. Therefore if by combination of a sufficient number of prisms, we succeed in perceiving such a quantity, so as to measure it, we need only to bring the middle of the slit into the line joining the two centers of the images of the sun which are tangent to each other in order to see the two spectra of the sun's limbs which are thus in contact, close together in the field of view, and then, under the most favorable relations, to observe the displacement in question. In this way then the position of the solar equator, and, in case of the practicability of measurements, the velocity of rotation in various heliographic latitudes would be determined, which would be of the greatest interest with reference to the opinions pronounced most recently on this point.

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* **Made in the optical establishment of H. Schroeder in Hamburg.**

But even without regard to a quantitave determination of the phenomenon in question, by means of a proof of it only qualitative, even a simple means would be found of separating *all the lines which arise from absorption in the earth's atmosphere, from those which owe their origin to the solar atmosphere*, since the displacement in question could evidently extend only to the latter.

Another subject for the investigation of spectrum analysis is the protuberances. As is well known, Lockyer and Janssen were the first who succeeded, independently of a total eclipse of the sun, in observing the spectra of these forms, which consist of three bright lines.

At the present time it is the object of most earnest endeavor to discover any methods which permit not only the observation of these lines but also simultaneously of the entire shape of the protuberance.

The length of the bright lines corresponds to the magnitude of the dimension of the protuberance concerned, which occurs in the direction of the slit. Consequently if we bring the slit successively into different positions so that it cuts the protuberance in as many directions, then we are able to construct the form of the image observed, as Lockyer has already done. Thereupon Janssen has proposed the construction of a rotating spectroscope, in order thus with a sufficient rapidity of rotation to survey rapidly the form of the whole protuberance during the continuance of the impression of light.

·Leaving out of consideration the mechanical difficulties of such a rotating spectroscope in which one of the bright protuberance lines must lie accurately in the axis of rotation, the design in view can be accomplished in a simpler and more complete manner by an oscillation of the slit perpendicular to its direction. In this way the protuberance could be observed in three differently colored images corresponding to the three different lines of its spectrum.

In this method, with a movable slit, the changes of brightness, through which the protuberance passes from its base, will however be considerably weakened, in proportion to the length of the path passed over by the slit; in the rotating spectroscope especially, the brightness of the protuberance itself would be weakened from the center of rotation out to the edge, and consequently the observation of the natural relations of the brightness of the image would be frustrated.

For this reason I have in view the introduction of another very simple method for the attainment of the object in question, of the practicability of which I am already convinced by experiments on terrestrial sources of light, to be described more in detail below. The principles on which this method depends are the following:

1. The apparent brightness (glanz, *claritas visa*)* of a strip of a protuberance is independent of the aperture of the slit under the hypothesis that it continues to have a perceptible breadth on the retina.

2. The brightness of the superposed spectrum increases proportionally to the width of the slit.

3. With an oscillating or rotating slit, the brightness of the superposed spectrum remains unchanged, while that of the image of the protuberance arising from the permanence of the impressions of light, diminishes on the other hand in accordance with a law depending on the number and duration of the excitations of the point of the retina concerned, which occur in a unit of time, and also upon the refrangibility of the strip of the protuberance observed.

If, for simplicity's sake, we suppose that the entire surface, over which the slit moves in its rotation or oscillation, is filled up by the protuberance, and if we suppose also that the intensity of the image arising, is inversely proportional to that surface, (corresponding to a uniform extension, over the surface, of the light passing through the slit), then under the assumption of the above three propositions, the relation of intensity between back-ground and protuberance, would remain the same; and we may

First, reduce the brightness of the image of the protuberance by an *oscillation* of the slit, and by this leave unchanged the brightness of the superposed spectrum (by 2); or we may

Secondly, open the slit *unmoved* so far that its aperture extends at once over the space, which in the first case the oscillation extended over. In this (by 1) the apparent brightness of the protuberance remains unchanged, but that of the back-ground is increased in the same ratio that it was previously weakened. Consequently under the suppositions made, the designed object would be accomplished much more simply in the second way, if, because of its blinding, care is continually exercised that the intense light of the real body of the sun should not penetrate the slit.

The slit need be opened only just so far that the protuberance or a part of it may appear in the opening. A suitable weakening of the entire field of view must be provided for, by means of polarizing or absorbing media, which are to be placed before the eye-piece, in order that the relative intensity between the protuberance and the superposed spectrum may be permitted to stand out as strongly as possible to the perception.

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* Lambert, Photometria, etc. §§ 36 and 37.

Led by these conclusions, I have sought, with the aid of terrestrial sources of light, to realize the conditions under which the protuberances are visible, in order, in this way, to test both methods and to convince myself of their practicability. For the better comprehension of the described experiments, let the following remarks first be premised.

The reason why the protuberances are invisible under ordinary circumstances, by screening off the intense image of the sun to the edge, lies in the strongly illuminated part of our atmosphere covering the image of the protuberance. In a total eclipse of the sun this superposed light is weakened to such an extent, that then the intensely illuminated protuberances raise themselves from the illuminated parts of the corona.

We can make an approximate estimate of the amount of the reduction of the diffused light of our atmosphere required for this, if we assume the mean illumination of the atmosphere in a total eclipse equal to that of a mean full moon. According to my photometric measurements* this illumination is 618000 times weaker than that produced by the sun; consequently then the selective absorption of colored media must stand in a similar relation in reference to the homogeneous light of protuberances, if we wish to make the protuberances visible in this way without dispersion as is generally sought at the present time.

On the contrary the possibility of accomplishing this end, with the aid of the prism by the dispersion of the superposed atmospheric light, depends essentially upon the circumstance, that this light is composed of rays of all degrees of refrangibility, while that of the protuberances, however, is composed of only three homogeneous assemblages of rays.

The superposition of an unhomogeneous mass of light upon a body shining with homogeneous light and limited by sharp boundaries, I have accomplished artificially, in the following way. The wick of an alcohol flame was impregnated with chlorid of sodium and chlorid of lithium. At a distance of 18 feet before this flame, a piece of plate glass was so set, at an angle of 45° to the direction of the observation, that the reflected image of a petroleum flame at one side covered the faintly shining alcohol flame, and, by reason of its much greater intensity, rendered it completely invisible. About one foot before the reflecting plate of glass, was situated a small lens of six inches focus, which directed a small image of the alcohol flame to the slit of the spectroscope. The latter was fastened to the end of a spring ten inches long, by means of which it could be put in oscillation of sufficient magnitude for about five minutes by first removing it from its place of equilibrium and then releasing it. Next then, the aperture of the slit was so far decreased, that, with the slit at rest, the double line D and, proportionally faint, the lithium line also appeared sharply defined in the field.

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* Photometrische Untersuchungen, etc., p. 105, fol., Leipsic, 1865.

As soon as the slit was put in oscillation, these lines transformed themselves into sharp images of the alcohol flame, of which the two sodium images covered about half. The apparent brightness of these three images was much less than that of the bright lines, and in consequence of this also their relief from the diffusely illuminated spectrum ground was in the same ratio less distinct than that of the lines with the slit in a state of rest.

When now I made use of the second of the above proposed methods, and opened the unmoved slit so far that the little image of the alcohol flame was only just contained by the rectangular aperture of the slit, I was surprised by a far greater delicacy and distinctness, with which the image of the flame relieved itself from the diffusely illuminated spectrum ground. Consequently the above reduction of the apparent brilliancy of the protuberance with an oscillating slit, assumed in the theoretical discussion to be in accordance with a simple law, appeared quite strongly to incline our favor to the method last employed.

I remark here that for this experiment only one of the above mentioned new prisms was employed. But it is evident that with an additional dispersion, the diminution of the superposed unhomogeneous light can be increased at pleasure.

As may be seen, no difficulty in principle stands in the way of the application of this method to the protuberances of the sun.* Practical success, however, is dependent essentially on this, whether a sufficiently strong dispersion of light can be obtained for the actual relation of intensity between the homogeneous light of the protuberances and of the superposed light of the atmosphere. If, however, we are authorized to conclude upon a very considerable relative brilliancy of the protuberances, from the intensity and distinctness with which their lines, especially the middle one, appear,—of which I am convinced by an actual inspection on the 24th of December of the past year at the observatory at Berlin,—then the means at my command at present consisting of four excellent systems of prisms must indeed be sufficient to solve satisfactorily in the way here proposed the problem of the visibility of the protuberances.

Supplement.

According to a communication by letter, lately received from Dr. Schallen at Cologne, Mr. Lockyer also has succeeded in observing the solar protuberances in their whole extent, according to the method developed by me in the above communication. By this communication, Mr. Lockyer has employed a spectroscope with *seven* prisms, and has communicated his results in a report held in the Royal Institution, which ought to appear in print June 15th. Since I have neither received this report up to the present time, nor even learned any details concerning Mr. Lockyer's results, it may be permitted me here to communicate the following in regard to the procedure employed by me.

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* On account of an incomplete arrangement of the instrument demanded I have till now been obliged to refrain from an actual trial of this method on the sun.

The spectroscope, which has been made according to my designs in the optical establishment of Mr. Fauber in Leipsic, possesses one excellent direct vision prism by Merz. The spectroscope was fastened in a suitable way to the six feet refractor of this observatory. The height of the slit amounted to 6′ 20″ of arc, and the aperture varied with the height and magnitude of the protuberance observed. And here let it be remarked that it is most advantageous in observing to bring the length of the slit tangent to the sun's limb. By this plan on the one hand a greater segment of the sun's limb is surveyed at one view, and on the other the advantage is gained of determining with great accuracy the position angle of the protuberance, since the entrance of the sun's disc makes itself immediately noticeable by a flashing out of a narrow band-formed spectrum in the middle of the field of view. This point of the first flashing out can be easily brought to the place of the protuberance in question by revolving the spectroscope about its longitudinal axis, and in this case, as is readily perceived, the slit is tangent to the point of the sun's limb occupied by the protuberance. The actual place of the spectroscope can be read off on a divided circle, and this gives the position angle of the protuberance.

In order to bring individual points of the sun's limb conveniently before the slit of the spectroscope, two different methods can be employed. By one method the object-glass of the refractor is so fastened in a ring that its optical axis is inclined about 15′ to the longitudinal axis of the telescope. If this ring is revolved by the help of a screw on the telescope to be worked by the observer, the optical axis of the object-glass describes a cone of about 30′ opening, so that successively different parts of the limb come in focus before the middle of the slit. Of course the position of the slit must be varied in a corresponding way by a revolution of the spectroscope.

By the other method, which affords the advantage of an unchanged position of the slit, the rays before their union into an image are sent through a reversion-prism, so called. If this is rotated about the axis of the instrument, the image of the sun also revolves about its center and permits successively different points of the limb to fall on the slit. The angle of position is determined by the position of the reversion-prism.

The size of the sun's image in the refractor, or, in other words, the focal length of the object-glass employed, plays an important part in the whole method. It follows immediately from the theory of the method developed above, that *with the same spectroscope* the contrast between protuberance and back-ground is dependent only upon the width of the slit. Since, then, with a *constant* width of slit, the smaller the sun's image, so much a greater part of the protuberance is surveyed at once, it follows that we must not seek to accomplish the amplification of the protuberance by the sun's image, that is, with a long focus of the object-glass of the refractor, but as much as possible by the lens apparatus of the spectroscope. This can be easily accomplished by using a collimator with a relatively short focus to that of the object-glass of the refractor. Suppose, for example, we have a refractor of ten feet focus with a spectroscope attached, in which the focal length of both object-glasses are equally large. If now with this it is required to open the slit to the breadth of one millimeter in order to take in at one view a protuberance of a certain size, with an image of the sun $\frac{1}{10}$ smaller, this aperture could be reduced to $\frac{1}{10}$ of a millimeter, through which not only can the protuberance be seen in its whole extent, but also with a ten times greater contrast with the spectrum back-ground. In order now to obtain again the amplification of the protuberance *in the field of view*, which was lost by the diminution of the sun's image, the focus of the collimator need only be made ten times shorter than that of the spectroscope. To continue with the example proposed, a ten times better effect would be obtained, *giving the same optical amplification* of the protuberance *with the same prism systems*, if there is chosen in place of the ten feet refractor a telescope of only one foot focus, and for the focal distance of the collimator about two inches, and that of the observing telescope about twenty inches. The quality of the images in this is influenced very little, as far as affected by the lens system, since the defect of chromatic aberration does not enter at all on account of the homogeneity of the light of the protuberances; consequently, properly selected unachromatic lenses can be used for such combinations without any hesitation, as I have convinced myself by numerous trials. The extraordinary compactness which such instruments for the observation of the solar protuberances possess, permits a very delicate clock movement, and presents the prospect of realizing in this simple way the idea expressed already in my former communication, namely, an artificial total eclipse of the sun, of an arbitrary length, for the observation in the future of all the protuberances present on the sun's limb at the same time.

Leipsic, August 26, 1869.

ART. IX.—*Polarity and Polycephalism, an essay on Individuality;* by H. JAMES CLARK, A.B., B.S., Prof. Nat. Hist. Kentucky University; Lexington, Ky.*

WE have already, in a general work† upon the development, morphology and classification of animals, entered our protest against that theory of individuality which assumes that the medusoid genitalia of Hydromedusæ should be considered as individuals in a higher sense than the hydræ are, no matter to how low a degree of development they descend nor at how high an elevation they arrive in the complicity and differentiation of their parts. We still adhere to that protest as far as the hydræ and medusoids are related to each other; but look upon them both in a modified light in reference to their individuality. We suppose it will not be questioned that, in the main, naturalists and physiologists have always defined in their own minds, and in their teachings, the zoölogical *individual* to be a *monocephalic* being; that they have taken as their standard the most highly developed creatures of the animal kingdom, whose *oneness* and independence place them on an equal footing with man in these respects. In the discussion of late years upon the individuality of the lower, compound, colonial denizens of the water, the main points at issue have always been to determine whether a certain form was, on one hand, an *individual*, either in its highest sense (a monomeric, independent integral) or one of several interdependent individuals which constitute a colony (a polymeric integer), or, on the other hand, was an *organ*, which formed only a part of an individual, whether the latter be monomerous (as in Hybocodon and Corymorpha producing free medusæ), or polymerous (as in Coryne with free medusæ).

The possibility of a third category of individuality had not arisen in the minds of philosophic naturalists until the question of the bilaterality of the two lower grand divisions of the animal kingdom had been discussed so vigorously, and elevated to such a prominence among the theories of the day as to extend its influence even to the determination of the oneness or duality of the members of the highest of all grand divisions, and indeed the highest of all animals, man himself. Here at this point we find breaking in upon us the Teratological essays of St. Hilaire, and the more recent decisions of Wyman upon the same subject, with the strange confirmations of Lereboullet, by his discoveries of the fissigemmation of the piscine egg, and the evolution of two heads or two tails from one center of development,—the *dualistic* tendency of the highest vertebrate emphasized by the presentation of the living, tangible reality.*

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* This is an extract from a forthcoming memoir on the anatomy and physiology of Lucernariæ.

† Mind in Nature, or the Origin of Life and the Mode of Development of Animals. New York, D. Appleton & Co. 1865.

Such possibilities among the vertebrata staring us in the face could not but send the thoughts flashing back among the inferior, less determined, less differentiated organizations; and the mind's eye needed not to dwell long among the many-headed Polypi and Hydromedusæ, the Crinoidæ, Bryozoa, Ascidiadæ, Pyrosomidæ, Salpæ, &c., before discovering a multitude of more than shadowy tendencies; it became fixed upon numerous sharply and clearly established, unmistakable dualities and pluralities; all arising from one common center, the *ovum*. Had we not the problem of plural individuality solved here,—*a polycephalism—?* the diffuse vitality of the animal-egg of the lowest ranks of life outspoken in the indetermined number and localization of the subdivisions of the Polyp or Hydromedusa corporation; and even the organization itself undecided as to whether it should exemplify its oneness in a simple unit of form, as in the *pseudoindividuum* of Bryozoa, Ascidiæ, or resolve its offices and configuration into the repetitive, multiplied sameness of the sexless and sexual *proles* of Salpæ, Tæniæ, Annelidæ and Hydromedusæ, or the excessive reiterations of the genitalia of Polypi.

The old type of monomerism, the vertebrate individual *par excellence*, has then become the modern, more than transcendental *duality*. The originals of multitudes of figures in St. Hilaire's 'Teratologie,' of the memoir of Lereboullet, and of the condensed aphoristic sketches of Wyman stand forth the real, material embodiments of the idea upon which all sentient life is founded. *Bilaterality* does not express the thought, it embraces too little; it is to be classed with antero-posteriority and dorso-ventrality, to signify the *subdominant* features of the animal architecture; features which evolve themselves as the concomitant resultants of the development of the primitive dominant which originally gave shape to *bipolar ovum*. The embryologist, and to his thoughts the subject is most germane, reflecting upon the physical aspects of the forming egg, would naturally arrange its features in two antagonistic fields; and thereupon attempting to define their position in regard to the contour of the concrete sphere, almost inevitably would give utterance to the word *polarity*. This is the dominant, main idea of sentient life.

It is *polarity* which is evinced when the self-dispersing, self-repellant potentiality of the animal-egg lays down the right and left of the germ on opposing sides of a line; when the cephalic and caudal areas grow in opposite directions from a common point of emanation; or when the animal and vegetative foundations project themselves into diametrically diverse, dorsal and ventral spaces. Each and all of these phenomena have a common point to rest upon; and they proclaim, by their mode of operation, the controlling influence of a power which, fixing itself upon that point, as it were, radiates itself through the whole organism, and disposes its several features in such a way that they all display, either in mode of evolution or by a direct connection, a polar tendency; a growing out of one pole and a dispersion toward the opposite one; features most developed and decided in configuration next the point of departure, and least developed and most diffuse and indeterminate in the opposite area; the latter always through life standing in the same relation to the former as supply does to demand, as nutrition does to the power which regulates the absorption of the nutriment.

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* See remarks of the author on this subject in "Mind in Nature," ut Sup., p. 85.

But *bilaterality* carries with it something more than the sheer dextral and sinistral opposition of the lateral halves of the body; it is not merely the bipartition of a *unit* of form; for the distal as well as the proximal edges of these halves,—the free borders and the margins of contact—are mutually interchangeable; the former may take the place of the latter, and yet leave the apparent bipartite unit undisturbed in internal relations.

Antero-posteriority exhibits the same interchangeability as bilaterality, but, although plainly enough, not so conspicuously in a comparative, homological sense as in the physiological interplay of the functions, such as we see in the relations of the allantois to respiration in the embryo, or in the ratio of excretion of the renal organs dependent upon the degree of activity of the respiratory and perspiratory functions; or in the relation of the reproductive organs to the vocal and respiratory, when the former are in an abnormal condition, or when they change from one period of life to another, from youth to adolescence; and in many other interdependent relations familiar to the morphologist of the present day.

Bilaterality, antero-posteriority, and dorso-ventrality, the three principal *subdominants of polarity*, have a very methodical disposition, and are quite pronounced and sharply defined among the higher groups of animals,—the more seemingly *units* of organization,—but if we go to the opposite extreme of grade we shall find, among the lower classes of life, that the polaric element (like the differentiation of organization, and that of function) is in an almost elementary condition, expressing itself vaguely in the scattered heads of a branch of Coryne, or Tubularia, or Clavellina; or a little more determinately in the distichous arrangement of the hydra heads of Dynamena and Sertularia, or in the singularly stellate disposition of the zoöids of Botryllus, with their common cloacal orifice.

When, however, polymerism, in its usually accepted sense, fails, as it does step by step in the gradually rising degrees of rank, polarity gains the ascendency in point of regularity and the closer intimacy and symmetrical arrangement of the components of the organization which it holds sway over. Thus it is that two, or more, scattered, consimilar parts, or complete organizations may combine to form a seeming *one*, an apparent, bipartite or multipartite *unit*. The multiple repetition of heads among the lower polymeric kinds is here reduced to a dual repetition, and the parts condensed into one form an approximative unit, a zoölogical *individuum*, as the highest expression of unity attainable by the vertebrate *zoön*.

The duality, nay the plurality of the subdivision of the vertebrate axis, as illustrated by the embryo fishes of Lereboullet, is recalled in the diffuseness of the many hydræ of the dendritic Campanulariæ, or disguises itself under the interminable heteromorphism of the Siphonophoræ; it is polymerous but dimorphous in Salpa, or polymerous but monomorphous in the fresh water Polyzoa; temporarily a polymerous, monomorphic *individuum* in the fissigemmating Hydra, it eventually resolves itself into disconnected *pseudo-individua;* for a time polymerous but dimorphic in the annelidan Myrianida of Milne-Edwards it finally assumes the appearance of a true, self-contained *individuum* in each one of the separate, independently moving *sexual segments*, and in the original budding-stock (the direct legitimate offspring of the egg) from which they shot forth.

The thorough historian of the multifarious, so-called alternate, generations of the Acalephæ will see nothing but a generative organ in the spermatic and ovarian sacs of Hydra; and detect nothing more in the grape-like clusters about the base of the head of Clava, or in the grouped moniliform projections behind the corona of tentacles of Eudendrium. The polymerism of these organs of Eudendrium is merely, and nothing more than a repetition of the simple sac of Clava; the diversity in form is only apparent. But one step higher in complicity and our observer will find in the tentaculiferous terminations of the reproductive sacs of Thamnocnidia and Parypha a premonition of a forthcoming cephalic independence, such as is already fully exemplified in the many hydras of the polymeric, dendritic mass. A similar progression toward cephalic freedom will also be seen in the simplest generative sacs of Laomedea amphora, and L. flexuosa, &c., and rising through successive degrees of complicity to those of Gonothyrea (Laomedea) Lovenii *All*, which not only are tipped with tentaculate processes as in Parypha and Thamnocnidia, but have developed within them a series of longitudinal tubes, like those in the homologous organ of Tubularia indivisa.

Gradually and methodically the progressive steps of complication lead on, with a more and more marked separation of the genitalia from a direct relation to the general mass, or even to the hydræ in particular, whilst a consentaneous development gathers around them and brings them into immediate alliance with an envelope whose *morph* is only a slightly varied repetition of that of the hydra, but whose greater degree of complicity gives it a better claim to be ranked as the highest among the *cephalic subdivisions* of the body. But the full aim of the train of development is not divulged here; its results only exemplify a part of it in the predominance of the reproductive function and a differentiation of the nutritive cavity into distinct channels of circulation, and the subordination of a definite region of its periphery to a tentacular, prehensile office.

Step by step, however, all the elements of a complete organism are successively absorbed out of the primitive hydra-mass, and remodelled into the fashion of a medusoid; the reproductive character has become a less obtrusive feature; motion attracts attention above all others; prehension has full sway in the highly developed tentacles; and the latter point, like fingers, to the self-sustaining power of the acalephan morph in the complete organization of the longitudinal and circular chymiferous channels, opening into the receptive cavity of a highly flexible, proboscidal *manubrium.* The preliminary processes of fissigemmation are complete; the primary genesis of the *ovum, in its integrity,* is finished; the primitive stock has become differentiated into two widely diverse varieties of one morph, the *hydroid cephalism* and the *medusoid cephalism.* Such is the condition at which the hydromedusariæ of Corymorpha, Hybocodon, Ectopleura (Tubularia) Dumortierii, Pennaria, Coryne mirabilis, Margelis, Bougainvillea, and many Campanulariæ have arrived previous to the disintegration of their mass into the free *pseudo-individual medusoids,* and their less independent, contemporary homologues, the persistent *hydroid cephalisms.*

No one holding the present prevailing views in regard to individuality would find a difficulty in seeing that the members of a chain of Salpæ are so-called *individuals,* notwithstanding they are attached obliquely end to end, and *organically* connected. Now although the self-dividing worm, Myrianida for example, the so-called *asexual* stock may become, by actual separation, two individuals, apparently, viz: *sexless* and *sexual,* yet once they were more closely connected organically than the Salpæ which do not separate. Is now the closer connection of the yet unseparated asexual and sexual parts of the worm to make them less distinct individuals than those of the Salpa? It would seem so, according to the advocates of individualism;

and therefore the Myrianida, with its posterior string of six or seven consecutive sexual *buds* is a *monocephalic* individual. But in the sexless Salpa-form budding the sexual chain we have a closer parallelism with the worm than in the chain alone, in fact an identity of relation; and yet for all that we would not think of calling the stock (sexless) and the chain (sexual) together one individual, with *one* head, but rather *many* headed, or in other words a *polymeric unit or individual*, of sexual and sexless *cephalisms*. Therefore by a parity of reasoning we ought to denominate the Myrianida and its buds as a succession or series of *cephalisms*. The fact that the worm components are more in one line than in the Salpa only makes an *apparently* more individualistic body. Among tapeworms the several heads (*cephaloids*) of the *scolex* (Cœnurus) of Tænia Cœnurus are not arranged in a line end to end, but all are free anteriorly, and connected with each other posteriorly by a common body. The closer connection of the subdivisions of the annelid is only one of degree; and as to having more organs in common than the Salpa it is rather like the community of interest which the coral cephalisms have in the main trunk.

Since the sexual and sexless are necessary to make up a complete organism, i. e., vegetative and reproductive, the one a completement of the other, neither *alone* can represent the *individual* unit, or whole cycle of life: and CEPHALISM therefore is a better term to indicate the potentiality of these subdivisions to live apart, although it does not always occur (as in Corals, Bryozoa, some Campanulariæ and Tubulariæ), or when apart (as in other Tubulariæ [T. Dumortierii], Laomedeæ [Eucope diaphana, &c.,] and Salpæ, Myrianida, &c.,) meaning more or less *incomplete* individuals (pseudo-individuals) which are either mainly vegetative or mainly reproductive, as the case may be.

We look upon cephalism then, on one hand, as having a controlling influence of a low degree of independence when shared in common by the multiple heads of a coral polypidom, and, on the other hand, as attaining to the highest independence as a controlling power, when the multiple parts of a so-called compound individual separate from each other, and are singly under the influence of this power. The latter obtains when Hydra or Actinia separates its buds from itself; or when the sexual part of the annelid worm subdivides from the asexual one. Cephalism of a low degree is more readily recognized in the aggregated cephaloids of Salpa than in the undivided worm, but, unlike the latter, remain connected cephaloids (in the chain) when separated from the budding stock.

By thus dividing the body of a Hydromedusa into two parts, which shall contain, severally, the vegetative dominant (*i. e.*, *vegetative cephaloid*) and the sexual dominant, (*i. e.*, *sexual cephaloid*) we avoid the absurdity of assigning individuality to the *egg-sac* of Hydra and others of its allies which have evidently a mere genital organ. Although we might be inclined to admit that some *cephalisms* may gradually become complete individuals, as when the buds of Actinia or Hydra separate from the parent body; on the other hand we must insist that an *individual* cannot retain the same significance when reduced to a mere *genital organ*, as when, in Coryne, a free medusoid (Sarsia) later in the season becomes an egg-sac; or the free medusa of Tubularia (Ectopleura) Dumortierii is represented in Tubularia (Thamnocnidia) spectabilis and Parypha crocea by a plain sac; or in Siphonophoræ where a subdivision may be either a sexual medusoid, or a sexless swimming-bell, or a mere "scale."

Under the term *cephalism* we include two forms, or *morphs*, viz: (1) the *cephalid*, or such subdivisions of a body as have a complete organization, whether united in common (as in some Vorticellidæ, Corals, Bryozoa, Crinoidæ, some Ascidiadæ, and Pyrosomidæ) or separating singly from the main stock (as in Hydra and Actiniæ), and (2) the *cephaloid*, or those divisions of a fissigemmating body which do not contain a complete organization, and may be either mostly *sexual* (as the so-called medusa of Hydromedusæ, or the posterior divisions of Myrianida and other worms, or the joints of Tænia, or the Cercariæ brood of Distoma, or the chain of Salpa) or mostly vegetative and *sexless* (as the hydra of Hydromedusæ, the Myrianida stock, the head of Tænia, the single, budding stock of Salpa or the budding Cercaria-nurses of Distoma.)

Art. X.—*On Laurentian Rocks in Eastern Massachusetts;* by Dr. T. Sterry Hunt, F.R.S.

In a paper read before the American Association for the Advancement of Science at Washington in April, 1854, and published in this Journal for September in the same year, (vol. xvii, page 193,) I noticed the crystalline limestones of northeastern Massachusetts, which were described by the late Dr. Hitchcock as enclosed in the great gneissic and hornblendic formation stretching through that portion of the state. These limestones, which are met with at various points from Bolton by Chelmsford on to Newburyport, present a close mineralogical resemblance to those of the Adirondacks and Laurentides, and also to those of the Highlands of New York and New Jersey, a resemblance which extends to the gneissic rocks which in these various regions accompany the crystalline limestones. I, at that time, accepted without examination the view maintained by Mather and H. D. Rogers, that these limestones in southern New York and New Jersey were altered Silurian strata, although mineralogically identical with those farther north of undoubted Laurentian age. Led by this conclusion to attach comparatively little importance to mineralogical and lithological resemblances, and guided by other considerations given in the paper just referred to, I then suggested that the crystalline limestones and their accompanying rocks in northeastern Massachusetts might probably be of Devonian age. The subsequent investigations of Hall, Logan and Cooke in the Highlands of New York and New Jersey have however left no doubt that these supposed altered Silurian rocks are really of Laurentian age, and led me to suspect that the same might be the case with those of eastern Massachusetts. This view, which was shared by Prof. James Hall, I ventured to put forward at the meeting of the American Association for the Advancement of Science at Salem in August, 1869, when I showed that it was probable, not only on lithological grounds, but from the fact that the Laurentian rocks appear to the southward of the great paleozoic basin in New Brunswick and Newfoundland, which are geologically but a northeastern prolongation of New England, and moreover from the outcropping of the lowest Silurian strata at Braintree near Boston. A few days later I visited Newburyport, and in company with Dr. Henry C. Perkins of that place, had, for the first time, an opportunity of observing the gneisses and limestones in question. Their aspect confirmed my suspicion of their Laurentian age, and led me to suggest to him the propriety of searching for *Eozoon Canadense* in the limestones which there occurs mingled with serpentine. Specimens of it were thereupon placed in the hands of Mr. Bicknell of Salem, well known as a skilled microscopist, and shortly after it was announced by Dr. Perkins that Mr. Bicknell had discovered in them the Eozoon. This notice, which appeared in September in a Newburyport journal, is reproduced in the American Naturalist for November. My own specimens collected in August last near Newburyport, at the locality known as the Devil's Den, did not however furnish any traces of Eozoon, and I may here remark that I had already, so long ago as 1864, caused slices to be made of a specimen of limestone from that locality, which were then examined by Dr. Dawson with negative results. In November, however, Mr. Bicknell visited Newburyport and got from a quarry about a quarter of a mile distant from the place just mentioned, specimens of a serpentinic limestone in which he again found Eozoon. Slices which he has kindly sent me have also been examined by Dr. Dawson, who confirms Mr. Bicknell's observation, and finds in them *Eozoon Canadense*, though fragmentary and not very well preserved. The tubuli, as in the specimens from Grenville, are injected with serpentine, and may be seen on etched surfaces as well as in transparent slices. A crystalline mineral is however abundantly disseminated in the limestone, and unskilled observers might have difficulty in recognizing the fossil.

Another locality, about twenty-eight miles to the southwestward of Newburyport, has however afforded me much better specimens. In company with Mr. L. S. Burbank of Lowell, a zealous and successful teacher of geology and mineralogy, I visited in October last, the limestone quarries of Chelmsford, some five miles from Lowell. This limestone and its accompanying gneiss closely resemble the Laurentian rocks of other regions, and scapolite, apatite and serpentine occur as associated minerals, though the latter was rare in the quarries then visited. A few days afterward Mr. Burbank kindly sent me specimens of a mixture of limestone and yellowish-green serpentine from another quarry in the vicinity, which I had been unable to visit, and these have proved to be rich in *Eozoon Canadense.* The continuous and complete calcareous skeleton of the fossil does not appear in these specimens, which seem like some portions of the rock from Grenville, as described by Sir W. E. Logan, to be made up of fragments of the calcareous shell of Eozoon, mingled with grains of serpentine, and cemented by crystalline carbonate of lime. In the specimens from Grenville, and from most other localities, the mineral matter replacing the sarcode and filling up the canals and tubuli in the calcareous Eozoon skeleton, is generally serpentine or some other silicate. Both Dawson and Carpenter however, it will be recollected, found that in the fragmentary Eozoon from Madoc, and in some small portions from Grenville, the injected mineral was like the shell itself, pure carbonate of lime, though readily distinguishable by differences in texture and transparency from the shell. Such is also the case with all the Chelmsford specimens yet examined, which abound in fragments of shell exhibiting in a very beautiful manner the cylindrical diverging and branching tubuli. The accompanying serpentine is disseminated in grains, but has no connection with the organic forms, so that, unlike the specimens in which it is the injecting mineral, the structure of these cannot be brought out by etching with acids.

These specimens from Chelmsford, it should be said, have been examined and satisfactorily identified by Dr. Dawson. The argument from mineralogical and lithological resemblances in favor of the Laurentian age of the limestone in question is therefore now supported by the undoubted presence in them of

Eozoon Canadense. In this connection it should be said that the crystalline rocks of Newburyport and Salisbury, though separated in Hitchcock's geological map from the gneisses to the southwest, and united to the syenites of Gloucester and Rockport, seem to me very unlike the latter, and closely related lithologically to the gneiss of Chelmsford, which encloses the crystalline limestone. The crystalline limestones occurring with gneissic rocks near Providence, Rhode Island, merit a careful examination for Eozoon, inasmuch as from their lithological characters they may with probability be supposed to be of Laurentian age.

Montreal, Dec. 13, 1869.

ART. XI.—*Contributions to the Chemistry of Common Salt: with particular reference to our home resources;** by C. A. GOESSMANN, Ph.D., Professor of Chemistry, Massachusetts Agricultural College, Amherst.

HOWEVER chemists and geologists may differ in regard to the methods by which chlorid of sodium has accumulated in the course of time within the waters of the ocean, there is at present but little dissent from the opinion, that the ocean has at all times been charged with salt, and that the saline residues of the oceanic waters of former geological periods, together with those of the present day, furnish us with our natural sources of supply.

The salt of commerce is chlorid of sodium more or less contaminated with various saline admixtures. These foreign substances may differ in *quality* or *quantity.* The differences in the kind of the foreign admixtures are due to the peculiarities of the source used for the manufacture. The differences in the *quantity* of the impurities, so far as the same kind of saline compounds is concerned, are determined not only by the condition of the source, but also by the mode of manufacture and by the amount of care bestowed upon the working. The fitness of a salt for domestic and industrial purposes depends quite frequently not less on its mechanical condition, than on its chemical purity; and as the composition of its natural as well as its artificial solutions exerts a most decided influence on both, it seems but proper that I should briefly consider the chief foreign saline compounds usually associated with the chlorid of sodium. To do this, we must go back to the primitive condition of our planet.

Accepting the theory that our earth has gradually passed from a gaseous to a solid condition, we may assume that the formation of chlorid of sodium took place mainly during the last stages of its consolidation, at a period when the more volatile elements reacted upon each other, in consequence of the diminution of temperature; that subsequently water caused the oxydation of numerous chlorids and sulphids, the acids of sulphur and of chlorine which thence resulted acting upon the solidified surface of the new planet; and, that finally these more or less violent physical and chemical revolutions resulted in the formation of the primitive ocean.

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* Read before the National Academy of Science at its Northampton meeting, August, 1869.

A more prolific field for speculation concerning the kind, the extent, and the order of succession of the chemical reactions, which may have preceded or accompanied the formation of this first ocean, cannot readily be conceived, since more complicated relations of matter and force can scarcely be presented for investigation. Speculations upon the last stages of the chaos, therefore, are very likely to lead us into a chaos of chemistry; for—to point out but one circumstance—the very important question as to the relative intensity of the various chemical agencies which may have been in action at any given point, is, and will always be, a mere matter of conjecture. Inasmuch as all our knowledge concerning chemical affinity, has been obtained by experimenting, under well defined conditions and on a comparatively very limited scale, it is evident that the application even of well established chemical laws to the determination of the violent reactions of the atmospheric agencies of that period upon the solid crust of our earth, should be made with caution. We are not without ingenious statements concerning the chemistry of that stage of our planet's history; yet, as their details in many instances are and always will be subjects of controversy, I deem it advisable to give merely an outline of those probable chemical and physical changes which bear directly upon the question here under discussion.

Taking into consideration the peculiar constitution of primitive rocks, their present surface condition, and the predominance in them of silicic acid, in connection with the probable character of the primitive atmosphere, there appears to be some force in the suggestion, that the decomposition of the silicates of iron, of alumina, of lime, of magnesia, of potassa, and of soda, by means of sulphuric acid, and particularly of hydrochloric acid, may have furnished the saline constituents of this primitive ocean. The compounds resulting from this chemical action being soluble in water, at least in part, were continually carried away in solution and accumulated finally in lakes and oceans. The chemical composition of those oceanic waters was dependent, therefore, on the nature and the extent of the surface disintegration, the concentration as well as the relative proportion of their saline constituents being very different from that found at present. But on the other hand we must concede, that an entire elimination of one or more of these constituents could only be accomplished by such an alteration in the physical and chemical condition upon our planet, or in the nature of the disintegrating agencies, as would create new affinities of sufficient power, to alter the originally existing compounds. The composition of the present ocean, as compared with that of saline deposits of a more ancient date, requires the assumption of such revolutionizing causes; causes, however, which may be looked upon as merely a natural consequence of the lapse of time during the history of our earth. The same agencies in fact, which are still at work in effecting changes in the character of the saline compounds of the present ocean, suffice to explain the gradual transformation of the primitive ocean into that of the present day. The mineral acids, the presence of which we had reason to suspect in the primitive ocean, became neutralized by degrees, and ceased to react upon the newly exposed rock; and as the temperature diminished, a new disintegrating agent, carbonic acid, became active. This acid, then so abundant in the atmosphere, aided by water and oxygen, and later by vegetable and animal life, though a slower is by no means a less powerful agent in effecting the decomposition of exposed rocks; it has enriched and is still enriching the ocean with saline compounds. Carbonates and silicates of alkalies and alkaline earths were in this way introduced into the oceanic waters; and thus a gradual removal of metallic and earthy oxyds, and in some cases of those of the alkaline earths also, was effected. The sulphuric acid, exchanging the oxyds of the earths and the metals for lime, formed a less soluble sulphate, so that the amount present became dependent upon concentration and temperature. The chlorine changed from its combinations with earths and metals, only to the equally soluble compounds of the alkalies and alkaline earths. Taking the chlorid of calcium, for example, which thus far has been noticed in every salt deposit of antetertiary date, and which must have been therefore one of the original constituents of the ocean from which the salt originated, we find it necessary to believe that the chlorids must have always exceeded the sulphates; since an excess of any sulphate, except sulphate of lime, would have caused the decomposition of the chlorid of calcium.* An increase of the chlorids of the alkaline earths, as of calcium and magnesium, and of the alkalies, particularly of sodium, if we may judge from the present composition of the ocean, was the final result of the changes now indicated. The agencies which produced these changes are still at work; and hence the oceanic waters of the present day are liable to similar alteration in composition. There are reasons to suppose that for some time past one of the main changes has been the decomposition of the sulphate of lime by carbonate of magnesia, forming carbonate of lime, sulphate of soda and chlorid of magnesium. The latter compounds are characteristic of the salt deposits of recent date, as well as of the present marine waters; they could make their appearance only after the chlorid of calcium had been removed. There are of course various other means, by which this result may have been accomplished; but I prefer to confine myself to the one now given, as being of particular interest in view of the fact, that chlorid of calcium is a characteristic constituent of the ante-tertiary ocean. These changes in the oceanic waters extend apparently over long periods of time; they are more likely the result of a prolonged than of an intense action,—even from the beginning. For these reasons it must always be quite difficult to point out with anything like a certainty, the condition, and the composition of the ocean waters, during the successive stages in the geological development of our planet. All we can venture to assert upon that point may be summed up in the following statements:—*first*: the similarity of certain extensively distributed metalliferous, and other deposits within the crevices and fissures of primitive rocks, finds an apparently satisfactory explanation in the assumption, that their formation is due to a more or less localized precipitation, under circumstances similar to those we have supposed to exist in the primitive ocean; *second;* the saline compounds contained in the oceanic waters of ante-tertiary date appear to differ essentially from those of the present day; the solutions of the primitive saline deposits of distinctly ante-tertiary date containing in every instance, more or less chlorid of calcium, while the waters of our present ocean are characterized, instead, by an excess of soluble sulphates, as sulphate of magnesia and of soda for instance, which renders the existence there of chlorid of calcium impossible. We do not hesitate on the strength of these observations, to speak in general terms of a primitive ocean,—of an *ante-tertiary* or *Silurian ocean*, and of a *Post-tertiary ocean*, including in the latter that of the present day.

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* The supposition that chlorid of calcium was one of the primitive constituents, can only lose its force when some other general cause for its universal diffusion can be pointed out.

Whenever during the various geological epochs a larger or a smaller body of salt water was cut off from the main ocean, either in consequence of a receding of the ocean, an infiltration into natural basins, or of changes in the level of the strata, and was subsequently placed under favorable climatic conditions for its evaporation and the subsequent preservation either in whole or in part of its saline residue, then a salt deposit was produced. Such saline residues, commonly known as *rock salt*, have been found in almost every geological horizon, from the Silurian upward, and in many localities they are still forming at the present time. Sometimes several independent deposits occur one above the other, interstratified with the rocks of the same geological basin.

Most of the foreign salts which accompanied the chlorid of sodium in the marine waters, being more soluble than the chlorid of sodium itself, accumulated during the evaporation in the residual liquid, commonly known as mother-liquor, which covered, at least in part, the separated crystalline mass of chlorid of sodium and sulphate of lime. The amount of these salts and the manner in which they overlie the solid saline mass, is dependent on the form and the shape of the basin in which the deposits accumulated. As these mother-liquors uniformly contained a large number of deliquescent compounds, we must assume very favorable climatic and meteorological conditions to account for their evaporation, since the rate at which this took place, must have exceeded by far that of the precipitation of moisture. Whether such a state of the atmosphere was the general rule in former ages, is of course quite uncertain. Moreover, although actual investigations have demonstrated the entire absence not unfrequently of the upper layers of such saline deposits, this fact cannot be admitted as an argument against the assumption of a common marine origin of all salt deposits. For, even granting that in former geological ages, the extraordinary state of the atmosphere did exist, which would be required to effect the solidification of the entire saline mass—mother-liquor and all—there remain numerous subsequent influences, by which a part or the whole of the saline residue of the original mother-liquors, even after their complete solidification, could have been removed from a saline deposit. In fact, considering the many casualties to which these saline accumulations are liable in the course of time, it would be strange indeed, if many entire and well preserved marine salt deposits should be found.

We may assume then with some propriety, in cases where salt deposits are found without their associated foreign saline compounds, that these have been lost, either by oozing out, never having been solidified, or after solidification, have been removed by the percolation of surface waters, by subterranean currents, by peculiar secondary chemical and physical reactions or by erosive action on the surface layers. In all probability quite as many deposits have been modified in their physical and chemical characters by the subsequent elevation of their enclosing strata, a circumstance which must have favored the percolation of surface moisture, as have been changed by denudation.* Indeed these changes have sometimes gone so far as to leave nothing but the layers of gypsum which are the substratum of all well investigated rock-salt deposits.

As the circumstances necessary for the preservation of entire salt deposits, or saline marine residues can scarcely have been the same, even throughout the same geological age, it is not surprising, that these deposits so frequently differ in their physical and chemical characters. In fact there is at present but one salt deposit known, and its discovery is of quite recent date (Stassfurth), where the entire saline mass of the ocean has been preserved; in this deposit the various saline compounds are to a large extent arranged in layers, which correspond closely with the degree of their solubility; yet even in this one instance there are facts noticeable, which leave no doubt that portions of the deposit have been exposed to peculiar disturbing influences at a later date.

But not only may salt deposits be placed under conditions which permit their re-solution, there may be in addition a re-evaporation of this solution, producing a secondary salt deposit. This requires only a repetition of the circumstances, which favored the formation and preservation of the original one. A primary salt deposit may be dissolved entirely or only in part; in either case the solution may or may not be changed in composition by filtering through the adjacent beds. Both primary and secondary salt deposits may occur in the same geological (*Post-Silurian*) formation. Secondary reactions of a physical as well as a chemical nature too may alter the character of the surrounding geological formations, and thus may indirectly affect the composition of a salt deposit, as is the case for instance at Stassfurth. The conditions which have now been given may suffice to explain to us the great variations we notice in the chemical composition both of rock salt and of brines; they may also serve as a suitable illustration of the great risk we incur, when we assume to draw conclusions of an absolute character from the geological formation in which the salt deposit has been found, as to the chemical composition of the oceanic waters of that geological age.

With these preliminary remarks upon the origin of chlorid of sodium and its associated salts, I pass on to consider the main sources of supply of common salt, with particular regard to those of this country.

The leading sources of supply for the manufacture of salt, as already stated, are three in number, *Rock salt*, *Brines*, and *Sea-water*. A. *Rock salt:* From what has been said it is manifest that rock salt from different localities may differ widely, both in physical and chemical properties. Usually it occurs either in densely aggregated masses of distinctly cubical crystals, or in compact masses having a conchoidal fracture; it is frequently colorless and transparent, yet is more often either red, yellow, or blue, rarely green. Its most frequent saline admixtures are 1st, sulphate of lime, the chlorids of calcium, magnesium, and potassium, and the bromid and iodid of magnesium; or 2d, the sulphates of lime, magnesia, and soda, the chlorid, bromid, and iodid of magnesium, etc. A rock salt, which contains more than from 2 to 3 per cent of these impurities is unfit for domestic uses; and a salt which contains carbonate, nitrate or borate of soda, or similar foreign substances is, especially in its natural state, frequently unfit even for many manufacturing purposes also.

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* As the rock salt deposit of Petit Anse, Louisiana, for example, and most likely that which furnishes the brines of Onondaga.

Rock salt deposits are frequently intercalated with layers of gypsum and clay, a fact due to the successive periods of evaporation; such conditions require especial care in mining the salt. Colorless and dry rock salt deposits having a fair composition and easy of access are mined directly with advantage; the salt obtained thereby, after being brought into a desirable form, is directly applicable to most domestic purposes, particularly meat packing. Colored salt deposits, or those which suffer from percolating waters, or which contain a large amount of foreign saline admixture, or clay; or finally, those which are located at very great depths, if worked at all, are usually dissolved, and their solutions treated like brines.

The northern part of this continent contains numerous salt deposits; some quite recently discovered, like that upon Petit Anse Island, Vermilion Bay, Louisiana, the one in Canada West, at Goderich on the shores of Lake Huron, and also that of Neyba, St. Domingo, West Indies, being of particular interest. Newspaper reports too tell of mountains of rock salt in Arizona and Nevada. The salt deposit at Goderich is buried in the shales of the upper Silurian (T. S. Hunt), at a depth of from eight to nine hundred feet; it is about forty feet thick, covers so far as present indications show, dozens of square miles, and is in close proximity to Lake Huron; its solution furnishes the superior brines at Goderich.* The salt deposit of Petit Anse, Louisiana, is apparently imbedded in Quaternary formations (E. W. Hilgard), and is covered merely by a diluvial drift from 16 to 18 feet in thickness; its extent is unknown, having been but partially explored. It is accessible by sea and by land, and is within 275 miles of the mouth of the Mississippi river. This Petit Anse rock salt, so far as at present exposed, is one of the purest on record.* At the present time there is but little rock salt, as such, used in the United States. Natural solutions of rock salt furnish us with the brines of Saltville in North Western Virginia, of Goderich, Canada West, and, as I believe, of Onondaga, N. Y.

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* Report on the Salt Resources of Goderich, Canada West, by Chas. A. Goessmann; Syracuse, N. Y., January, 1868.

B. *Brines.*—Brines are either natural or artificial; that is, they are either natural solutions of saline deposits, or they are made artificially by dissolving rock salt. In regard to natural brines we are quite frequently ignorant of the exact location, the extent and the nature of the saline mass from which they originate, while in the case of artificial brines, we are familiar at least to some extent, with the nature of the sources from which our supply is drawn.

Brines differ in strength and in composition, scarcely two of them being alike. In strength they vary from three to twenty-six per cent of saline matter, though weak brines are frequently strengthened for manufacturing purposes by adding rock salt. Their composition depends on the peculiar nature of the original source, and on the secondary influences to which they are exposed in passing to the surface; an intercepting stratum often modifying materially their original composition.† Moreover brines from the same salt deposit frequently differ widely in composition; the upper parts of the deposit containing as a general rule, more of the foreign salts than the lower portion; the exceptions being accounted for by extensive surface erosions and percolating waters.‡ Again, the upper layers of salt deposits being more exposed to surface action, as for instance, to infiltrations, which may cause chemical changes, are thus particularly liable to suffer from the accumulation of foreign admixtures, derived from the disintegration of the overlying rocks. Brines originating from the surface layers of a salt deposit are therefore usually inferior in composition to those coming from its lower portion.

The impurities of brines are those which are found in rock salt, but in many instances they contain also the carbonates of lime, magnesia, and protoxyd of iron, carbonic acid, and hydrosulphuric acid, besides organic matters. These last named have found access to the brine in most cases during its passage to the surface.

Brines are divided§ according to the character of the saline admixture, into two classes; those of the *first class* contain the chlorids of calcium and magnesium, and sulphate of lime; those of the *second class* contain no chlorid of calcium, but only chlorid of magnesium, with the sulphates of lime, soda and magnesia; the first class are supposed to originate chiefly from the saline residue of the oceanic waters of ante-tertiary date, while the second class represent most probably derivatives of the former. All the brines found east of the Mississippi river belong, so far as my own information extends, to the first class, as they contain chlorid of calcium. The second class of brines is largely represented west of that river; for instance in Nebraska, Kansas, and Arkansas.

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* See my statement in a report of the American Bureau of Mines, On the rock salt deposit of Petit Anse, etc., New York, 1867; and also Dr. E. W. Hilgard's statements in Proceedings of American Assoc. for the Advancement of Science, Aug., 1868.

† See my contribution to the Chemistry of Mineral waters, etc. Syracuse, N. Y., Feb., 1866, also this Jour., 1866.

‡ See my contribution to the Chemistry of Brines, this Jour., July, 1867.

§ Ibid., p. 80.

I. 1*st Class of brines and salt.**

	Brine of Goderich C. W.	Brine of Onondaga, N. Y.	Brine of Mich. (Saginaw Valley.)		Salt of Ohio, (Hocking valley.)		Brine of Kentucky.	Brine of Tenn
			I.	II.	I.	II.		
Sulphate of lime,	0·5433	0·5747	0·0755	0·4887	0·0881	0·1060	traces	0·2978
Chlorid of calcium,	0·0216	0·1533	2·9422	0·4020	1·1360	0·6140	0·7009	1·1793
Chl'd of magnes'm,	0·0336	0·1440	1·2616	0·3710	0·4744	0·0409	0·7312	0·9587
" of sodium, etc.	24·1433	15·5317	19·8595	12·5315	92·8	95·7	6·7684	9·8952
Water,	74·8	83·	76	86·0	5·9	3·4	91·	87·

II. 2*d Class of brines and salt*; it includes all kinds of salt made from oceanic waters.*

	Nebraska (brine).	Nebraska (salt).	Kansas (salt).
Sulphate of lime,	0·1256	0·2475	1·1222
Sulphate of soda,	0·5808	0·3912	0·3511
" " magnesia,	----	----	0·1794
Chlorid of magnesium,	0·1542	0·0790	0·2400
Chlorid of sodium, etc.,	0·3150	98·12	93·06
Water,	90·	0·8	4·8

The value of a brine for manufacturing purposes does not depend entirely either upon its density or upon the relative percentage of chlorid of sodium and of foreign saline admixtures; the *kind* of the impurity is of the greatest importance; for instance, the sulphate of lime and even the sulphate of soda are considered far less objectionable, within proper limits, than a corresponding amount of the deliquescent chlorids of calcium and magnesium.

Saline springs are scattered all over the United States, their number having been largely increased by the recent extensive explorations for oil; the amount of brine which they can yield is apparently inexhaustible. Foremost among them are the brines of New York, (Onondaga co.), southeastern Ohio, western Virginia, Michigan, Pennsylvania, and of late Nebraska and Kansas. The brines of New York, Ohio, Virginia, Pennsylvania, Michigan, Tennessee, Kentucky, and Canada West, resemble each other closely. They all belong to the first class of brines, and all contain chlorid of calcium; they differ merely in the relative proportion of the impurities which they contain. The brines of Lincoln co., Nebraska, and of Smoky Hill river valley, Kansas, etc., contain chlorid of magnesium, and the sulphates of lime and soda, but no chlorid of calcium. They represent the second class of brines. Most of the saline waters of this country are, even in their natural state, strong enough to be worked directly by artificial heat. All our home manufactured salt, coarse as well as fine, with the exception of a small quantity obtained from sea-water, has thus far been made from natural brines; fully one half of the whole amount for a number of years having been obtained from the brines of Onondaga, New York State.

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* These analytical statements by no means express the commercial value and the relative purity of the salts and brines, they claim merely to indicate their peculiar character.

C. *Sea-water.*—The water of the ocean is a weak brine; it contains from three and one half to four per cent of saline matter, of which three fourths is chlorid of sodium and one fourth is impurities; it is free from chlorid of calcium, and belongs properly to the second class of natural brines. Its impurities consist mainly of chlorid of magnesium, the sulphates of lime, magnesia and soda, besides smaller quantities of chlorid of potassium, bromid and iodid of magnesium, and carbonate of lime, with traces of oxyd of iron, etc. Sea-water varies but little in composition and concentration, except in localities where either a limited body of water is prevented from unrestricted communication with the ocean, or where a large influx of fresh water causes its dilution. It represents the main source of supply for the manufacture of salt in France, Portugal, Spain, Italy, the West Indies, and Central and South America; it is used also largely for the production of salt in England, Belgium and Holland, being frequently employed for the solution of rock salt of inferior color. In the United States it has been turned to advantage but to a very limited extent. Three hundred to three hundred and fifty thousand bushels cover, in all probability, our present production of salt from sea-water. The States of Massachusetts, North Carolina and Florida on the Atlantic, and of California on the Pacific Ocean, have mainly been interested in this branch of industry. The late increase in the production in Florida, and on the Pacific coast, more than compensates for the falling off elsewhere.

In concluding this paper it may be well to re-state the fact, that the comparative commercial value of various samples of salt, so far as their composition is concerned, does not always depend upon the relative proportion of chlorid of sodium to foreign admixture. A mere analytical statement giving the percentage of impurity, without specifying its kind and nature, gives no reliable standard by which to pronounce upon their commercial value, since the various impurities, which the salt has retained, are objectionable in quite different degrees; thus a sample which contains from one to one and a half per cent of foreign salts, consisting almost exclusively of sulphate of lime, may claim to be a very fair article of common salt; whilst if it contains but one half of that amount of the chlorids of calcium and magnesium or of the sulphates of soda and magnesia, together with the chlorid of magnesium, or of the carbonates of lime and magnesia, it would be considered quite objectionable, at least for table and dairy purposes. Again, the superior fitness of a sample of salt for many domestic purposes, does not depend entirely on its chemical composition; its mechanical condition is quite frequently of not less importance. A well devised mode of manufacture, ought therefore, not only to aim at the most perfect removal of foreign impurities, it must also secure to the salt the best mechanical condition. The manufacturer in selecting a mode of working is consequently limited in his choice; he must often sacrifice purity to mechanical condition, being obliged to adopt a method which promises to secure most economically a desirable article from his brine in the purest condition possible. So far as the form is concerned there are two kinds of salt in commerce; (a), *coarse salt* including salt made by solar heat, and rock salt crushed to a suitable size; and (b), *common fine salt*, or boiled salt obtained by artificial heat and more rapid evaporation; both kinds have their special market value. The amount of salt consumed for the promotion of animal life far exceeds that required by vegetables, but the amount demanded for meat packing and for the dairy business is very much larger. In the industrial arts, we can scarcely claim that any of our home resources have been turned to account. A proper exposition of the various uses to which salt is applied in chemical manufacturing industry would be a description of one of the most important chemical arts of the present day. To do justice to this question would require more time than I feel entitled to claim. I shall therefore confine myself merely to the presentation of a few statistics in regard to the quantity annually produced.

England produces from thirty to thirty-two millions of bushels of salt, (56 lbs. each), of which seventeen millions are used for the manufacture of soda ash, sal soda, caustic soda, bicarbonate of soda, hydrochloric acid, bleaching powder, etc., etc., while about two-thirds of the rest is exported, leaving scarcely one-fifth of its whole production for domestic consumption.* The production of salt in the United States amounts at present to from sixteen to eighteen millions of bushels, and the consumption to from thirty-two to thirty-four millions of bushels; in other words, almost one bushel to every head of its population. This large consumption of salt is due to our extensive meat packing and dairy business; the consumption for manufacturing purposes being scarcely worth mentioning. Almost one-half of the amount of our present consumption, it will be noticed, is imported. We must consequently increase our production to twice the present amount, if we would supply our present demand; and if we would support the industrial arts at the rate England does, we must increase this production more than three times. Our natural sources of supply would, even in their present incomplete state of development, suffice to furnish the amount required. Our prospects might therefore be considered highly gratifying, if the question of quantity only were involved. But as the cheapness of the raw material and of labor are not less important points, which ought to be duly weighed before entering upon a new industrial enterprise in which skill and capital have for years secured a monopoly in the market, we must acknowledge that we are not yet fully prepared to enter the lists as successful competitors. Some of our older salines are not yet sufficiently explored to warrant the expectation of a cheap supply from them in their present condition, and many of our recently discovered brines are too far from cheap communication or from centers of skill and industry, to be to any extent available for our present emergencies.

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* England abolished its tax on salt in 1823. The price has varied only within narrow limits for many years past; the production in some localities has changed much since that time; for instance, Worcestershire produced in 1823 but 9,000

In some of the countries in Europe, where the government holds the salt monopoly for revenue purposes, the practice has for obvious reasons been adopted of taxing the salt used for domestic purposes, while the salt used for manufacturing purposes is supplied at cost, being first rendered unfit for domestic application by the addition of ground charcoal, oxyd of iron, etc. These additions are selected with reference to the particular use for which the salt is intended. May intelligently directed individual enterprise, supported by a wise legislation, soon recognize the proper means by which our home production may be best stimulated, and thus our chemical industry receive the most important element for its successful and rapid development.

tons, while its present production is 200,000 tons; the Cheshire salt works are capable of making one million of tons per year; in both cases the sources of supply exceed the demand. The Worcestershire works export annually 50.000 tons, while the Cheshire works export 650,000 tons. The Stoke's works employ 500 hands, produce about 3,000 tons per week, consuming 1,500 to 2,000 tons of coal. A fair workman at 2s. per ton will make 28s. per week.—(*Chem. News*, No. 377, Feb., 1867, page 88.

Amherst, Mass., August, 1869.

ART. XII.—*Account of a fall of Meteoric Stones near Danville, Ala., with an analysis of the same ;* by J. LAWRENCE SMITH, Louisville, Ky.

ALTHOUGH the meteorite of Danville, Alabama, fell in Nov., 1868, and an analysis has been made of it during the past summer, it is only recently that I obtained a complete account of the phenomena attending its fall.

On Friday evening, Nov. 27th, 1868, about five o'clock, Mr. T. F. Freeman, of Danville, (about lat. 34° 30′ and long. 87° W. Greenwich), on stepping from his house, was startled by a loud report, so much like artillery that for the moment its origin was attributed to the firing of a small piece of artillery kept in the village, but on inquiry it was ascertained that no firing had taken place there, but that the sound was heard at the village, and attributed to very heavy artillery at Decatur, Trinity, Hillsboro, or some other point to the northward of Danville. During the war, artillery had been often heard in the valley of the Tennessee, and various speculations were indulged in as to what was meant by this cannonade at such a time of day and in such a direction.

The following day, Mr. Wm. Brown, living three miles west of Danville, brought to the village a piece of rock which he said fell near him and some laborers, who were picking cotton. He dug it up at a depth of about 1½ to 2 feet. It weighed about 4½ lbs., and had the characteristic aspects of a meteoric stone ; but it was broken by the party obtaining it, and all but about half a pound, now in my possession, has been scattered and probably lost or thrown away.

Several other stones fell in the same vicinity. Some negroes working in a cotton field on the plantation of Capt. McDaniel, half a mile from Danville, heard a body fall with a whizzing, humming sound, and strike the ground near them with tremendous force ; but they were alarmed and did not approach the spot that night ; a rain fell during the night and no trace of it could be found the next day. Various other stones were heard to fall in different parts of the adjacent country. Two brothers, by the name of Wallace, were ploughing in their field, about 1¼ miles N. W. of Danville ; they distinctly heard two or three fainter reports, after the first loud one, and heard the sound of two falling bodies whizzing down, one to the right and the other to the left of them.

With the above data, and the known geography of the country, its direction must have been N. E. and S. W., but it is impossible to say from which of these quarters it came.

The portion of the meteorite that I possess has a large portion of it covered with the usual black crust. Its general aspect is rough and dull; a portion of the outer surface, not covered with the black coating, is nevertheless a surface that it had when it reached the ground, for on this surface are streaks and little patches of a bright, pitchy matter, which was once fused, and was derived either from another part of the coating that was thrown off in a melted state from the coated portion, and whipped around, (as it were), on to the unfused surface as the stone fell through the air, or from an incipient fusion that was begun on the denuded surface, and arrested by the termination of the fall. Where the black crust reaches the denuded places, it appears to be rounded off, as if it had been melted matter passing from another portion of the stone, and rolled over the surface of the borders.

The broken surface has a dark gray color, and is somewhat oolitic in structure, but not as much so as many other meteoric stones. There are veins and patches of a slate-colored mineral running through it. Pyrites and iron are also to be seen diffused through the stone; thin flakes of the iron giving that slickenside-like appearance to a fracture not unfrequently seen in this class of bodies. There seems to be more of iron in the slate-colored mineral than in the other parts. There are a few patches of white mineral, which I take to be enstatite. The specific gravity of the stone is 3·398.

For further examination, a portion of the meteorite was separated mechanically into three parts; the pyrites, the metallic iron, and the earthy minerals. As in the case of most meteorites, the earthy minerals were so intermixed that it was impossible to separate the different varieties, three of which were easily traceable by the eye.

The iron separated with great care from the pulverized meteorite constitutes 3·092 per cent of the entire mass, and an analysis furnished

Iron,	89·513
Nickel,	9·050
Cobalt,	0·521
Copper,	minute quantity
Phosphorus,	0·019
Sulphur,	0·105
	99·208

The sulphid of iron detached very carefully from the mass of the meteorite gave

Iron,	61·11
Sulphur,	39·56
	100·67

which corresponds with the protosulphid of iron, FeS. Whether it contains any of the sulphid known as troilite I am not prepared to say.

The stony minerals were freed as much as possible from iron and pyrites, and one gram treated with ten grams of hydrochloric acid on a water bath, and evaporated nearly to dryness, then filtered and the filtrate well washed; after which, the residue in the filter was warmed with a solution of caustic soda to dissolve any silica belonging to the portion dissolved by the acid; it was then filtered again and washed. The result was

Soluble portion,	60·88
Insoluble portion,	39·12

The treatment by a solution of caustic soda or potash is of importance for a correct result, as otherwise a portion of the silica of the decomposed minerals will be estimated with the portion that is undecomposed.

The insoluble portion was analyzed; for although the analysis made in this way cannot furnish any positive indication in regard to the true mineral constitution of the meteorite, it is, nevertheless, an important guide. It was found to consist of

Silica,	50·08
Alumina,	4·11
Protoxyd of iron,	19·85
Magnesia,	20·14
Lime,	3·90
	98·08

From all the circumstances connected with this mineral, its physical characters, &c., it is doubtless a pyroxene of the augite variety.

The soluble portion, owing to the unavoidable presence of a little iron and pyrites, simply furnished results on analysis that showed it to be mostly olivine. The only matter, as a whole, freed as much as possible from pyrites and nickeliferous iron, gave

Silica,	45·90
Protoxyd of iron,	23·64
Magnesia,	26·52
Alumina,	1·73
Lime,	2·31
Soda,	·51
Potash,	·64
Oxyd of manganese, a minute quantity, not estimated.	
Oxyd of Chrome, " " " "	
Phosphorus, " " " "	
Lithia—marked reaction with the spectroscope.	
Sulphur,	1·01

The excess in the footing up of the analyses above 100 per cent is due to the fact that a part of the iron, estimated as protoxyd, is combined with sulphur forming sulphid of iron.

This meteoric stone is similar in every respect to that which fell March 28th, 1859, in Harrison county, Indiana, (which locality I see referred to, in catalogues of meteorites, as Harrison county, Kentucky). This meteorite is therefore composed of

Nickeliferous Iron,....................
Olivine,.............................
Pyroxene,...........................
Protosulphid of iron,...................

with minute quantities of schreibersite, chrome iron, and probably albite.

In concluding these observations on the Danville meteorite, I cannot but feel more and more convinced of the importance of a thorough reëxamination of the mineral nature of the meteoric stones, and in the present case, I am not at all satisfied that the mineral characteristics are perfectly made out.

Art. XIII.—*Contributions to Zoölogy from the Museum of Yale College. No. V.—Descriptions of Echinoderms and Corals from the Gulf of California;* by A. E. Verrill.

The Museum has recently received a large and important collection of Radiata, collected by Capt. J. Pedersen in the vicinity of La Paz. The following notes and descriptions relate to some of the more interesting species only.

Echinoidea.

Meoma nigra Verrill.

Meoma nigra Verrill, Trans. Connecticut Acad., i, p. 317, 1867.
Kleinia nigra A. Agassiz, Bulletin Mus. Comp. Zoöl., p. 27. 1863.

Of this interesting species there are ten specimens in the collection, which show considerable variation from the type formerly described by me, as well as among themselves.

The largest is 4·85 inches long, 4·25 broad, 2·10 high; the smallest 3·85 long, 3·40 broad, 1·75 high. The outline, as seen from below, varies but little and is broad-oval, somewhat emarginate anteriorly, obliquely truncate posteriorly, and slightly compressed laterally, or, in other words, nearly heart-shaped. The anal area is large, somewhat sunken, and is at the extreme posterior end of the shell, occupying the greater part of the truncated portion. Its form varies from regularly elliptical, acute at each end, to broad-oval, rounded below and acute above; its position varies from nearly vertical to decidedly oblique, and it is so nearly terminal as to produce a posterior emargination in a dorsal view of the shell. In a side view some specimens are decidedly depressed, but most are regularly arched, while one is decidedly elevated at the apex. There is considerable variation in the depth of the anterior ambulacral groove, and also in the number and prominence of the large tubercles, which are more or less restricted to the region enclosed by the peripetalous fasciole. The fasciole itself shows remarkable variations, but does not agree at all with that of *M. grandis*, as figured by Gray. The portion crossing the anterior interambulacral regions varies less than other parts, but in some the intermediate transverse portion is nearly straight, in others strongly curved and often crooked, in one it is bent up into a right angle on each side of the ambulacral groove; its bend or angle near the antero-lateral grooves is also variable, both in form and extent, it being twice as large in some specimens as in others, and in one an irregular, crooked branch passes from the apex of the angle on the left side to the anterior groove. In the posterior interambulacrum the course of the fasciole is quite variable, in five examples it crosses with a strongly curved upward bend, without any distinct angle, rising highest in four specimens on the right side, in the other forming a nearly straight transverse middle portion; in three specimens it forms a sharp angle on the right side; in one a similar angle on the left side; in another there is a strong median angle, its apex pointing to the anal region, and another to the right of it, pointing to the summit; in all the specimens it bends inward further than in *M. grandis*. The lateral part of the fasciole also varies, especially on the left side; in five (but not the same five that agree in the posterior region) it has but one angle, near the antero-lateral grooves, where it rises highest; in three it has two angles, rising highest at the posterior one, and nearly straight between; in two others, which also have two angles, the transverse part is double. On the other side the fasciole varies in the same way, but not in the same specimens, for some have two angles, both on the right and left; others two only on one side; others one on both sides. The anal fasciole is also variable; usually the subanal branch is wanting or indistinct, though indicated by a band of smaller tubercles, but in one specimen it is well marked and the subanal disk is clearly and perfectly circumscribed. In this the subanal disk is very broad, bilobed, narrowest in the middle, scarcely heart-shaped, the anterior border being nearly transverse, and the posterior border nearly parallel with the anal region and about ·15 of an inch from it. In others the posterior border is more curved. One specimen has but three ovarial openings, the rest four. The proportionate length of the ambulacral grooves varies considerably, both in different specimens and on opposite sides of the same individual, sometimes those on the right being longest, sometimes those on the left, and not uncommonly a longer anterior one is offset by a shorter posterior one on the same side.

Mr. A. Agassiz thinks this species may be identical with *M. grandis* Gray, described as from Australia, but if the figure of the last be at all correct my specimens differ widely from that described by Gray. The position of the anal area, especially, is quite different, it being in Gray's figure at a considerable distance from the posterior end, and therefore more ventral and nearer the subanal fasciole. The peripetalous fasciole is also very different from that of any of my specimens.

Agassizia subrotunda Gray.

Catalogue Ech. of British Mus., p. 63, tab. 3, fig. 2; Verrill, Proc. Bost. Soc., vol. xii, p. 381.

A. ovulum Lutken, Vidensk. Medd., p. 134, tab. 2, fig. 8; Verrill, Trans. Conn. Acad., vol. i, p. 320.

Of this species there were about a dozen specimens, mostly more or less broken, which show but little variation and agree well with Gray's figure.

Mr. A. Agassiz regards this and *A. ovulum* Lutk. as identical with *A. scrobiculata* Val., which may well be the case if the figures in the Voyage de la Vénus be incorrect, as he states. The figures are certainly very unlike our specimens.

Clypeaster speciosus Verrill, sp. nov.

Depressed, gradually rising toward the apex; the lower side sometimes slightly concave from near the edge of the mouth, in other specimens flat except close to the mouth, which is much sunken. Outline oblong-pentagonal, with rounded angles and slightly concave sides. The anterior end slightly elongated. Interambulacral regions decidedly concave between the ends of the ambulacral rosette; the ambulacral regions enclosed by the pores slightly raised, narrow, elongated, widening but little outwardly and somewhat acuminate at the end, which is often nearly enclosed by the pores. The interambulacra are broader and decrease much more rapidly toward the apex than in *C. rosaceus.* Anal opening tranvsersely oval, or rounded, situated about its own diameter from the edge of the shell.

Length of largest specimens 4·60 inches; breadth 3·90; height 1·15. Length of anterior petal, from the apex 1·90, its breadth ·82, breadth of enclosed space ·50; length of anterior petals 1·70, breadth ·85, breadth of enclosed space ·48; length of posterior petals 1·80 and 1·85, breadth ·94, of enclosed space ·58 and ·60; diameter of anal opening ·20; of actinal opening ·33.

Thirty-five specimens of this species are contained in the collection. They show but little variation in outline, except what is due to age, though some specimens are more elevated toward the apex than others; in regard to the flatness or concavity of the lower side there is, however, great variation, though Dr. Gray used this character in dividing the genus into sections. The youngest specimens are 2·30 long by 2·10 wide, and are more oval in form and scarcely angular, but have the flatness and form of ambulacral rosette characteristic of the larger specimens, as well as the same position of the anal opening.

From *C. rosaceus* of the Atlantic this species differs widely, the former having a much more elevated and thick form, with broader and more obovate ambulacra, which are much more swollen; the lower side is much more concave, and the anal opening nearer the edge.

Some of the species described by Dr. Gray from the Indo-Pacific faunæ (*C. Australasia*, *C. testudinarius*) seem to be more closely allied, but only a direct comparison of specimens can settle the true relations of these species. It may be that *C. testudinarius* is the same and its locality incorrect, its outline being nearly identical, but the upper side is said to be evenly convex, and the lower side concave from the margin.

This species is of especial interest as the first of the genus known from the Pacific coast of America, although the genus was known to occur on nearly all other tropical coasts.

Encope grandis Agassiz.

Of this very distinct species there are numerous specimens in the collection, varying in size from 3 inches in length by 3·20 wide, to 4·60 inches by 4·40. There is but little variation in outline and general appearance, and in all the margin is thick, with the five large notches widely open, though in the larger there appears to be a tendency to close the anterior pair. The posterior interambulacral opening is large and broad-oval with thickened borders in all, but there is a variation of more than 50 per cent in its relative size; the region around it is in all more elevated than the central region and considerably swollen. The form of the ambulacral rosette varies considerably. The three anterior petals are subequal and usually long-oval, obtusely rounded at the end, but in one case they are narrower and more elliptical, especially the odd anterior one, which is widest in the middle, tapering to each end, and in another they are broader and more dilated outwardly than usual; the two posterior ones are much longer, widest outwardly, and curve somewhat around the posterior opening, but they vary considerably in relative width. The following are the proportions in two extreme specimens:

From abactinal center to posterior edge,	2·20	2·20
Center to anterior edge,	1·98	2·00
Center to lateral edge,	2·20	2·10
Length of anterior odd ambulacral petal, from center,	1·28	1·25
Greatest breadth of do.,	·50	·68
Breadth of its enclosed area,	·20	·30
Length of antero-lateral pair,	1·25	1·15
Breadth of do.,	·50	·65
Breadth of enclosed area,	·16	·27
Length of posterior pair,	1·65	1·55
Breadth of do.,	·45	·62
Breadth of enclosed area,	·12	·20

The branchings of the ambulacral grooves beneath are quite constant in their arrangement, but the relative breadth and form of the enclosed areas are quite as variable as in the dorsal rosette. The region about the anal opening and around the posterior foramen is sometimes deeply concave or excavated; in most cases slightly so; and sometimes not at all.

Encope Californica Verrill, sp. nov.

Test broad, thin at the edge, rounded anteriorly, broadest behind the middle, sub-truncate or rounded posteriorly; usually about as broad as long, sometimes broader than long. Apex behind the center. In profile the outline descends from the center to the anterior edge, but rises from the center to the posterior foramen, from which it descends rapidly to the edge. The posterior interambulacrum is, therefore, swollen and the test is most elevated near its foramen. Ambulacral rosette with the petals long-oval, somewhat obovate, broadly rounded outwardly; the anterior pair shortest and most rounded; the odd anterior one somewhat longer and narrower and a little shorter than those of the posterior pair, which are of about the same form and not curved. Posterior foramen variable in form and size, usually rather small, regularly oval, or rounded, sometimes long oval, or even narrow and elongated, occasionally quite large and broad oval, often obovate beneath, sometimes constricted in the middle. Ambulacral foramina also quite variable in form and size, but commonly small and rather regularly oval, often at a considerable distance from the margin.

Two specimens, showing the extreme variations, give the following measurements:

Length of test,	4·75	4·30
Breadth,	4·65	4·30
Center to anterior edge,	2·45	2·15
" " anterior foramen,	1·80	1·65
" " lateral edge,	2·35	2·15
" " " foramen,	1·60	1·50
" " posterior edge,	2·45	2·25
" " postero-lateral foramen,	1·85	1·70
" " posterior foramen,	1·15	1·10
Length of " "	·67	·60

Breadth of posterior foramen,	·22	·26
Length of anterior ambulacral petal, from center,	1·42	1·32
Breadth where widest,	·65	·50
Breadth of enclosed area,	·30	·18
Length of antero-lateral petals,	1·28	1·10
Breadth,	·67	·50
Breadth of enclosed area,	·28	·20
Length of postero-lateral petals,	1·58	1·35
Breadth,	·68	·53
Breadth of enclosed area,	·25	·16

Of this species there are 74 specimens in the collection from La Paz, and I have seen others from Cape St. Lucas. It varies considerably in outline, and in the form of the openings, especially the posterior one; the ambulacral rosette varies somewhat in the form of the petals, as shown by the above measurements; the ambulacral grooves beneath also vary in direction. But all the specimens agree in having their greatest elevation behind the center, or the posterior interambulacral region swollen. This peculiarity, which is found to depend upon a very different internal structure, will readily separate this species from *E. occidentalis* V., and from *E. micropora* Ag., whether those be the same, as Mr. A. Agassiz supposes, or not.* In *E. occidentalis* the greatest elevation is in front of the center, and there is a regular slope from thence to the broad, thin, posterior edge, and the sections show that the wide space between the central cavity and the posterior foramen is filled with a pretty firm, alveolary tissue having comparatively small spaces, but in *E. Californica* the same region is much less extensive (owing to the relatively larger central cavity and jaws) and is filled with a much less firm and more open tissue, with large cavities.

The difference is therefore analogous, in respect to external form at least, to that which separates *E. Michelini* from *E. emar-*

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* Mr. A. Agassiz, in the Bulletin of the Museum of Comp. Zoölogy, No. 9, p. 266, 1869, says: "from a careful comparison of specimens of *E. cyclopora*, *micropora*, and *perspectiva* (Ag. species) there is no doubt that these are only nominal species, all identical with Verrill's *E. occidentalis*." The latter is the same as *E. tetrapora* Ag. (non Gmelin), but I believe Mr. Agassiz goes too far in uniting all these other forms into one species, for if this can be done there is no reason whatever for keeping the species from the two coasts separate, for there is often much less difference than between some of those forms, which he has united. But since the localities of the types of *E. micropora*, *E. cyclopora*, and *E. perspectiva* were unknown, the two last having been described from unique specimens, it seems to me more probable that some of them represent imperfectly known species from other regions; for such species have been recorded from Africa and other little explored localities. Mr. Agassiz does not state what he regards as the characteristic of the polymorphous species thus constituted, nor in what respect it differs from *E. emarginata* of the Atlantic, nor does it appear certain that he has examined authentic specimens of all these forms. Dr. Lutken referred both *E. micropora* and *E. grandis* to the Atlantic *E. emarginata*, to which he also united *E. Valenciennesii* Ag., *subclausa* Ag., *oblonga* Ag., *quinqueloba* Esch., and *E. Michelini* Ag. In regard to *E. grandis* and *E. Michelini* he appears to have erred, but the figures of *E. micropora* resemble some forms of *E. emarginata* more than *E. occidentalis*, from which *E. cyclopora* and *E. perspectiva* differ still more widely.

ginata,—a distinction which Mr. A. Agassiz admits as valid in that case, where there is, however, much less difference in the form of the rosette and other characters.

The only figure in the monograph by Prof. Agassiz, which approaches this species, is that of *E. emarginata* (Tab. 10), which has, to some extent, the same posterior elevation. The specimen figured was from an unknown locality, and may possibly represent a variety of this species rather than of the *E. emarginata* of Lutken, A. Agassiz, etc., which is common on the Atlantic side of tropical America.

Astropyga venusta Verrill, op. cit., p. 296.

Two fine large specimens of this rare species are in the collection.

The largest is 5·80 inches broad and 2·10 high. It was previously known to me only from Panama Bay and San Salvador.

Tripneustes depressus A. Ag.; Verrill, op. cit., p. 375.

Of this large species there were 19 specimens, with their spines partially preserved. They are quite variable in form, but mostly even more elevated than ordinary specimens of *T. ventricosus*. Some are conical, others broadly rounded above. The name, therefore, is a decided misnomer. The largest spines on the upper surface of the largest specimen are ·45 of an inch long, ·04 in diameter, and rapidly taper to the acute point; those of the lower surface are often ·60 of an inch long, ·04 in diameter, tapering but little, the end blunt.

Several specimens give the following proportions:

Diameter, (inches)	**5·80**	**5·40**	**5·35**	**5·25**	**5·10**	**4·90**	**4·75**	**4·60**
Height,	**3·00**	**3·40**	**2·90**	**3·25**	**2·85**	**2·65**	**2·85**	**2·80**

ASTROIDEA.

Pentaceros occidentalis Verrill.

Oreaster occidentalis **Verrill, op. cit., p. 278, 1866.**

Of this hitherto rare species there are 21 specimens of various sizes. They show but little variation except that due to age or state of preservation. Some specimens are so dried as to leave the disk and rays plump and rounded above, while in others the interradial spaces are so shrunken as to make both the rays and disk angular. In some most of the upper and part of the lower marginal plates bear small obtuse spines or tubercles; in others there are few or none of these; the two smallest specimens have none, though others scarcely larger, have quite a number. The smallest specimen has the longer radius 1 inch; the shorter ·50. This, however, has nearly the form and all the essential characters of the adult, though the spines and tubercles are less numerous.

The name of the genus, *Pentaceros* Gray, has priority over *Oreaster* M. & Tr., which was substituted on the ground that the former was in use among fishes,* which I have been able to confirm, although Mr. A. Agassiz adopts it.

Among the other starfishes are *Gymnasteria spinosa* Gray (large); *Amphiaster insignis* Verrill, *Nidorellia armata* Gray, *Ophidiaster pyrimidatus* Gray, *Linckia bifascialis* Gray, *Acanthaster Ellisii* Verrill, *Mithrodia Bradleyi* Verrill, etc.

CORALS.

Fungia elegans Verrill, sp. nov.

Coral when young regular and round, often becoming slightly oval; when adult usually more or less angular, the edge plicated, forming six to twelve lobes. The upper surface becomes very convex in mature specimens and the lower surface deeply concave and covered with very numerous, fine, subequal, elevated costæ, which are finely dentate on the outer half, becoming nearly entire and very faint toward the center, which usually shows the scar where attached when young. Septa thick and rather crowded, very unequal, the six primaries very prominent and thick at the inner end; those of succeeding cycles successively shorter and less elevated. Edges of septa unevenly crenulate, or finely dentate. Columella slightly developed, loosely spongy; median fosette small, narrow, elongated, the two septa in the direction of its longer diameter much less elevated and thinner than the rest. Trabiculæ stout, conspicuous, often coalescing into continuous transverse plates.

The smallest unattached specimens are ·90 of an inch broad by ·35 high; ordinary specimens are 1·90 broad by 1·10 high; the largest 2·35 by 1·20.

This is a small and very distinct species, remarkable as the first one of the genus described from America.

Among the other corals are *Pocillipora capitata* V., *P. capitata, var. porosa* V., *Cœnopsammia tenuilamellosa* E. & H., *Porites* (two species), *Stephanaria stellata* V., *Callipodium Pacificum* V., *Eugorgia aurantiaca* V., *E. nobilis, var. excelsa* V., *Leptogorgia rigida* V., *L. Agassizii* V., *L. media* V., *Muricea austera* V., *M. appressa* V., etc. Most of these are represented by numerous large and beautiful specimens.

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* Cuvier and Val., vol. iii, p. 30, 1828; Gunther, Catal. Fishes British Museum, i, p. 212.

ART. XIV.—*Note on the Generic relations and Synonymy of the Common Sea-urchin of New England* (*Euryechinus Drobachiensis* Verrill); by A. E. VERRILL.

THIS common sea-urchin, which is found upon the northern coasts of Europe, and both upon the Atlantic and Pacific coasts of northern America, was referred to the genus *Echinus* by all writers up to 1846, but under various specific names: *E. Dröbachiensis* Müll., *E. neglectus* Lam., *E. granularis* Say, *E. chlorocentrotus* Brandt, etc. Prof. Agassiz, in the introduction to his monograph of the Scutellidæ, p. 7, 1841, proposed several new subdivisions of *Echinus*, merely giving generic names and citing "les types." Among these was *Toxopneustes* with "*E. pileolus*" as its type. But afterward, in the introduction to Valentin's Anatomy of the genus *Echinus*, Dec., 1841, he revised the genus then known as *Echinus*, and made many judicious and necessary changes. At that time he separated and characterized the genera, *Temnopleurus*, *Tripneustes*, *Amblypneustes*, *Holopneustes*, *Stomopneustes*, and *Toxopneustes*.* For the last he here takes for type, the *Echinus tuberculatus*. The genus *Echinus* he restricted (p. ii) so as to include only "the species near *Echinus esculentus*, and which have, like it, the ambulacra formed of several arched series of pores disposed in pairs." To this group he referred all the European species, including not only *E. Melo*, *E. esculentus*, *E. Flemingii*, *E. granularis*, *E. brevispinosus*, *E. æquituberculatus*, *E. miliaris*, etc., but also *E. neglectus* (*Dröbachiensis*) *and E. lividus*, which now constitute my genus *Euryechinus*. It is plain, therefore, that at that time the genus *Toxopneustes* had nothing to do with such species as I have called *Euryechinus*. Later, in the Catalogue Raisonné, 1846, *Toxopneustes* (sub-genus) was extended so as to include, in addition to the typical species, several previously placed by Agassiz in *Echinus*. Still later Desor removed part of the species thus erroneously added, under the name of *Sphærechinus*, but he still left in it *Dröbachiensis* and *lividus*. Therefore as these were evidently generically distinct from the typical *Toxopneustes* and had received no generic name, I formerly proposed that of *Eurychinus*,† and gave my reasons in detail.

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* The following is the description: Le genre *Toxopneustes* a des ambulacres formés de séries arquées de doubles pores, convergeant vers le milieu des aires et séparées par des rangées parallèles de petits pores. Chaque série arquée se compose de six à neuf paires de pores. Vers la bouche il y en a moins; mais elles sont plus rapprochées. Les tubercules des séries principales sont assez grands; l'ouverture inférieure du test offre dix échancrures peu profondes. Je prends pour type de ce genre l'*Echinus tuberculatus*; j'en connais quelques espèces inédites."

† Proc. Boston Society Nat. Hist., x, p. 341, 1866, and Transact. Connecticut Acad., i., p. 394, 1867.

To any unprejudiced person it will be evident from this statement that, as left by Desor after his latest restrictions, *Toxopneustes* included two generic groups, one of which was the original and typical *Toxopneustes* of 1841, the other an unnamed genus, (*Euryechinus*). The fact that Mr. A. Agassiz, apparently overlooking the true state of the case, renamed in 1862 the original *Toxopneustes* section, calling it *Toxocidaris*, cannot affect the matter in the least, since in this instance there can be no doubt as to which group was originally designated by the name, *Toxopneustes*. Yet Mr. Agassiz in a recent paper* goes considerably out of his way to bring up this matter again, and inserts the following note under *Echinometra Michelini*:

> "I cannot see the propriety of the changes made by Verrill in the limitation of Toxopneustes, by substituting Euryechinus for a group of Echini, which are perfectly well known by all writers on Echinoderms as Toxopneustes. For the following reasons it seems to me, even granting all his premises, that the changes he proposes are not warranted. The type of the genus at the time the Monog. d. Echinides was written was never used in the restricted sense now common, but was coextensive with a group of species. When Toxopneustes was first proposed, it was applied to a so-called typical species which future investigations showed did not belong to the genus. The author took the earliest opportunity possible to point out his mistake by substituting for it another type, and giving a description which *applies not only to Toxocidaris as Mr. Verrill would have it, but also to all the species since removed as Sphærechinus by Desor.* Desor, who had edited the Catalogue Raisonné, and probably knew accurately what group of Echini was defined as Toxopneustes, was the first, in his Synopsis, to limit Toxopnuestes by removing from it certain species as Sphærechinus, and restrict Toxopneustes to such forms as (*T. neglectus*) *T. drobachiensis* Ag., but still including the species which I have since, in the Bulletin of the Museum, separated as Toxocidaris. All these limitations, even were they not accepted, have the priority over a similar limitation which Verrill makes twelve years after a proper limitation of the genus has been recognized, and eighteen years after a mistake (upon which Mr. Verrill bases the whole of his proposed changes) has been corrected by the author himself; nothing, moreover, is gained in accuracy by the change proposed by Verrill, *T. tuberculatus* being probably only a nominal species, and one concerning which we have, at any rate, no authentic information sufficient to form the basis of a sweeping reform. At the present rate of retrospective application of the laws of priority, we are fast drifting into the most absurd anachronism by applying the present condition of our knowledge of any group to works written twenty or thirty years ago in an entirely different spirit, when the idea of type, genera, etc., had a totally distinct signification from what it has at the present day."

A formal reply to this is perhaps unnecessary, yet I would venture the following remarks:

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* **Bulletin of the Museum of Comp. Zoölogy, No. 9, p. 260, November, 1869.**

1. I am not aware that any other zoölogist has denied the validity of "type-species" (especially when particularly designated as such) in works even much earlier than 1841.

2. Even if the original description of *Toxopneustes* would include *Sphærechinus*, yet, as I have explained above, Agassiz himself referred the species of the latter to *Echinus* in the same article, *and he probably knew accurately what group was defined as Toxopneustes at that time.*

3. All the "limitations" by Desor, which are said to have priority over mine, I have always admitted and adopted.

4. I do not base any changes upon "a mistake," for whether "*E. pileolus*" or "*E. tuberculatus*" be taken as the type of *Toxopneustes*, the claims of *Euryechinus* remain unaffected, and I have, when making the change, distinctly stated my conviction that the reference to *E. pileolus* should not be regarded, and have adopted *Toxopneustes* in place of *Toxocidaris* A. Ag. (See Trans. Conn. Acad.).

5. Whether *E. tuberculatus* be "a nominal species" does not affect the character of the genus to which it belongs.

6. I know of no more fruitful source of confusion than the transferring of a name from the group to which it originally belonged to another totally distinct from it, and re-naming the first group.

7. Had Mr. Agassiz, before naming *Toxocidaris*, looked a little more closely into the early synonymy of *Toxopneustes*, all confusion in this case might have been avoided.

8. The fact that there is a *future* for zoölogical nomenclature, as well as a *past*, should not be forgotten, nor that a just and reasonable application of the universally recognized law of priority is the surest way of securing future stability.

ART. XV.—*On the Early Stages of Brachiopods;* by E. S. MORSE.*

THE writer made a visit to Eastport, Maine, early in the summer, for the purpose of discovering the early stages of a species of Brachiopod (*Terebratulina septentrionalis* Couth.) so abundant in those waters. As little has been known regarding the early stages of this class of animals the facts here presented will be of interest, as settling beyond a doubt their intimate relations with the Polyzoa. As the subject will be fully presented at the meeting of the American Association, only the more important features will be mentioned here. In a few individuals the ovaries were found partially filled with eggs,

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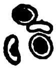
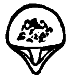
* From the American Naturalist, September, 1869.

The eggs (fig. 1) were kidney-shaped, and resembled the statoblasts of Fredericella. No intermediate stages were seen between the eggs and the form represented in fig. 2. This

1.



2.



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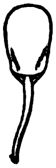

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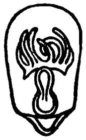

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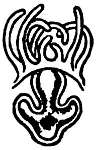

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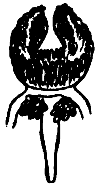

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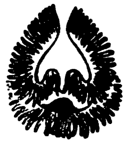


stage recalled in general proportions Megerlia or Argiope in being transversely oval, in having the hinge-margin wide and straight and in the large foramen. Between this stage and the next the shell elongates until we have a form remarkably like Lingula (fig. 3), having, like Lingula, a peduncle longer than the shell, by which it holds fast to the rock. It suggests also in its movements the nervously acting Pedicellina.

In this and the several succeeding stages, the mouth points directly backward (forward of authors), or away from the peduncular end (fig. 4), and is surrounded by a few ciliated cirri, which forcibly recall certain Polyzoa. The stomach and intestine form a simple chamber, alternating in their contractions and forcing the particles of food from one portion to the other. At this time also the brownish appearance of the walls of the stomach resembles the hepatic folds of the Polyzoa. Fig. 5 shows a more advanced stage, where a fold is seen on each side of the stomach; from this fold the complicated liver of the adult is developed, first, by a few diverticular appendages, as seen in fig. 6.

When the animal is about one-eighth of an inch in length the lophophore begins to assume the horse-shoe shaped form of Pectinatella and other high Polyzoa. The mouth at this stage (fig. 6) begins to turn toward the dorsal valve (ventral of authors), and as the central lobes of the lophophore begin to develop, the lateral arms are deflected, as in fig. 7. In these stages an epistome is very marked, and it was noticed that the end of the intestine was held to the mantle by attachment, as in the adult, reminding one of the *funiculus* in the *Phylactolæmata*. No traces of an anus were discovered, though many specimens were carefully examined under high powers for this purpose, the intestine of the adult being repeatedly ruptured under the compressor without showing any evidence of an anal aperture.

ART. XVI.—*On the existence of a Crocodile in Florida;* by JEFFRIES WYMAN, M.D.

IT has been shown by different paleontologists, especially by Dr. Leidy and Prof. Cope, that several species of Crocodilians existed in North America during the Cretaceous and Miocene periods, all of which became extinct. At the present time two living species of true Crocodiles, viz: *C. acutus* and *C. rhombifer*, are known in South America, and both range as far north as Cuba and San Domingo, but we have not been able to find a record of the presence of either of them within the limits of the United States, the Alligator being the only representative of the family to which it belongs.

While a guest of J. M. Forbes, Esq., on board the yacht Azalea, I had an opportunity of visiting Key Biscayne Bay in March, 1869, and while there Mr. William H. Hunt, of Miami, presented me the cranium here described. The animal to which it belonged, as I was told by the person who killed it, was shot near the mouth of the Miami river, which opens into the above mentioned bay. I was also informed that another had been killed in the same neighborhood.

The length of the head (from the alveoli of the incisors to the end of the occipital condyle) is 462^{mm}, and the greatest breadth 191^{mm}. The whole number of teeth is 68, viz: $\frac{19}{15}-\frac{19}{15}$; in the upper jaw the 4th and 10th, and in the lower the 4th and 11th are the longest. The first 12 teeth above and the first 11 below have the enamel fluted, and in both jaws the teeth behind these are marked with finely reticulated grooves. These hinder teeth are shorter and blunter, and thus more closely resemble the teeth of the alligator. The upper jaw is strongly convex and protuberant along the back of its middle portion.

When compared with a somewhat larger head of the *C. acutus* from South America (length 462^{mm}, breadth 211^{mm}), the Florida specimen closely agrees with it in the above as well as some other particulars. The following table shows some differences, however, relating chiefly to the proportion of the breadth to the length. In the table the length is assumed to be 1·000, and the measurements are in fractions of the length.

	South America.	Florida.
Greatest breadth of cranium	0·456	0·483
Length of palatine opening	0·214	0·210
Breadth " "	0·075	0·086
Breadth across pterygoids	0·300	0·313
Breadth at 4th tooth	0·162	0·169
Breadth at 10th tooth	0·218	0·235
Breadth at contraction behind 5th tooth	0·123	0·131
Breadth at contraction behind 12th tooth	0·190	0·210

In the South American specimen the markings of the surface of the bone consist chiefly of pits, while in the Florida specimen the markings are more in the form of grooves. In the first the parietal foramina are separated by a flat surface of bone 15 millimeters wide, while in the second there is only a blunt ridge. In this last also the lower jaw is somewhat longer, and its hinder part less recurved. The Florida specimen being the smallest is assumed to be the youngest; the differences of proportion above referred to may therefore depend upon age, as in young individuals the breadth is relatively greatest. In view of this we find no sufficient reason for considering the Florida specimen as belonging to any other than the sharp-nosed species (*C. acutus*). This conclusion is still further supported by a portion of the skin of the neck, which is preserved, and in which the nuchal plates are the same as in the species just named.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the emission and absorption of heat radiated at low temperatures.*—MAGNUS has communicated to the Royal Academy of Sciences in Berlin an important memoir on radiant heat, the principal results of which are, in the author's own words, as follows:

(1.) Different substances heated to 150° C. radiate different kinds of heat.

(2.) There are bodies which radiate only one kind of heat and others which radiate many kinds.

(3.) Perfectly pure rock salt belongs to the first class. Just as its ignited vapor or that of one of its constituents, sodium, sends out only one color, so this, even at 150° C., radiates only one kind of heat. It is monothermic, as its vapor is monochromatic.

(4.) Rock salt absorbs the heat radiated by rock salt in great quantity and more strongly than that of sylvin (KCl) and other kinds of heat. It therefore does not, as Melloni and Knoblauch suppose, permit all kinds of heat to pass through with equal facility.

(5.) The absorption of heat by rock salt increases with the thickness of the absorbing plate.

(6.) The great diathermancy of rock salt does not depend upon a small absorbing power for different kinds of heat, but upon the fact that it only radiates a single kind of heat, and consequently also absorbs only this, and that almost all other bodies at the temperature of 150° C. send out heat which contains only a small portion or none of the rays which are radiated by rock salt.

(7.) Sylvin behaves like rock salt but is not monothermic in an equal degree. In this also we trace the analogy with its own ignited vapors or with those of potassium, which, as is well known, gives an almost continuous spectrum.

(8.) Fluorspar almost completely absorbs pure rock salt heat. We ought therefore to expect that the heat which it radiates should be strongly absorbed by rock salt. Nevertheless 70 per cent of it pass through a rock salt plate of 20^{mm} thickness. With reference to the sum of the heat which fluorspar radiates, which is more than three times greater than that from rock salt, this phenomenon may be explained, but the subject requires further investigation.

(9.) If it were possible to form a spectrum of the heat radiated at 150° C., this spectrum, if rock salt were the radiating body, would contain only one band. If sylvin were used for radiating, the spectrum would be more extended, but would still occupy only a small portion of the spectrum which would be produced by the heat radiated from lamp black.—*Pogg. Ann.*, cxxxviii, 333.

W. G.

2. *On the reflection of heat at the surface of fluorspar and other bodies.*—In a second paper, Magnus remarks that the newly discovered property of rock salt and other substances of sending out at 150° C. rays of but one or but few wave lengths makes it possible to institute experiments on the reflection of non-metallic surfaces The results of experiments have distinctly shown that such surfaces reflect different kinds of heat, or heat rays of different wave lengths, in very different degrees. A remarkable illustration is furnished by fluorspar. Of the heat radiated by different substances and received at an angle of 45°, quantities differing but little in amount are reflected by the following bodies:

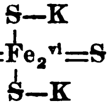
Silver,	from	83 to 90	per cent.
Glass,	"	6 to 14	"
Rocksalt,	"	5 to 12	"
Fluorspar,	"	6 to 10	"

But fluorspar reflects 28–30 per cent of the heat from rock salt, while silver, glass and rock salt reflect no greater proportion of this than of other kinds of heat. Fluorspar reflects from 15 to 17 per cent of the heat from sylvin, which confirms the result deduced from the transmission of heat. To an eye which could distinguish the different wave lengths of heat, as well as the colors of light, fluorspar would appear brighter than other substances if all were illuminated at the same time by rays from rock salt. The same would be the case if rays from sylvin were incident, only in this case the fluorspar would be less bright. As the experiments of Magnus established the fact that even at 150° C. different substances send out very different kinds of heat, it follows that in every space an extraordinary number of different wave lengths are continually crossing each other. This variety of intersection is further greatly increased by the elective reflection which takes place at the different surfaces. An eye which could distinguish different wave lengths of heat, like the colors of light, would see all objects in the greatest variety of colors without any sensible heating of the objects themselves.—*Pogg. Ann.*, cxxxviii, 174. W. G.

3. *On the heat of the stars.*—Mr. Huggins has endeavored to determine whether a measurable amount of heat is radiated from the fixed stars. The incident rays were received upon the object-glass of a telescope of 8 inches aperture, in the focus of which was placed the surface of a thermo-electric pile consisting, for stars, of one or two pairs, for the moon, of 24 pairs. An astatic galvanometer of great delicacy was employed. The thermo-electric pile was enclosed in casings of pasteboard stuffed with cotton so as to exclude all changes of temperature as much as possible. The author describes the minute precautions taken, for which we must refer to his paper. The telescope could be directed to a star by means of the finder, the needle being at rest, and the image of the star kept by clockwork upon the face of the pile. It was found that the needle almost always began to move as soon as the image fell upon the pile, and that when the telescope was then directed to the sky near the star the needle usually began, after a minute or two, to return to its original position. From 12 to 20 observations were made upon the same star, and these observations repeated on other nights. In this manner observations of Sirius gave a deviation of the needle of 2°; those of Pollux 1½°. Castor gave no deviation; Regulus a deviation of 3° and Arcturus in 15 minutes also 3°.

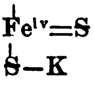
Observations of the full moon did not give corresponding or reliable results. The results obtained with the stars are not strictly comparable, as it is uncertain whether the sensitiveness of the galvanometer was always the same.—*Proceedings of the Royal Society*, No. 109, 1869. W. G.

4. *On new sulphur salts.*—Schneider has discovered and described a remarkable series of sulphur salts which are formed by simply fusing together certain metals in the form of powder with sulphur and potassic carbonate. Sulphid of iron and potassium is formed by fusing together 1 part of pulverized iron, 6 parts of dry potassic carbonate and 6 parts of sulphur, until the mass has become uniformly and quietly fused. Water diffuses from the cooled mass much potassic sulphite and hyposulphite and leaves the new compound in crystals. The new salt has the formula $K_2S . Fe_2S_3$ or atomistically,



$$\begin{array}{c} S-K \\ | \\ S=Fe_2^{vi}=S \\ | \\ S-K \end{array}$$

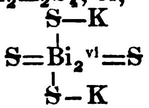
It forms long flexible needles of brilliant luster and purple brown color. At ordinary temperatures it is permanent. Acids even when very dilute decompose this body easily with evolution of hydric sulphid and separation of sulphur. Heated in a current of hydrogen it loses one atom of sulphur preserving its luster and crystalline structure. The new compound has the formula $K_2S . 2FeS$, or,



$$\begin{array}{l} S-K \\ | \\ Fe^{iv}=S \\ | \\ S-K \end{array}$$

Schneider considers it probable that in fusing ferric oxyd with potassic or sodic carbonate similar compounds, containing oxygen in place of sulphur, are formed.

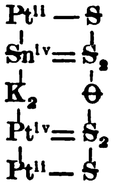
When bismuth is used in the above process in place of iron, delicate light steel gray brilliant crystalline needles are formed which have the formula $Bi_2K_2S_4$, or,



$$\begin{array}{c} S-K \\ | \\ S=Bi_2^{vi}=S \\ | \\ S-K \end{array}$$

This compound is easily and completely decomposed by chlorhydric acid with evolution of hydric sulphid. The author has obtained similar compounds containing copper, iron and copper, and platinum. They are beautifully crystalline and will form the subject of future more extended description.—*Pogg. Ann.*, B. 136, p. 460. W. G.

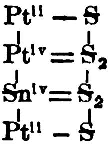
5. *On a new series of crystallized platinum compounds.*—By the action of stannous chlorid upon platinic chlorid, Schneider obtained a substance having probably the empirical formula, $Na_2Pt_2Sn_2O_6$, which though not yet thoroughly studied, forms the starting point from which the author obtained some very remarkable new compounds. When an intimate mixture of 4 parts of the above compound, 6 of potassic carbonate and 8 or 10 of sulphur are fused together, and the cold fused mass is treated with water, a cochineal red crystalline powder remains, easily obtained pure by washing. Under the microscope this compound is seen to consist of sharply defined six-sided tables of an almost metallic luster, which vary in color, according to their thickness, from light yellowish red to deep garnet red. The crystals are insoluble in water and unchanged by it. Dilute chlorhydric acid colors them at once dark greyish-black and takes up potash, but without evolution of any gas whatever. The residue is free from potash and is not attacked by boiling chlorhydric or nitric acid and but slowly by aqua regia. The original red body when heated in a current of hydrogen loses $\frac{2}{3}$ of its sulphur. The residue then gives up potash to chlorhydric acid, and the residue thus obtained loses all the remaining sulphur, when heated in hydrogen, and leaves a mixture of tin and platinum. From this it appears that the last $\frac{1}{3}$ of the sulphur is retained through the influence of the potash. The direct expression of the results of the analysis of the red compound is $Sn\,O\,\bar{K}\,Pt_3S_6$. The simplest atomistic formula appears to be the following:



$$\begin{array}{ccc} Pt^{ii} & - & S \\ | & & | \\ Sn^{iv} & = & S_2 \\ | & & | \\ K_2 & & \Theta \\ | & & | \\ Pt^{iv} & = & S_2 \\ | & & | \\ Pt^{ii} & - & S \end{array}$$

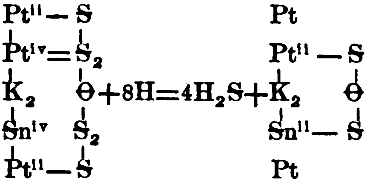
The action of chlorhydric acid on this salt is then readily explained, since, without writing the whole formula, we see at once that

$K_2\Theta+2HCl=H_2\Theta+2KCl$. The residue must therefore have the formula,



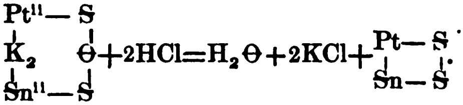
$$\begin{array}{ccc} Pt^{II} & - & S \\ | & & | \\ Pt^{IV} & = & S_2 \\ | & & | \\ Sn^{IV} & = & S_2 \\ | & & | \\ Pt^{II} & - & S \end{array}$$

The action of hydrogen upon the new compound may then be expressed by the equation:



$$\begin{array}{ccccccc} Pt^{II} & - & S & & Pt & & \\ | & & | & & Pt^{II} & - & S \\ Pt^{IV} & = & S_2 & & | & & | \\ | & & | & & & & \\ K_2 & & \Theta & +8H=4H_2S+ & K_2 & & \Theta \\ | & & | & & | & & | \\ Sn^{IV} & & S_2 & & Sn^{II} & - & S \\ | & & | & & & & \\ Pt^{II} & - & S & & Pt & & \end{array}$$

Hence the residue consists of metallic platinum mixed with a simple oxy-sulpho salt in which tin and platinum are now diatomic. The action of chlorhydric acid on the oxy-sulpho salt may be represented by the equation:



$$\begin{array}{cccc} Pt^{II} - S & & & \\ | \qquad | & & & \\ K_2 \qquad \Theta & +2HCl=H_2\Theta+2KCl+ & \begin{array}{c} Pt - S \\ | \quad\; | \\ Sn - S \end{array} & . \\ | \qquad | & & & \\ Sn^{II} - S & & & \end{array}$$

The last term when heated in hydrogen then gives hydric sulphid and a mixture of tin and platinum. The corresponding sodium salt may be obtained in the same manner and closely resembles the potassic compound.—*Pogg. Ann.*, cxxxvi, p. 105. W. G.

7. *Contributions to a knowledge of conjugate bodies in inorganic Chemistry.*—Under this title Blomstrand has communicated a number of interesting notices of what may be termed atomic aggregation. Platino-cyanid of potassium takes up two atoms of iodine with great facility, forming an iodo-platinocyanid, $K_2Cy_4Pt+I_2=K_2Cy_4PtI_2$, which crystallizes in large brilliant brown crystals. Chlorine and bromine displace the iodine, forming a corresponding chlorid and bromid, the former being the salt described by Knop. The corresponding salts of other positive metals are easily obtained. Iodine in like manner unites directly with auro-cyanid of potassium forming long brilliant brown needles, KCy_2AuI_2. Similar compounds of chlorine and bromine also exist. Double nitrite of platinous oxyd and platinum, $K_2Pt(N\Theta_2)_4$, also takes up chlorine and bromine readily, forming yellow crystalline salts which the author formulates as follows: $(K-\Theta-N\Theta=N\Theta-\Theta)_2PtCl_2$ and $(R-\Theta-N\Theta=N\Theta-\Theta)_2PtBr_2$.

Blomstrand regards the ammonia-nitrite of Lang $(NH_3)_2Pt.(N\Theta_2)_2$ as an intermediate term between the ammonia and the nitrous series and gives it the formula $(H_3N=N\Theta-\Theta)_2Pt$. This salt also unites with chlorine and bromine, forming very beautiful crystals. The bromid has the formula $(H_3N{=}N\Theta-\Theta)_2PtBr_2$ and is the bromid of a true base in which the bromine may be replaced by oxygen acids. The author terms the base plato-nitrosamin. The paper contains many interesting and instructive illustrations of the author's peculiar views of the qualitative influence produced on compounds by the substitution of electro-positive or electro-negative elements—views which are set forth with much clearness and force in his recently published work "Die Chemie der Jetztzeit."—*Berichte der Deutschen Chemischen Gesellschaft*, 1869, No. 9, p. 202. W. G.

II. MINERALOGY AND GEOLOGY.

1. *On the Surface Geology of the Basin of the Great Lakes, and the Valley of the Mississippi;* by DR. J. S. NEWBERRY, (Ann. Lyc. Nat. Hist. N. York, ix, 213, 1869).—In this important paper the author, after reviewing the general facts with regard to the drift, remarks as follows on the history of the era:

"1st.—That in a period probably synchronous with the glacial epoch of Europe,—at least corresponding to it in the sequence of events,—the northern half of the continent of North America had a climate comparable with that of Greenland; so cold, that wherever there was a copious precipitation of moisture from oceanic evaporation, that moisture was congealed and formed glaciers which flowed by various routes toward the sea.

"2nd.—That the courses of these ancient glaciers corresponded in a general way with the present channels of drainage. The direction of the glacial furrows proves that one of these ice rivers flowed from Lake Huron, along a channel now filled with drift, and known to be at least 150 feet deep, into Lake Erie, which was then not a lake, but an excavated valley into which the streams of northern Ohio flowed, 100 feet or more below the present lake level. Following the line of the major axis of Lake Erie to near its eastern extremity, here turning northeast, this glacier passed through some channel on the Canadian side, now filled up, into Lake Ontario, and thence found its way to the sea either by the St. Lawrence or by the Mohawk and Hudson. Another glacier occupied the bed of Lake Michigan, having an outlet southward through a channel—now concealed by the heavy beds of drift which occupy the surface about the south end of the lake—passing near Bloomington, Ill., and by some route yet unknown reaching the trough of the Mississippi, which was then much deeper than at present.

"3rd.—At this period the continent must have been several hundred feet higher than now, as is proved by the deeply excavated channels of the Columbia, Golden Gate, Mississippi, Hudson, &c., which could never have been cut by the streams that now occupy them, unless flowing with greater rapidity and at a lower level than they now do."

After further explanations he takes up the subject of the *Drift Deposits.*

"The Drift deposits which cover the glacial surface, consisting of fine clays below, sands and gravel above, large transported boulders on the surface, and the series of lake ridges (beaches) over all, form a sequence of phenomena of which the history is easily read.

"The lower series of blue or red clays—the 'Erie clays' of Sir William Logan—over a very large area, rest directly on the planed and polished rock surfaces. These clays are often accurately stratified, were apparently deposited in deep and generally quiet water, and mark a period when the glacial ice-masses, melted by a change of climate, retreated northward, leaving large bodies of cold, fresh water about their southern margins, in which the mud produced by their grinding action on the paleozoic rocks of the Lake District was first suspended and then deposited.

"On the shores of Lake Erie these clays contain no boulders, and very few pebbles, while farther north and west boulders are more abundant. This is precisely what might be expected from the known action of glacial masses on the surfaces over which they pass. Their legitimate work is to grind to powder the rock on which they rest;. an effect largely due to the sand which gathers under them, acting as emery on a lead wheel. The water flowing from beneath glaciers is always milky and turbid from this cause. Rocks and boulders are sometimes frozen into glaciers, and thus transported by them, but nearly all the boulders carried along by a glacier are such as have fallen from above; and a moraine can hardly be formed by a glacier except when there are cliffs and pinnacles along its course.

"In a nearly level country, composed of sedimentary rocks passed over by a glacier, we should have very little débris produced by it, except the mud flour which it grinds.

"The Erie clays would necessarily receive any gravel or stones which had been frozen into the ice, either as scattered pebbles or stones, distributed to some distance from the glacial mass by floating fragments of ice, or as masses of frozen gravel, or larger and more numerous boulders near the glacier. In some localities torrents would pour from the sides and from beneath the glacier, so that here coarse material would alone resist the rapid motion of the water, and the stratification of the sediments would be more or less confused."

The author next mentions the evidence of a general subsidence, greater to the north, and an ameliorated climate, as succeeding to the Glacial era; and then makes the following observations on the "*Yellow Sands and Surface Boulders.*"

"I have mentioned that on the Erie clays are beds of gravel, sand and clay, and over these again great numbers of transported boulders, often of large size and of northern and remote origin.

"These surface deposits have been frequently referred to as the direct and normal product of glacial action, the materials torn up and scraped off by the great ice ploughs in their long journeys from the North; in fact, as some sort of huge terminal and lateral moraines. I have, however, disproved, as I think, this theory of their transportation in a paper published some years since (Notes on the Surface Geology of the Basin of the Great Lakes. Proc. Bost. Nat. Hist. Soc., 1863), in which it is urged that the continuous sheet of the Erie clays upon which they rest, and which forms an unbroken belt between them and their place of origin, precludes the idea that they have been transported by any ice-current or rush of water moving over the glacial surface: as either of these must have torn up and scattered the soft clays below.

"There is, indeed, no other conclusion deducible from the facts than that these sands, gravels, granite and greenstone boulders—masses of native copper, &c., which compose the superficial Drift deposits—have been *floated* to their resting places, and that the floating agent has been ice, in the form of *icebergs;* in short, that these materials have been transported and scattered over the bottom and along the south shore of our ancient inland sea just as similar materials are now being scattered over the banks and shores of Newfoundland.

"If we restore in imagination this inland sea, which we have proved once filled the basin of the lakes, gradually displacing the retreating glaciers, we are inevitably led to a time in the history of this region when the southern shore of this sea was formed by the highlands of Ohio, &c., the northern shore a wall of ice resting on the hills of crystalline and trappean rocks, about Lake Superior and Lake Huron.

"From this ice-wall masses must from time to time have been detached,—just as they are now detached from the Humboldt Glacier,—and floated off southward with the current, bearing in their grasp sand, gravel and boulders—whatever composed the beach from which they sailed. Five hundred miles south they grounded upon the southern shore; the highlands of now Western New York, Pennsylvania and Ohio, or the shallows of the prairie region of Indiana, Illinois and Iowa; there melting away and depositing their entire loads—as I have sometimes seen them, a thousand or more boulders on a few acres, resting on the Erie clays and looking in the distance like flocks of sheep—or dropping here and there a stone and floating on east or west, till wholly dissipated.

"These boulders include representatives of nearly all the rocks of the Lake Superior country, conspicuous among which are granites with rose-colored orthoclase, gray gneiss, and diorites, all characteristic of the Laurentian series; hornblendic rocks, massive or schistose, and dark greenish or bluish siliceous slates, probably from the Huronian; dolorites and masses of native copper apparently from the Keweenaw Point copper region.

"In the Drift gravels I have found pebbles and small boulders of nearly all the paleozoic rocks of the lake basin, containing their characteristic fossils, viz: the Calciferous Sandrock with *Maclurea*, Trenton and Hudson with *Ambonychia radiata*, *Cyrtolites ornatus*, Medina with *Pleurotomaria litorea*, Corniferous with

Conocardium trigonale, *Atrypa reticularis*, *Favosites polymorpha*, Hamilton with *Spirifer mucronatus*, &c.

"The granite boulders are often of large size, sometimes six feet and more in diameter, and generally rounded.

"Along the southern margin of the Drift area, especially on the slopes of the highlands of Northern Ohio, the Drift sands and gravels are of considerable thickness, forming hills of 100 feet, or more in height, generally stratified, but often without any visible arrangement. These deposits are very unevenly distributed, with a rolling surface frequently forming local basins, which hold the little lakelets or sphagnous marshes so characteristic of the region referred to. These are the beds to which I have alluded as constituting, in the opinion of some geologists, a great glacial moraine, but from the fact that they are locally stratified, and overlie the older blue clays, I have regarded them as transported, not by glaciers but by icebergs.

"Possibly some part of this Drift material may have accumulated along the margin of the great glacier, moved by its agency; but in that case we should expect to find in it abundant fragments of the rocks which outcrop in the region under consideration, whereas I have rarely, if ever, seen in these Drift gravels any representatives of the rocks underlying the South margin of the lake basin.

"By whatever agency transported, the Drift gravels have, like the boulders, for the most part come from some remote point at the North, and were once spread broadcast along the southern shore of the inland iceberg-bearing sea.

"In the retreat of the shore line during the contraction of the water surface down to its present area, every part of the slope of the southern shore, between the present water surface and the highest lake level of former times, *i. e.*, all within a vertical height of 300 feet or more, must in turn have been submitted to the action of the shore waves, rain and rivers, by which if, as is probable, the retrograde movement of the water line was slow, these loose materials would be rolled, ground, sorted, sifted and shifted, so that comparatively little would be left in its original bedding; the fine materials, clay and sand, would be washed out and carried farther and still farther into the lake basin, and spread over the bottom, to form, in short, the upper sand layers of the Drift."

Dr. Newberry closes with remarks on the origin of the great lakes, in which he states his conclusion that:

"1st. Lake Superior lies in a synclinal trough, and its mode of formation therefore hardly admits of question, though its sides are deeply scored with ice-marks, and its form and area may have been somewhat modified by this agent.

"2d. Lake Huron, Lake Michigan, Lake Erie, and Lake Ontario, are excavated basins wrought out of once continuous sheets of sedimentary strata, by a mechanical agent, and that ice or water, or both."

2. *On the Nature and Cause of the Glacial Climate*; by **Joseph John Murphy**, Esq., F.G.S. (Q. J. Geol. Soc., xxv, 350, 1869).—In the present paper I purpose to show how far I agree with, and where I differ from, Mr. Croll as to the views on the cause of the glacial climate set forth in his paper in the 'Philosophical Magazine' for August, 1864.

Mr. Croll's conclusions may be stated in the three following propositions:—

1. A glacial period occurs when the eccentricity of the earth's orbit is at a maximum, and the solstices fall when the earth is in perihelio and in aphelio.
2. Only one hemisphere, the northern or the southern, has a glacial climate at the same time.
3. The glaciated hemisphere is that of which the *winter* occurs in aphelio.

I agree with Mr. Croll as to the first two propositions, but differ as to the third. I believe that the glaciated hemisphere is that of which the *summer* occurs in aphelio.

The following propositions are self-evident when stated:—

When the eccentricity of the earth's orbit is very considerable, and the winter of either hemisphere occurs in perihelio and its summer in aphelio, the nearness of the sun in winter will cause a mild winter, and his remoteness in summer will cause a cool summer.

Conversely, in the opposite hemisphere at the same time, the winter will occur in aphelio and the summer in perihelio: the remoteness of the sun in winter will cause a cold winter, and his nearness in summer will cause a hot summer.

Suppose, for instance, that when the eccentricity of the earth's orbit is much greater than at present, the midwinter of the Northern hemisphere occurs in perihelio; then

the *Northern* hemisphere will have a *mild winter* and *cool summer*,
the *Southern* hemisphere will have a *cold winter* and *hot summer*.

So far (granting Mr. Croll's astronomical data, for which he cites Leverrier, and which I believe are indisputable) there is no room for doubt. I have now to discuss the question, what effect these diversities of climate will have in producing glaciation.

Mr. Croll thinks the hemisphere of *cold winter* will be the glaciated one. I think, on the contrary, the hemisphere of *cool summer* will be the glaciated one.

On this subject it is needless to attempt to make any deduction from theory. We have plenty of observed data; and I think I can show that they all go to prove a cool summer to be what most promotes glaciation, while a cold winter has, usually, no effect on it whatever.

Forbes, in his work on Norway and its Glaciers, p. 206, quotes "the excellent generalization of von Buch, that it is the temperature of the summer months which determines the plane of perpetual snow." This indeed is almost an identical proposition; for perpetual snow is snow that lies through the heats of summer; and it would appear obvious enough, had it not been frequently overlooked, that it must be the temperature of summer which, other things being equal, determines the level of summer snow.

But, according to the same authority (Forbes's 'Norway and its Glaciers,' p. 206), "another cause affecting exceedingly the level of the snow-line is the amount of snow which falls."

These laws are illustrated in detail by the following table. In constructing it I have assumed, what is tolerably near the truth, that the temperature of the hottest month of the year decreases in ascending at the rate of 1° F. for every 300 feet. The temperatures are taken, as accurately as I have been able to do it, from Dove's map. My authorities for the heights of the snow-line are, for the first four, Durocher as quoted by Mr. Hopkins in the 'Proceedings of the Geological Society' for Dec. 17, 1851, for the rest, Mrs. Somerville's 'Physical Geography,' p. 314. The temperatures are in degrees of Fahrenheit. The heights are in feet.

	Temperature of hottest month at sea level.	Height of 32° F. in hottest month.	Height of snow-line.
Pyrenees	74·5	12750	9300
Caucasus	77	13500	10300
Mont Blanc	72·5	12150	9000
Bernese Alps	72·5	12150	8800
Scandinavian Fjelde, 61° 43′ N	59	8100	5500
Mageroe, Norway, extreme north	45·5	4050	2160
Himalaya, about 31° N., *north side*	83·75	15525	16620
The same, " *south side*	83·75	15525	12980
Andes, near Quito	79·25	14175	15795
" 18° N.	81·5	14850	14772
" near Valparaiso	68	10800	12780
" 37° 40′ S..................	63·5	9450	7960
Straits of Magellan	45·5	4050	3390

It is evident by this table that the snow-line rises above the line of 32° for the hottest month of the year where the snow-fall is small, and sinks below it where the snow-fall is great. In the Caucasus, the Alps, and the Pyrenees, the snow-line is about three-fourths of the height of the line of 32° for the hottest month of the year; in the Fjelde of Norway, about two-thirds; in the Peruvian and Chilian Andes above, but in Patagonia and Tierra del Fuego below; above, on the north side of the Himalaya, but below on the south side. These contrasts are all to be explained by the difference in the amount of snow-fall, which is greater on the south than on the north side of the Himalaya, greater in Patagonia and Tierra del Fuego than in Chilé and Peru, and probably greater, at least in winter, in Norway than in Central, Southern, or Eastern Europe.

The dependence of the height of the snow-line on summer temperature and on amount of snow-fall, to the exclusion of winter temperature, may be best shown, perhaps, by two extreme cases. The mean temperature of the Altai mountains (according to Mr. Hopkin's paper cited above) is below freezing; yet in consequence of the comparatively warm summer, and the small snow-fall, the height of the snow-line (Mrs. Somerville's 'Physical Geography,' p. 61) is about 6000 feet. On the Straits of Magellan, on the contrary, though the mean temperature is several degrees above freezing, the height of the snow-line (see table) is little more than half as much.

It is well known that, other things being equal, the magnitude of glaciers depends on that of the snow-fields in which they rise; and as of course any depression of the snow-line will enlarge the snow-field, it follows that the lower the snow-line the further will the glaciers descend below it. As a decrease of about 3° F. is due to every 1000 feet of ascent in the hottest month, it follows that a fall of temperature to that extent in the hottest month would lower the snow-line by about 1000 feet; and in many cases it is likely that the glaciers in such a case would descend 1000 feet further below the snow-line than at present, thus gaining a total increase of 2000 feet of descent. This might not have much effect on the climate of Central Europe, but it would have a very great effect in those high latitudes where the glaciers would reach the sea and give origin to icebergs; for we know that icebergs have great influence as transporters of cold.

In particular cases the effect of a comparatively slight fall of summer temperature would be very great. I quote from Forbes' 'Norway and its Glaciers,' p. 215:—

"Though the surface actually covered by perpetual snow in Norway be small, yet the mountainous districts and tablelands everywhere approach it so nearly that the snow-plane may be said to *hover* over the peninsula, and any cause which should lower it even a little would plunge a great part of the country under a mantle of frost."

And again, p. 243:—

"It is exceedingly probable that a diminution of the temperature of the summer months by 4° only would at once place *one-fourth* of the surface of Norway within the snow-line; and so vast a mass of snow would refrigerate the climate, especially the summer temperature, to such a degree as would unquestionably pour glaciers into the head of every fiord in western Norway. . . . The lowering of the snow-line over so large a surface would deteriorate the climate and lower the mean temperature, which would lower the snow-line still further."

The change in the eccentricity of the earth's orbit is in all probability amply sufficient to account for this or a much greater change in summer temperature.

I take the following data from Mr. Croll's paper. The recently ascertained error in the old determinations of the sun's distance affects both distances alike, and consequently does not affect their ratio. Along with the maximum distances of the sun at present and at greatest eccentricity, I state the proportionate quantities of heat the earth will receive under those two different conditions:—

	Sun's maximum distance.	Ratio of heat received.
At present	96,473,205 miles	100
At greatest eccentricity..	102,256,873 "	90

So that in the one case the earth receives about one-tenth less heat than in the other.

The sun's maximum distance occurs at present a little after the midsummer of the northern hemisphere. When it occurred at the same time of the year during the period of greatest eccentricity, the earth at our midsummer was receiving only nine-tenths of the quantity of heat which it now receives at that time of the year. I cannot calculate the effect on climate; but it must have been very great, not only directly, by depressing the snow-line, but as Forbes remarks in the place cited above, indirectly by chilling the air—and I will add, by filling the North Sea with the icebergs which must have broken off from the glaciers that filled the Norwegian fiords, as they do now from the glaciers of Greenland. We have plenty of evidence of iceberg action during the glacial period.

I believe I have shown that glaciation depends chiefly on a cold summer, but partly also on an abundant snow-fall. I have now to show that a period of cold summers, caused as I have explained, must be also one of snowy winters; so that the two conditions favorable to glaciation will occur together.

During the mild winter of the glaciated hemisphere, there is a hot summer in the opposite one. Increase of temperature promotes increase of evaporation in a much greater ratio than that of the increase of temperature; and increased evaporation in the summer hemisphere will produce increased snow-fall in the winter one. We know that at present the vapor raised in one hemisphere is to a great extent precipitated in the other; for, were it not so, the southern hemisphere, by reason of its greater extent of ocean surface, would have a rainier climate than the northern; and such does not appear to be the case on the whole. Besides, during a glacial period, the atmospheric circulation between the two hemispheres, at the time of the earth's minimum distance from the sun (which on my theory was in the winter of the glaciated hemisphere), must be more active than ever it is now; for when the earth, at either solstice, was nearer the sun than is ever the case now, and the difference of temperature between the two hemispheres consequently at its greatest possible amount, this would produce a very active circulation of atmospheric currents between the two hemispheres, which would involve the deposition as rain or snow in the winter hemisphere of a great part of the moisture evaporated in the summer one.

[The author continues with remarks on fiords as results of the Glacial era, a subject long since discussed in this Journal.]

3. *Geological Report of the exploration of the Yellowstone and Missouri Rivers;* by Dr. F. V. Hayden, Assistant under the direction of Captain (now Lieut. Col. and Brevet Brig. Gen.) W. F. Reynolds, Corps of Engineers, 1859–60, with a colored map. 174 pp. 8vo. 1869.—Several years have elapsed since the explorations for this report were made, and many of its general conclusions have appeared, through the author, in this Journal. The details here presented will be read with much interest. The

map is large and very instructive, showing, more accurately than had been done before, the distribution of the Tertiary and Cretaceous formations, and the girt of Carboniferous and Potsdam rocks around the high metamorphic ridges of the summit. Some further explorations of the mountains are required to make certain all the points in this distribution.

The last thirty pages of the volume are occupied by a report on the Cretaceous and Tertiary plants of the region, by Dr. J. S. Newberry, now Professor of Geology and Paleontology in the School of Mines, Columbia College, New York. Dr. Newberry's extensive knowledge of the fossil Botany of North America has enabled him to give his memoir great value. Dr. Hayden's absence in the mountains, during the past year, while his report was in press, accounts for its many typographical errors.

In the exploration of last year here referred to, Dr. Hayden was engaged in a new Government Survey under the direction of the Department of the Interior; and the first pages of the "Preliminary Field Report of the Survey" have been received by us. The Report promises to be one of great value to the science, and a notice of its results may be looked for in our next number. The Hon. T. D. Cox, Secretary of the Interior, states in his annual Report for the year 1869, that "Dr. Hayden entered on his labors in the field the last of June, at Cheyenne, Wyoming Territory. His route was through Denver, the silver and gold mining region of Georgetown and Central City, the Middle Park, Colorado City, and Fort Union to Santa Fé, returning through the San Luis Valley and South Park to Denver. The exploration, though brief and rapid, was eminently successful, and the collections in geology, mineralogy, botany, and zoology were extensive. His preliminary report bears date Oct. 15. It is accompanied by two other reports made to him by his assistants—one on mines and mining, the other on agriculture. These papers are a valuable contribution to our knowledge of the subjects which they embrace, and merit careful perusal."

4. *Mineralogy Illustrated;* by Dr. J. G. v. KURR, Prof. Roy. Polytechn. School at Stuttgart, etc. 22 large colored plates interleaved with explanatory text.—The figures in this Atlas represent in colors crystals or massive specimens of various minerals, especially the ornamental species, and is intended to aid the student in their identification. The figures are in general well colored; and although the text is not wholly free from its German idiom, the work may be of value in the study of the science.

5. *Tableau Mineralogique;* by M. ADAM, Commander de la Légion d'Honneur, etc. 102 pp. 4to. Paris, 1869.—A classified catalogue of the minerals in the splendid collection of Mr. Adam, of Paris, with a brief statement of the character and composition of the species.

III. BOTANY AND ZOOLOGY.

1. *Botanical Notabilia.*—Some announcements of recent publications with cursory remarks, and items of intelligence are here brought together in the expectation that they may be interesting or useful to botanical readers of the Journal. The only one of the publications here mentioned which can be said to possess much general interest is the first, viz:

An *Address of George Bentham, Esq., President of the Linnæan Society, London,* read at the Anniversary Meeting, May 24, 1869. It is the latest one of that series of pertinent annual discourses by which Mr. Bentham's presidency has been distinguished, and which it is hoped he will continue. A preceding one was reprinted in this Journal, and it would be well that this should be also. The topic is *Geographical Biology,* considered first for plants and then more succinctly for animals. Although distribution is one of the strongest points of derivative doctrine, yet it is wonderful to see, in the light of this sober and impartial survey, how entirely the whole aspect of philosophical natural history in this regard has changed within two decades. "Centres of creation," and the like are of the language of the past, here replaced by Bentham's happy term of "areas of preservation." And the conclusion, tardily reached "that the present geographical distribution of plants was in most instances a derivative one, altered from a very different former distribution," has been followed by the conviction that the present species themselves are equally derivative, and have a changeful history, some steps in which may be dimly surmised by the study of cognate forms, extant or fossil. At the point now reached, if not by general yet by large consent, the problems we are led to consider are such, that it is indispensable to have a term of wider application than "species" technically means; and Mr. Bentham here appropriates to this use the word *Race,* to denote either permanent variety (the old meaning of the word when definitely restricted), or species, or groups of two or more near and so-called representative species, i. e., for those collections of individuals or resembling groups of individuals whose association in the way of lineage is taken for granted by this class—or rather by these classes—of naturalists. As the term was only beginning to get fixity in its restricted sense, it will take the wider sense without confusion or difficulty, and with the advantage of a vernacular instead of a new-coined purely technical word.

The Miscellaneous Botanical Works of Robert Brown, which Mr. Bennett has edited for the Ray Society (2 vols. 8vo), are now completed by the Atlas of Plates, in small folio, 38 plates. The best thanks of Botanists, especially of the younger ones, are due to Mr. Bennett and to the Council of the Ray Society.

Another publication by the Ray Society for which botanists will be grateful is the *Vegetable Teratology, an Account of the principal Derivations from the usual construction of Plants,* by Dr. Masters. It was published last autumn, and forms an 8vo volume of nearly 600 pages, illustrated by over 200 wood-cuts.

The collection of cases is ample, the arrangement clear and scientific, and the general handling of morphological questions and theories of structure shows a full acquaintance with the subject. One cannot here well advert to points which invite discussion or further remark. Indeed a treatise of this character and upon such topics ought to be reviewed *in extenso*. It should be observed in passing that it may not be quite correct to speak of DeCandolle as "giving in his adhesion to the morphological hypothesis of Goethe (p. xxii, also p. 476): it is generally understood that when the *Théorie Elémentaire* and the *Organographie* were written DeCandolle knew nothing of Goethe's essay, and had never heard of Wolff.

A memoir of the Life of the late Prof. Wm. H. Harvey, principally made up of his own inimitable letters, edited by a surviving relative, has lately been published by Bell and Dalby, London. The book was at first intended for private distribution; but it was well decided, as his many friends and admirers in this country will be glad to know, that it should be issued through a publisher in the usual way. It makes an octavo volume of goodly size. Such a charming photograph of a lovely character will interest many to whom Dr. Harvey was personally unknown.

The *Genera of South African Plants* was one of the late Dr. Harvey's first undertakings when established, it was thought for some years, at the Cape of Good Hope. It was published at Cape Town, in 1838, and most useful it was in inspiring and developing the study of Botany in the Colony: nor is its usefulness superseded by the elaborate *Flora Capensis*, which was carried on with remarkable promptitude while Dr. Harvey lived, but which remains unfinished. He had prepared in a good degree the materials for a new edition of the *Genera*, which has now been edited by Dr. Hooker, the succinct introduction to Botany originally prepared by Mr. Bentham for his British Flora, and since added to all the Colonial Floras, being prefixed.

As to these Colonial Floras, that of Australia is pushed forward with Mr. Bentham's customary vigor, under the great advantage of his unsurpassed knowledge and his enviable opportunity of being able to devote his whole time without distraction to Systematic Botany. The fourth volume of the *Flora Australiensis* appeared almost a year ago: it contains most of the Monopetalous orders after the *Compositæ*, excepting *Verbenaceæ*, *Labiatæ*, &c., which are probably already in type for the fifth volume. Under *Breweria* Mr. Bentham justly remarks that this genus ought not to have been referred to *Bonamia* of Madagascar, as I had rashly done; yet he is wrong in the supposition that Thouars' plant not Convolvulaceous, but allied to *Ehretia* and *Cordia*. On inspection it proves to differ from *Breweria*, and to agree with *Cressa*, in the corolla, which not plaited in æstivation. There are probably two species.

The *Flora of Tropical Africa* by Prof. Oliver, "assisted by other botanists," though not exactly a Colonial Flora, is upon the same model. It is founded upon the African collections which have accumulated at Kew. Considerable as they are already, they represent the vegetation only of the outskirts of a vast *terra incognita*, of which we may hope to know something ere long, and which may give to this work as it advances an exceeding interest. This first volume, which was published in 1868, includes the orders antecedent to *Leguminosæ*. The *Malvaceæ* and their allies are by Dr. Masters, who has paid much attention to the structure of the flowers of this group; the *Ampelideæ*, *Sapindaceæ* and *Connaraceæ* are by Mr. Baker.

Dr. Oliver has just published a neat little *First Book of Indian Botany*, on the basis of his well-known elementary *Lessons* for home use, only the illustrations of the natural orders, the examples, &c., are from Indian plants, and considered in view of Indian Botany.

The ninth part of Dr. Seemann's *Flora Vitiensis*, nearly concluding the *Phanerogamia*, has been for some time issued; and the 10th, which will complete this laborious work, is in press. The Ferns are by Dr. Caruthers of the British Museum, so favorably known for his researches in fossil botany.

Mr. Baker, the Assistant Curater of the Kew Herbarium, after completing the *Synopsis Filicum* which was barely commenced by Sir Wm. Hooker, has made the results of that work the basis of a very interesting paper *On the Geographical Distribution of Ferns*, in the last (26th) volume of the Transactions of the Linnæan Society.

The publication of Sir Wm. Hooker's *Icones Plantarum* closed a dozen years ago with the 10th volume, completing a thousand plates. Dr. Hooker with the assistance of Mr. Bentham and Prof. Oliver, has now commenced a third series, of which two parts are issued, one in November, 1867, the other in June, 1868, and a third part is in press. The plates are numbered on from volume 10 (which is a convenience in citation) viz: 1001 to 1050. They are taken from plants in the Kew Herbarium, and will serve to illustrate some of the work going on in that richest of botanical collections, and notably, as it proceeds, the *Genera Plantarum*. Thus far nearly all are from the southern hemisphere. Two plants, however, are North American, viz: *Arceuthobium brachypodum* of Engelmann, from New Mexico, in which Prof. Oliver directs attention to some apparent peculiarity of the ovule, which needs investigation in the fresh plant; and *Leitneria Floridana* of Chapman, which Prof. Oliver describes as having a thin but evident layer of albumen around the embryo, and he agrees with Chapman in referring the genus to *Myricaceæ*, although with misgivings.

A whole volume of the *Journal of the Linnæan Society* (the twelfth, issued in advance of the eleventh), is occupied with an enumeration and description of all known Mosses of South America, by Mr. Mitten, founded primarily on Spruce's collections, which have been distributed into sets and disposed of. Mitten's *Musci Austro-Americani* thus forms a substantial 8vo volume of 659 pages. A double number of the eleventh volume is equally devoted to Mr. Spruce's South American collections and researches, viz., to the *Palmæ Amazonicæ*, an account by himself of the

Palms of the regions visited by him, a paper rich in generally interesting and readable as well as technical scientific matter.

Dr. Anderson has a paper on the Palms of Sikkin in the preceding number of the same volume; Dr. Kirk writes on the Copal of Zanzibar, and makes out that the old or fossil copal is the produce of the same tree, a species of *Trachylobium* from which recent Zanzibar copal is at present yielded. The other articles most noteworthy for us in the eleventh and the later numbers of the tenth volumes, are one by A. W. Bennett of London on *the Structure and Affinities of Parnassia*, upon which there is somewhat to be said whenever the present writer has opportunity to make reexaminations in the view of correcting certain probable mistakes or oversight. *On chemical reaction as a specific character in Lichens*, by Dr. Lauder Lindsay, who comes to the conclusion that these chemical tests introduced of late are of little value or reliability. *Notes on Jussiæa* by Charles Wright, a letter to Dr. Hooker, showing what excellent characters, hitherto nearly overlooked or in some cases misapprehended, are furnished by the seeds, &c., in this genus: and finally *Fungi Cubenses*, by Messrs. Berkeley and Curtis, founded on the very ample and largely novel collections in this order made by Mr. Charles Wright. It is to be stated here that these Fungi, accurately determined by the above distinguished mycologists, have now been distributed into sets, and placed on sale. Application for the few remaining sets may be made by letter to Mr. Wright, at the Harvard University Herbarium, Cambridge, Mass.

General Munro, who, when stationed in Canada, did much for American Agrostology, and now in England is able to command time for systematic work in his favorite order, has published in the Transactions of the Linnæan Society, a Monograph of *Bambusaceæ*. These, as now known, amount to upward of 170 species, belonging to 20 genera, our Cane of the Southern States being the solitary representative of the tribe in this country. The rare flowering which renders the study of the group difficult, is a common, though not universal feature. The common Bamboo of India is thought to blossom in any district only once in thirty or forty years, and when a general flowering occurs, the copious seed, giving sustenance to thousands, has often averted a famine.

For a French edition of his little work on the Fertilization of Orchids by insects, Mr. Darwin had occasion to prepare an appendix, enumerating the principal contributions to our knowledge of this subject, which have been made since this volume called attention to it and made the enquiry popular. There being as yet no call for a new edition in England, this appendix, of a few pages in extent, was turned into English and printed last summer in the Annals and Magazine of Natural History.

Dr. Baillon, now the Professor of Botany in the School of Medicine, Paris, commenced the publication of the "*Adansonia, Receuil Periodique d' Observations Botanique*" (8vo,) in 1860. and the 8th volume was completed in the summer of 1868. Each volume is illustrated by ten or twelve plates.

A large portion of the articles are by the able and indefatigable editor, the first being his essay on the female flower of *Coniferæ*, and against gymnospermy, which at the time attracted much attention. (Apropos to which, it is said that the doctrine of *gymnospermia* was first propounded by Targioni Tozetti. I have not seen the passage in which it is said to be recorded.) A second article describes, earlier than elsewhere, the setiform bodies which stand in place of petals in *Asarum Europæum*; but states that they are wanting or generally so in *A. Canadense;* they are, however, very commonly present. In the seventh and eighth volumes are brought together a very interesting collection of Trécul's various papers on the latex, in which he shows that this juice is contained in the spiral, dotted, and other ducts as well as in the so-called vessels of the latex.

Prof. Baillon commenced in 1868, a work on Organogeny, like that of Payer's, of which it would be a sort of supplement, but extending to the fruit as well as the flower. Only a single fasciculus has yet appeared of this *Traité du Developement de la Fleur et du Fruit*, (V. Masson et fils), with one plate, devoted to the flower of *Santalaceæ.* A more generally interesting work of the same author, and one which is pushed forward with vigor, is his *Histoire des Plantes* (imperial 8vo. Hachette & Cie, Paris), in a series of monographs of families, with wood-cut illustrations, the text with full readable details in French, with copious references, and then full generic characters in Latin. The three parts already published (1868–1869), comprise the *Ranunculaceæ*, the *Rosaceæ*, the *Connaraceæ* and the *Leguminosæ-Mimoseæ.* To give an idea of Baillon's valuation of genera, and how greatly he would reduce them, it may be noted that he refers back both *Eranthis* and *Coptis* to *Helleborus*, joins *Caltha* as well as *Calathodes* to *Trollius*, *Aconitum* to *Delphinium*, *Trautvetteria* to *Ranunculus*, and *Cimicifuga*, to *Actæa;* moreover, he refers the latter genus along with *Thalictrum* to the *Clematideæ.* *Crossosoma* he doubtfully places by the side of *Pæonia*, which appears to be the best that can be done with it, giving weight rather to the perigynous petals and stamens than to the arillus. He calls attention to his discovery, first recorded in the Adansonia, of three or four additional but minute and early abortive ovules in *Anemone* and *Clematis.* In *Rosaceæ*, beyond the reductions made by Bentham and Hooker, *Chamærhodos* also is united with *Potentilla*, and *Waldsteinia* as well as *Coluria* with *Geum;* Torrey's *Coleogyne* is well placed next to *Cercocarpus* and *Purshia;* but Baillon is much mistaken in supposing that he was the first to refer *Lutkea* or *Eriogynia* to *Spiræa*; it was so referred in the Flora of N. America more than a quarter of a century ago. In Torrey's *Emplectocladus* Baillon has rightly detected a *Prunus*, not far removed from Humboldt's *Amygdalus microphylla.*

It is rather late to notice Burean's *Monographie des Bignoniacees* (Baillière, 4to) of which the two parts issued bear the date of 1864, but the work is hardly known in the country and has not advanced far. The 200 or thereabouts pages of letter-press are given to a prolix account of the history of the classification of the order, its affinities, organography, &c. The thirty-one plates appear to be excellent. There is a figure of our *Bignonia capreolata* L., under the name of *Anisostichus capreolata*, of *Tecoma stans* as *Stenolobium stans* Seem., of *T. radicans* as a *Campsis* (in the letter-press), and of *Catalpa bignonioides*.

The other section of the 16th volume of *De Candolle's Prodromus* has just been issued. The two parts form indeed independent volumes, and are paged and indexed as such, so that for all time botanists will have to quote *DC.* Prodr. 16 (1), p., &c., which is to be regretted, but there is no help for it. The present (prior) part, of 450 pages besides 65 intercalated ones, contains the *Buxaceæ* and some other plants excluded from the *Euphorbiaceæ*, by Dr. Müller; the *Empetraceæ* by Alph. DeCandolle himself (*Empetrum* reduced to one species, *Corema* of two, and a *Ceratiola*); *Cannabineæ* by the same (the *Ulmaceæ* and *Artocarpeæ* postponed not being ready), the *Urticaceæ* (i. e. the *Urticeæ*) by Weddell; *Piperaceæ* by Casimir DeCandolle (the *Saurureæ* made a mere tribe, and the *Pipereæ* mainly included under Piper of 635 species, *Peperomia* of 389!); *Chloranthaceæ* by Solms-Laubach of Halle, reduced to three genera; and finally *Garryaceæ* by the editor, comprising nine species of *Garrya*. It appears that the latter end of the volume was printed first, which explains the omission of *G. buxifolia*, a species discovered in Northern California by Bolander, and published a year and a half ago. If one ort wo collaborators will now bring up their arrears, the editor may very soon have the great satisfaction of announcing the completion of the great Dicotyledonous series.

The incomparable *Flora Danica* goes on, and the 47th part, with plates 2701 to 2760. has come to hand. Among the plants of interest to us which it contains is *Pyrus* (*Sorbus*) *Americana*, our Mountain Ash, with the red petioles and pointed leaflets, from Greenland.

In the Journal of the Natural History Society of Copenhagen (Vidensk. Middel. Naturhist. Forening Kjobenhavn) for the year 1866, issued in 1867–8, Oersted has an elaborate paper on the classification of Oaks, with a catalogue of all the species, and many illustrations, both copper plates (of female flowers) and wood-cuts; of the latter, the cuts from impressions of the leaves, showing the venation, are peculiarly excellent. Being a later version of *Quercus* than that of DeCandolle, and with points of its own, it is commended to the attention of Dr. Engelmann, who is likely to have most to do with the American species. To illustrate venation and the nature of the surface of foliage, photography may be turned to good account, far more than is now commonly thought of. We have seen a photograph from a specimen of one of the coriaceous-leaved Oaks of the Dutch Indies which was truly wonderful in its rendering.

Dr. Lange, in the same publication for 1867 (issued in 1868), describes the *Pyroleæ* and *Monotropeæ* of Mexico and Central America. Four of the former group are identified with United

States species; and the fifth, *Pyrola Liebmanni* of Langé may be added, as it is apparently undistinguishable from one of our Western forms which already has several names, among them *P. bracteata*. Zuccarini's *Monotropa coccinea* is described in full and beautifully figured. The lively red color, in which it chiefly differs from *M. uniflora*, may not be of specific importance, considering that what we take for *M. Hypopitys* in the United States is often flesh-color or red in the warmer parts of the country.

A far more interesting Monotropeous plant has been collected by the French expedition to Mexico, viz: *Pleuricospora* Gray, (in Proceed. Am. Acad. 1867), with mature fruit, well showing the parietal placentation, and the close-coated seeds.

Prof. Bunge of Dorpat has brought out the first part of his Monograph of the *Astragali* of the Old World. It forms a fasciculus of 140 pages, in the current volume (11 of the 7th series) of the Memoirs of the St. Petersburg Academy of Sciences (1868), and contains the *Claves Diagnosticæ* of all the species, under their subgenera and sections. We have a goodly number of species in this country, mainly beyond the Mississippi; there are about 200 known in America,—a small matter compared with those of the Old World, of which Bunge here characterizes a little short of a thousand species! It is a subject of congratulation that, thanks to the author's great carefulness and knowledge there is hardly any double employment of specific names for *gerontogæan* and *neogæan* species. It would have been better to have avoided the use of names for sections in the same form as those of genera by adopting a plural termination or some like device.

A remarkable parasitic plant of the Sonora desert, indicated in 1854 in *Plantæ Thurberianæ*, but fully published and illustrated as much as ten years later by Dr. Torrey in the *Annals of the New York Lyceum*, his *Ammobroma Sonoræ*, has recently been investigated, along with its allies, by Count Solms-Laubach, a young botanist of high promise, now resident at Halle. It was thought that the incomplete specimen brought by Col. Gray and now belonging to Dr. Torrey was the only one extant. But it appears that much better ones were gathered by C. Schukard, draughtsman of the party, and sent along with a drawing, to Dr. Behr of San Francisco, who presented them to the late Prof. Schlechtendal of Halle. Solms-Laubach, a former pupil of Schlechtendal, found the specimens in his herbarium, also good ones of *Lennoa madreporoides*, and having likewise examined Kunth's *Corallophyllum cæruleum*, had thus before him all the known representatives of the group except Nuttall's *Pholisma*, which is known in Europe only by the specimen in the Hookerian Herbarium which served as the original of the figure in the *Icones Plantarum*, and probably by one in Nuttall's own herbarium now belonging to the British Museum. The results are just published in an elaborate memoir: "*Die Familie der Lennoaceen, von Hermann Grafen zu Solms-Laubach,*" a separate issue from the eleventh volume of the Transactions of the Natural History Society of Halle, bearing the date of 1870. The three plates illustrate in detail the three plants examined, viz: *Ammobroma Sonoræ* Torr., *Lennoa madreporoides* Llav. & Lex., and *L. cærulea* Solms, which is Kunth's *Corallophyllum cæruleum*. The order *Lennoaceæ* of Torrey is divided into two sections, 1st, with stamens in one rank and the anther-cells parallel for *Pholisma* and *Ammobroma*, each of a single species; 2nd with the alternate stamens inserted lower than the rest, and the anther-cells contiguous at the apex but diverging below, for *Lennoa*, of two species. Solms doubts, and with good reason, whether *Ammobroma* should not be referred to *Pholisma*, which would leave two very well defined genera. The difference in the ovule, whether horizontal or perpendicular becomes of small consequence when it is seen that the rhaphe in the former case is on the lower side, indicating a pendulous ovule. By several pertinent considerations Solms reinforces Dr. Torrey's opinion that the affinities of these plants are with the Erical alliance and not at all with the *Orobancheæ;* but the insertion of the stamens, even more than the structure of the ovary and fruit, forbids union with the *Monotropeæ.* That character belongs, however, to a great part of *Epacrideæ*, and Solms shows us that the structure of the ovary is the same as in some of the latter, and in certain *Vaccineæ.* The cells, as he shows, are half carpels, each carpel being biovulate and divided into two uniovulate *locelli* by intrusion from the back, as in *Gaylussacia*.

Professor Braun, continuing his study of the *Rhizocarpeæ* and the like, published last year a little monograph of the Australian species of *Isoetes*, with a preface containing general remarks on those of Europe and North America. He has also published more extensively upon *Characeæ;* and Dr. Leonhardi, the Professor of Philosophy in the University of Prague has discussed the European species in detail in the Proceedings of the Natural History Society of Brunn, and in the *Lotos.* We have from Dr. Hegelmaier a second dissertation on *Callitriche*, aided as to the American species by Dr. Engelmann, who placed the notes and specimens of his herbarium at his disposal. He recognizes *C. marginata* Torr., in Bolander's collection; *C. microcarpa*, a new species of Engelmann from Wright's Cuban collection; *C. Japonica* of the same, collected by Wright in Japan; and describes a new California species, *C. Bolanderi.* Hegelmaier has published his Monograph of *Lemnaceæ*, in 4to, but we have not yet seen it.

Prof. Miquel has carried on the *Annales* of the Leyden herbarium as far as to the fifth fascicle of the fourth volume; the papers in the current volume thus far relate almost wholly to the botany of the Indian Archipelago.

The seventh volume of Walpers' *Annales Bot. Systematicæ*, continued by Dr. C. Müller, gets on but slowly: the first and second fascicles bear the date of 1868, and we have only the third for 1869, which comes down only to the *Zygophylleæ.*

Dr. Pritzel's *Iconum Botanicarum Index Locupletissimus*, a perfect model for typography and arrangement as well as careful editing, was supplemented in 1866 by a *pars altera*, of nearly 300 pages, bringing down the references to plates to the end of the year 1865. The first edition of the main work has been reprinted verbatim, along with this supplement, instead of redigesting the whole, which would have been preferable. But it will temporarily do very well as it is, if the editor will soon re-edit the whole, incorporating into one alphabetical order the plates published down to 1870.

In the summer of 1868 appeared the 44th, 45th and 46th fascicles of Martius' *Flora Brasiliensis*, comprising the *Loranthaceæ*, a masterly work by Dr. Eichler; the *Oleaceæ* and *Jasmineæ* by the same (the character of *Chionantheæ* "semina exalbuminosa" still kept up, though it is not so either in *Chionanthus Virginica* itself or in the original *Linociera ligustrina;* see Proceed. Amer. Acad., 5, p. 331, etc.); the *Loganiaceæ* by Progel, and the *Styraceæ* by Seubert. Since the lamented death of Von Martius, Dr. Eichler has brought out the *Balanophoreæ* (in fasc. 47), with an admirable investigation and illustration of the intimate structure or anatomy as well as of the floral characters and morphology of this family, and has clearly shown it to have the ovulation of *Santalaceæ* and *Loranthaceæ* (some of them very exactly of the former), and to belong to that alliance.

The 48th part, the *Convolvulaceæ* by Meissner, has just been issued, but has not yet come to hand. Before his death, Von Martius had fortunately completed such arrangements with the Brazilian Government on the one hand and with Dr. Eichler on the other, as to secure the continuance and completion of this great work, that is, if it can be done within ten years; and Dr. Eichler is pushing it forward vigorously. He is placed at disadvantage, however, by the seemingly ill-advised refusal of the Bavarian Government to purchase and retain in Munich the library and herbarium of the *savant* who has done far more than any one else for natural science in Bavaria; and so the library of Martius is to be dispersed by auction next March; and the herbarium, which his will forbids the dispersion of, still awaits a purchaser. A pamphlet, *Das Herbarium Martii*, gives particulars of the contents of this collection. It comprises, 1, the general herbarium, estimated to contain about 60,000 species in 300,000 specimens, over half the species South American, especially Brazilian; 2, The Palm collection, which ought to be of very great importance; 3, A collection of fruits and seeds; 4, of woods; 5, a very rich and well-prepared collection of drugs and economical products, the larger part of which was formed by his brother Theodore Martius, when Professor of Pharmacy at Erlangen. Here is an unusual opportunity for some American University.

We have received a second edition (1864) of an *Atlas des Pflanzengeographie*, by L. Rudolph of Berlin, (published by Nicolas),* intended for schools, &c. It is well done, and if in English would answer an excellent purpose in our higher schools, or wherever Botany and Physical Geography are taught. It is strictly an Atlas, of nine folio plates or maps, charged with details, which are printed partly in colors, and with descriptive letter-press on the reverse. The centennial anniversary of the founder of Plant-geography should awake new interest, and give increased popularity to the subject.

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* Received from E. Steiger, German bookseller, Frankfort St. New York, along with a copy of Pritzel's Index (mentioned on the preceding page.)

The new President of the ancient Imperial Academy Naturæ Curiosorum, succeeding Carus, is Professor Behn of Hamburgh.

An interesting bit of botanical literature, is Pursh's *Journal of a Botanical Excursion in the Northeastern parts of Pennsylvania and New York, during the year* 1807. Dr. Thomas P. James found the manuscript journal of Pursh with some other papers accompanying the herbarium of Dr. B. S. Barton, in the possession of the American Philosophical Society, and had it printed in successive numbers of Meehan's *Gardener's Monthly*, from which it is now collected in the form of a little volume of eighty-seven pages. It will be remembered that this journal contains Pursh's notes made at the time of his discovery of *Scolopendrium*, and the station is indicated with sufficient precision to be determined, as was done, we believe, by Mr. Paine. If the Fern no longer exists at that very station, we know of it at three or four neighboring localities. Pursh's quaint and simple narrative of his wanderings and hardships is well worth perusal.

The Botanical Necrology for the year 1869, may happily be dismissed in few words.

Antonio Bertoloni, the most venerable botanist of our day, who has occupied the chair of botany in the ancient university of Bologna for considerably more than half a century, died, in the full possession of his mental powers, on the 17th of April, shortly after the completion of his ninety-fourth year. The very next day died the equally distinguished professor of the sister University of Turin, *Giuseppe Moris*, in the seventy-third year of his age. At Prague on the 28th of July, died the professor of Physiology, *J. E. Purkinje*, at the age of eighty-two. He was known in botany as the author of a neat little treatise, "*De cellulis antherarum fibrosis*," etc. A. G.

2. *Recent Explorations of the Deep-sea Faunæ;* by A. E. Verrill,—A new era in the history of marine zoölogy may be said to have commenced in 1860, when Dr. Wallich obtained a number of Worms, Crustacea, Bryozoa, and Echinoderms at depths varying from 445 to 1913 fathoms, and from these observations inferred that the deep-sea had its own peculiar fauna, at depths far greater than life had previously been supposed to exist, unless in the lowest forms, such as the Rhizopods obtained from soundings and described by Bailey and others. This inference was at once confirmed by the observation of A. Milne-Edwards, who in 1861 reported a number of living mollusca and corals, found adhering to the telegraph cable between Algiers and Sardinia, when taken up for repairs, on portions that had been sunk to depths of 1093 to 1577 fathoms. Some of these were new, others were known only as Tertiary fossils. In the same year the Swedish

expedition to Spitzbergen obtained Tunicates, a Zoöphyte, a Crustacean, and a bivalve-shell from 1400 fathoms. Later, G. O. Sars has made extensive explorations by means of the dredge at the Lofoden Islands, and on the Scandinavian coast in depths of 200 to 300 fathoms, and in some cases down to 450 fathoms. Prof. Sars reports,* as the result of his son's explorations, 427 species of animals living between 200 and 450 fathoms, and representing most of the marine classes: Rhizopods 68; Sponges 5; Polyps 20; Hydroids 2; Crinoids 2; Ophiurians 14; Asterians 7; Echinoids 5; Holothurians 8; Gephyrea 6; Annelids 51; Crustacea 106; Polyzoa 35; Tunicates 4; Brachiopods 4; Conchifers 37; Gasteropods 53. He also records 9 fishes living at those depths. Of the whole number 42 were found at 450 fathoms, and 46 were confined to the region below 200 fathoms, while most of the others extend upwards to quite shallow waters. Among the many remarkable new forms the *Rhizocrinus Lofotensis* Sars is especially notable, and has been the subject of an admirable memoir by Prof. Sars.† It is remarkable as giving us a new living type of attached Crinoidea, belonging to a family most characteristic of the Jurassic formations, and is of especial interest in this connection, as it has also been found since at several other very remote localities, even as far south as Florida, at similar depths. In 1867 the Superintendent of the U. S. Coast Survey undertook an extensive exploration of the Gulf Stream, which has already yielded most important results in deep-sea zoölogy. The dredging, under the charge of L. F. de Pourtales has been carried on during three past seasons, between Florida and Cuba, from the shallow waters of the shore to the deepest waters of the mid-channel, which seldom exceeds 550 fathoms, the dredgings not having gone beyond 700 fathoms. From these explorations large and most remarkable collections have been obtained, embracing numerous representatives of nearly all classes of marine animals. A part of these have been described in the Bulletin of the Museum of Comparative Zoölogy,‡ but the Crustacea, Worms, part of the corals, and most of the Mollusca are still undescribed. Enough has been done, however, to reveal a wonderfully rich fauna and make us look for still greater discoveries in the future.

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* Videnskabs Selskabs, Förhandlingar, 1867, pp. 246 to 275; and Annals and Magazine Nat. History, vol. 3, p. 423, June, 1869.

† See this Journal, xlviii, p. 142.

‡ No. 6.—*Contributions to the Fauna of the Gulf Stream at great depths.* By L. F. de Pourtales, Assistant U. S. Coast Survey, December, 1867. Contains descriptions of Worms, Polyzoa, Brachiopods, Halcyonoids, Corals, Hydroids, and Crinoids

No. 7.—Same title, Dec., 1868. Contains general account of dredgings, with descriptions of Brachiopods, Crinoids, Holothurians, Corals.

No. 9.—*Preliminary Report on the Echini and Star-fishes dredged in deep water between Cuba and the Florida Reef*, by L F. de Pourtales; *prepared by* Alexander Agassiz, October, 1869. Contains descriptions and tables of distribution of the Echini; descriptions of the young of Echini and their development; and a list of the star-fishes.

No. 10.—*Preliminary Report on the Ophiuridæ and Astrophytidæ dredged in deep water between Cuba and the Florida Reef*, by L. F. de Pourtales; *prepared by* Theodore Lyman, November, 1869. Contains: I, General remarks and tables of distribution; II, Descriptions of new genera and species, with Critical Remarks.

Another exploration,* which has yielded some of the most important results, was undertaken in 1868, by Dr. W. B. Carpenter and Dr. Wyville Thomson to explore the region between Scotland and the Faroe Islands. They were furnished with a Government vessel, fitted for the purpose, but owing to very bad weather could use the dredge only nine days during the cruise of six weeks. They obtained, however, quite a variety of animals at from 400 to 650 fathoms, including the *Rhizocrinus Lofotensis* and a remarkable variety of vitreous sponges, which seem to be very characteristic of the deep-sea fauna. But their observations upon the temperature at great depths are of the greatest importance. An extensive area was discovered between the Faroe and Orkney Islands, where the minimum temperature in 450 to 550 fathoms was from 32° to 33¾°. This region was characterized by an arctic assemblage of animals and a sandy and stony bottom. South and west from this "cold area," and extending far to the west of the Faroe Islands, was a warm area where the minimum temperature at 450 to 650 fathoms was 46° to 49°, the surface temperature being 52° to 54½°, while over the "cold area" the surface temperature was nearly the same, 50° to 52°. The warm area was characterized by a bottom of soft mud, composed chiefly of the shells of *Globigerinæ*, "*coccoliths*," "*coccospheres*" and other forms of Protozoa. Living upon this bottom were various vitreous sponges, allied to *Hyalonema*, and living like it with their long, slender siliceous spicules buried in the mud like rootlets, and several *Annelids*, *Rhizocrinus*, *Kophobelemnon*, *Terebratula*, *Lophohelia prolifera*, etc. The character of the bottom and its fauna strikingly recalled the chalk of the Cretaceous period, and the authors of these discoveries claim that *the* chalk formation has been continuous since the Cretaceous, and is *still forming* in the depths of the Atlantic. Another expedition was undertaken during the past season by the same parties, but we have seen no statement of the results, except that *Buccinum undatum* was dredged living in

No. 11.—*List of the Crinoids obtained on the coasts of Florida and Cuba, by the United States Coast Survey Gulf Stream Expeditions, in* 1867, 1868, 1869. By L. F. de Pourtales. Contains descriptions of eight species of Crinoids (six of *Antedon*), with a table showing their distribution.

No. 12.—*List of Holothuridæ from the Deep-sea dredgings of the U. S. Coast Survey.* By L. F. de Pourtales. Contains six species, three of which (*Echinocucumis typica*, *Cucumaria frondosa*, *Molpadia borealis*) are referred to north European species, and two others are regarded as possibly identical.

No. 13.—*Report upon Deep-sea Dredgings in the Gulf Stream, during the Third Cruise of the U. S. Steamer Bibb, addressed to* Professor Benjamin Peirce, Superintendent U. S. Coast Survey, by Louis Agassiz, November, 1869. Contains a general account of the work done and its results, together with an account of the rock formations now being deposited in that region, also observations on the young stages and mode of growth of several genera of corals, etc.

* Proceedings of the Royal Society, vol. xvii, p. 168, December, 1868; and Annals and Mag. Natural History, vol. 4, p. 112, August, 1869.

1,300 fathoms,* Dr. E. Perceval Wright also made an exploration 30 miles off Setuval, Portugal, in search of *Hyalonema*, which he dredged in 450 fathoms, and first ascertained its true mode of life.† From the same region he and Dr. Gray have described three species of Halcyonoid corals. By the same expedition a shark and a small fish were caught at the same depth. The recent Swedish expedition to Spitzbergen also made important discoveries, of which we have not yet seen full reports.‡ Dr. Smitt and Mr. Ljungman on the cruise of the Swedish frigate "Josephine," which visited the United States last summer, dredged some interesting animals on the newly discovered "Josephine Bank," between Lisbon and the Azores, in 117 fathoms; among them are *Ophiomyces frutectosus* Lyman, *Pteraster militaris*, *Rhizocrinus Lofotensis* Sars, and *Echinocucumis typica* Sars, all of which have been found also off Florida by Pourtales.§

These discoveries have very important bearings upon Geological science and Physical Geography, as well as Zoölogy, and will cause important changes in many generally accepted theories. The following are some of the results already made out:

1. It is certain that animal life does not begin to diminish sensibly at 100 fathoms and nearly disappear at 300 fathoms, as supposed by Forbes and generally believed previous to these late investigations. It is equally certain that the greatest depths yet reached with the dredge have an abundant and diversified fauna, with representatives of most classes of marine animals. The deep-sea animals are in part new and peculiar to great depths; in part found also in shallower waters; in part previously known as Tertiary or Cretaceous fossils.

2. It follows that abundance of fossils in a geological formation is not, of itself, evidence of shallow-water origin.

3. It is certain that *bright colored* animals are found at the greatest depths yet explored, and although uniform red and white are the most common colors among deep-sea animals, yet examples of nearly all the other colors have been observed, among Protozoa, Radiata, Mollusca, and Articulates,‖ as in fact we might have an-

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* American Naturalist, vol. 3, p. 383, Sept., 1869. Since this article has been put in type I have had the pleasure of reading the preliminary reports of this most important expedition, by Mr. J. Gwyn Jeffreys, in "Nature," pages 135 and 166, Dec., 1869. The explorations on the Atlantic coast of Ireland by Mr. Jeffreys, extended down to the depth of 1476 fathoms, revealing a diversified fauna of an arctic character at all depths. Prof. W. Thomson conducted the explorations in the northern part of the Bay of Biscay, where the dredgings reached 2435 fathoms, with excellent results. Dr. Carpenter took charge of the additional explorations north of Scotland. The results of this expedition are of great interest and the new discoveries very numerous. It added 117 species of mollusca to the 451 previously regarded as belonging to the British fauna. Of these 56 are new to science. The mollusca obtained in 2435 fathoms are *Pecten fenestratus* (Mediterranean), *Dacrydium vitreum* (Arctic), *Scrobicularia nitida* (Finmark to Sicily), *Neæra* (new), *Dentalium* (new).

† Annals and Magazine of Nat. Hist., Oct., 1868.

‡ The Swedish expedition of 1868 obtained in soundings a *Cuma* and a fragment of *Astarte* from 2600 fathoms.

§ Bulletin Mus. Comp. Zoöl., pp. 347 and 357.

‖ See the remarks of Prof. Sars on this subject in the work quoted above.

ticipated from the fact that many burrowing annelids, crustacea, and shells, which are rarely if ever exposed to the light, are brilliantly colored.

Therefore the presence of color-markings on fossil shells is not an evidence of their shallow-water origin, as believed by Forbes and others. The finely colored specimens of *Voluta Junonia*, dredged living by Pourtales in 350 fathoms, are a sufficient refutation of this theory.

4. Prof. Sars mentions several species of deep-sea Crustacea having perfectly developed eyes, showing that light penetrates to far greater depths than is commonly supposed.* But the decrease of sea-weeds and their almost complete disappearance at about 100 fathoms may perhaps be due to the diminution or modification of the light

5. The generally received theory that the temperature of the water at great depths is everywhere the same, in all latitudes, viz: that at which sea-water has its greatest density, formerly said by Herschel and others to be 39°, but more recently shown to be that of the freezing point of sea-water (25°·4 to 27°·4 Fahr.), is not true, at least for depths down to 700 fathoms, as shown by the observations of Thomson and Carpenter.

6. The same observers have satisfactorily shown that temperature is the main agent in determining the distribution of deep-sea animals, which had been previously shown to be the case among shallow-water forms, and that mere *depth* or *pressure* has little or no influence directly. But the chemical composition of the water (as the amount of oxygen) and the character of the bottom doubtless have their influence.

7. The last observation is connected with the discovery of two contiguous, but distinct, deep-sea faunæ, living side by side at similar depths (400 to 600 fathoms); one eminently arctic in character, occupying the "cold area" on a sandy and stony bottom; the other similiar to that of the region between Florida and Cuba, and occupying the "warm area," with a bottom of calcareous infusorial mud. The last fauna, or at least some of its species, extends over a region of vast extent, both in latitude and longitude, some of the species ranging from Florida to within the Arctic Circle, and on both sides of the Atlantic.

8. The discovery of species, genera, families, and even sub-orders, supposed to have become extinct, leads us to expect that other lost forms may still have living representatives in the vast unexplored regions of the ocean.† Among the remarkable discoveries, may be mentioned the *Rhizocrinus*; the *Haplophyllia paradoxa* Pourt., a living representative of the "Rugosa" group of corals, supposed

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* Other remarkable instances are mentioned by Jeffreys: as *Lacuna tenella* and a stalk-eyed crab from 808 fathoms; *Trochus minutissimus* Mighels and *Ampelisca* from 1230 fathoms; a stalk-eyed crustacean from 1476 fathoms; a large new *Fusus* from 1207 fathoms; a *Pleurotoma* from 2090 fathoms; and *Octopus cocco* from 632 fathoms, all of which are said to have well developed eyes.

† The Porcupine expedition obtained a number of species of shells previously known only from the Tertiary (Coralline Crag and Red Crag).

not to have lived since the Carboniferous period; various new forms of sea-urchins, described by A. Agassiz, having their nearest allies in the Cretaceous, etc. The discovery of the living Cystidean noticed in the last number of this Journal is another fact of the same kind, and so remarkable that it may not seem unreasonable to anticipate hereafter the discovery of living Ammonites and Trilobites.

9. These investigations have thrown great light upon the mode of deposition of certain geological formations, especially the chalk, and at the same time illustrate the manner in which, under the influence of currents of different temperatures, a chalk and a sandstone, with entirely distinct faunæ, may be deposited side by side at the same depth and, supposing the currents to be modified, how the two might alternate, thus accounting for the extinction of faunæ and the re-appearance of "colonies," as noticed by Barrande.

10. Dr. Carpenter and Thomson claim that the chalk formation now depositing in the deep Atlantic is a direct continuation of *the chalk* formation of the Cretaceous period, some of the living species being regarded as identical with Cretaceous fossils, while others, closely allied to fossil forms, are supposed to have been modified during the lapse of time by "natural selection," or in some other way.

3. *Catalogue of the Mammals of Massachusetts; with a Critical Revision of the Species;* by J. A. Allen. Bulletin of the Museum of Comp. Zoölogy, No. 8. Cambridge, October, 1869.—This includes not only a list of the species, with their principal synonyms, but also valuable information concerning their distribution, and their relations with the species of the old world. Mr. Allen admits much greater variations in the species of mammals than most modern writers, and shows that this variation is often as great in specimens from different parts of North America as between specimens from the two continents. Consequently he has regarded many of our mammals as identical with those of Europe and Asia. Among these are the black bear, common wolf, red fox, two weasels, mink, sable, beaver, moose, and reindeer. In some cases, however, he has probably gone altogether too far in uniting species; as in considering the black-bear, the grizzly-bear, the barren ground bear, and the European bear all one species; and in his treatment of the genus *Blarina.* He has also overlooked a specimen of *Neasorex palustris* in the Museum, from Norway, Me., described by me in the Proc. Boston Society of Natural History. v.

IV. ASTRONOMY.

1. *Report on the Recent Eclipse of the Sun;* issued under the direction of the Superintendent of the Naval Observatory.—This handsome volume of more than 200 pages has appeared first of any of the reports of the government expeditions, only detached portions of those of the Nautical Almanac and Coast Survey having yet been published. Commodore B. F. Sands,

Supt. of the Naval Observatory, took early measures to secure thorough observations, and assigned four of the five professors and two of the other astronomers at that institution, to the duty of observing the eclipse. In pursuance of the plans Prof. Hall and Mr. Rogers repaired to Plover Bay, on the Asian shore of Behring's straits, Professors Newcomb, Harkness and Eastman established themselves at Des Moines in Iowa, and Mr. Bardwell went to Bristol, Penn. The War Department lent its assistance by detailing Dr. Curtis of the Army Medical Museum, who has won deserved distinction by his success in micro-photography, and who accompanied the Des Moines party. The volume contains reports from each of these gentlemen, and to these are added others from Mr. J. H. Lane, of Washington, from Mr. W. S. Gilman, Jr., of N. Y., a lover of astronomy who has given special attention to solar observations, and from Gen. A. J. Myer, chief signal officer of the army, who witnessed the eclipse from the summit of a mountain in Southwestern Virginia,—these with the prefatory introduction of Commodore Sands composing the work now issued, which is copiously illustrated with cuts and engravings.

Prof. Newcomb's point of observation was near the Court-house in Des Moines, latitude $41^\circ\ 35'\ 4''$, on long. $1^h\ 6^m\ 17^s{\cdot}0$ west from Washington. Following out the ideas previously suggested by him in this Journal, he affixed circular screens of different diameters to a horizontal arm, projecting from the gable end of the Court-house, so as to occult the sun for his points of observation; the largest being intended to hide the corona from the field of his 4-inch comet-seeker. After a sharp determination of the first contact with a power of 40, in this instrument, and an aperture reduced to one inch, he carefully pointed an 8-inch telescope, roughly mounted for the occasion, and the comet-seeker, to the sun at known moments; thus fixing their positions for the purpose of mapping the places of any objects which the totality might disclose in either field of view, after which he designed moving the telescopes into other positions, and counting any objects which might there be visible. Then retiring into a dark place, he awaited the total phase.

The corona appeared less bright than he had anticipated, and careful scrutiny along the ecliptic showed not the faintest trace of any flush of light extending beyond the rest in that direction. Nothing was visible in either of the two telescopes, nor did a sweep near the ecliptic with the comet-seeker disclose any object whatsoever. This important question being thus answered, and the visibility of any inferior planet or group capable of accounting for the motion of the perihelion of Mercury being made highly improbable, Prof. Newcomb proceeded to the study of the corona, with and without the aid of the comet-seeker. The great protuberance in the S.W. quadrant seemed to him strongly pink, not uniform in structure, nor at all resembling a flame, but like a huge pile of cumulus clouds, illuminated by white and red light. He was able to trace the corona with his naked eye only to a distance equal to the moon's semidiameter and he saw no long rays of light, but a jagged and serrated edge. The sky at Des Moines was however quite hazy. But seen through a green glass the corona seemed composed of four or five prominent portions, smooth in outline, shading away imperceptibly, and in no case exceeding $5'$ in height. He regards the serrated appearance and the pointed rays as subjective phenomena. The moment of end of totality was noted with the naked eye, and the last contact with the comet-seeker, as before. Comparing his observations with the computed times, he finds the contacts to have occurred later than predicted by $12^{s}\cdot 5$ for the first, $10^{s}\cdot 4$ for the third, and $7^{s}\cdot 8$ for the last; whence he infers

Peirce's ☽ — Hansen's ⊙ is too great by $5''\cdot 3$,
whereas, Hansen's ☽ — LeVerrier's ⊙ is too great by $2\cdot 7$.

Prof. Harkness constructed a building for himself, Prof. Eastman and Dr. Curtis at another point in the city of Des Moines, and determined his position as latitude $41^\circ\ 35'\ 35''\cdot 9$, long. $1^{h}\ 6^{m}\ 16^{s}\cdot 05$ W. from Washington, by means of observations which are reproduced with great fullness of detail. At the beginning and end of the eclipse he was engaged with Dr. Curtis in taking photographs, upon which he had decided to rely for the times of first and last contact; but during the totality he devoted himself entirely to the spectroscope. With this he obtained, from a very bright part of the corona in the long beam to the southward, a continuous spectrum without absorption lines, but with a single bright line at 1497 of Kirchhoff's scale. From the great protuberance at 240° he obtained the lines C and F, that near D, and three others, two of which were *b* and the well-known third hydrogen line. The remaining one was that which he saw in the corona, and in all the protuberances without exception, and recorded as corresponding to K. 1497. It must we think have been K. 1474, which was recognized by so many other observers and which Lockyer and Young have always found in the chromosphere. The moderate dispersive power of Prof. Harkness's spectroscope would, as he states, render it difficult for him to discriminate with it between near lines.

Prof. Eastman's report contains the results of meteorological and photometric observations (which during the two days preceding the eclipse must have been far from encouraging), of observations of the contacts and of the corona. Of this and the appearances of the protuberances to him, he has given two colored engravings. In each the same outline serves for the corona; and indeed he expressly states that he "could not detect the least change in the color or position of the rays during totality." Beside these labors he assisted Prof. Harkness during the totality, by directing the spectroscope. Amid so many avocations it is surprising that Prof. Eastman could have accomplished so much, and that so well. We cannot avoid the conviction that had he been master of his own time during the total phase, he would not have failed to detect the decided fluctuations of form which the corona exhibited to more than one observer at Burlington. Like Prof. Newcomb, he found the color of the protuberances strongly red.

Dr. Curtis's report relates to the photographic operations which are described with very great minuteness. To those familiar with the exquisite photographs which he has made at the Army Med. Museum, it is needless to say that his preparations were evidently made with great forethought, and that abundant skill was displayed. A haze which veiled the sky, rendered a much larger exposure requisite than would otherwise have been given, but the photographs are of high excellence. The two taken during totality are given in engraved representations, impressions from the originals accompanying. These are very fine, rivaling those of the Burlington and Ottumwa parties. For the first, the exposure was 66 seconds; but details of the protuberances are beautifully shown, which would not have borne an exposure of one-third that length in a transparent atmosphere. Even the lunar motion seems scarcely to have interfered with the definiteness of the image.

It is to be regretted that a national enterprise carried out at the public expense should ever be made an agency for personal polemics, which indeed seem peculiarly out of place when aimed at any private citizen of the same nation, since he is theoretically one of those in whose behalf the investigation has been made. The greater part of one of the quarto pages is occupied by a note in fine print in which Dr. Gould is rather sharply assailed for his views as given on p. 435 of our last number. This is no place for any maintenance of those views, but two paragraphs deserve a word of comment.

> "Dr. Gould adduces, as an additional argument in favor of his assumption, the observation that the long coronal beams appeared to him to be "variable," while the "aureole" photographed was evidently "constant" during the time of totality. This argument however loses some of its force when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable both in form and position during the whole period of the total obscuration."

We are far from attributing to Dr. Curtis any intention of giving his language the discourteous significance which some might infer, otherwise no rejoinder could be possible. But we would suggest that special observations devoted to a particular point should *prima facie* outweigh general negative impressions. In the present case, if Dr. Gould's evidence be regarded as for any reason incompetent, there happens to be corroboration of his statement by other gentlemen whose attention he called to this point. And there is in addition the reasonable certainty that less than a twelvemonth will decide the question definitely, since attention will doubtless be specially directed to this point at the eclipse of Dec. next. Perhaps it might be the wisest, as it would certainly be the most considerate, course, to await such decision.

In the other passage Dr. Curtis says:—

> ". . . . the circumstance affords but another example . . . . of the necessity that a critic before attempting to draw scientific inferences from photographic representations should himself become something of a photographer. . . . . . And by a singular coincidence, evidence that Dr. Gould has not a practical acquaintance with the art would seem to be afforded in this same published letter by his total misinterpretation of another purely photographic effect, viz: the apparent encroach-

ment of the prominences upon the disk of the moon as seen in the photographs. This curious appearance, instead of being due to "specular reflection" is wholly a dark-room phenomenon, as will be explained in the text."

That Dr. Curtis's explanation is entirely adequate in most cases we readily concede. But when Ulloa in 1778 saw the brightness of the protuberances projected so far upon the lunar disk that he thought the sun was shining through a hole in the moon, there were no photographs. And had circumstances brought to Dr. Curtis's notice as many dozens of cases as have come to our knowledge, in which the great protuberance of last August was seen both by scientists and laymen, with and without optical aid, to appear as "a deep notch in the moon," we think he would have qualified his language. Dr. Gould has expressed his belief that this phenomenon is due to specular reflection from the lunar surface; others may with great reason attribute it to the influence of irradiation; but it cannot be solely a photographic effect, however such effects may coincide.

Dr. Curtis has also given the results of some interesting experiments made to determine the origin of the glow seen upon the sun, around the moon's limb, on the photographs; a glow which has been found real and not the result of contrast, in all the impressions taken last August. This he believes to be a result of diffraction, a view which we fully shared, as those present at the August session of the National Academy will remember. And by experiments involving diffractive action, he has produced artificial eclipse-photographs exhibiting the same appearance. But the recent experiments of Prof. Morton, of which Dr. Curtis must have been unaware, settled the question some time since by the production of other artificial eclipse-pictures by methods which exclude diffraction, yet manifest the crepuscule as clearly as in the original photographs. Such pictures have for a considerable time been in our possession, and Prof. Morton found the explanation of the phenomenon in a local redevelopment of the negative. This explanation seems fully to account for Dr. Curtis's results as well as for his own.

Mr. Lane's attention was given to teloscopic observation of the contacts and the corona; observations which were evidently carried out with his characteristic ability, but space is unfortunately wanting to us for a full description of his paper. Among other things, he measured the dimensions of two of the flocculent masses, or "cometoid bodies," in the protuberant aggregation at 110°; and he was able to trace the outline of the moon's eastern limb for $2^m\ 5^s$ after the end of the total obscuration.

Mr. Gilman's observations were made at Sioux City, Iowa, latitude $42°\ 47\frac{1}{2}'$, longitude $1^h\ 16^m\ 23^s$. His memoir is illustrated by numerous interesting diagrams, among which are sketches of spots and faculæ on August 5, 6, and 7. Some seconds previous to the occurrence of totality, the great protuberance and a large portion of the corona were seen by two of his companions. This protuberance (240°) was where Mr. Gilman had seen a bright double facula just before the first contact. Examined with a 4-inch telescope, it appeared of an orange-color, dotted with minute flakes of brilliant crimson. Another highly interesting fact is that four of the party saw an object which they confidently believed to be a star near the limits of the corona, in the neighborhood of the great protuberance. The position described was that of π *Cancri* [miswritten π *Leonis*, in *Astr. Nachr.*, lxxiv, 375] which was seen with one of the telescopes at Burlington. If this star was detected or seen by the unaided eye, no higher testimony could be brought to the keenness of the observer's vision; for its magnitude is but 5·8, being only three-tenths of a magnitude brighter than the faintest star visible to the average eye on moonless nights.

Mr. Bardwell, besides observing the times of contact, searched for inferior planets, and with the same negative results which others obtained.

Gen. Myer was at the summit of 'White Top Mountain,' near Abingdon, Va., 5,530 feet above the sea level. He saw the bright specks noted by so many as flitting across the moon's disk, and which some, erroneously, as we believe, have supposed to be meteors. With a deep-red shade-glass, "while some two digits of the sun were still uneclipsed," he noted a luminous prominence of yellowish color upon the moon's limb. Many of the details of the total phase were distinctly visible to the naked eye in this clear atmosphere, and when the last line of sunlight separated into beads, the guides exclaimed that the sun was breaking to pieces!

We have dwelt so long upon the observations within the territory of the United States, that no opportunity remains to give details regarding the observations of Prof. Hall at Behring's Straits (latitude 64° 20′, longitude 6^h 25^m). After their long voyage and hopeful endurance, they had the disappointment of seeing clouds drift over the sky, about an hour before the eclipse began. Within an hour after it had ended, the sky was cloudless again. The sun and moon were invisible from a short time before until a short time after totality, but the darkness was greatest between 9 17½ and 9 20 A. M.,—during which period there was less light than at the midnight preceding. The report is accompanied by copious meteorological and magnetic observations, which bear witness to the fullness of the preparations made, and testify that nothing within the control of Prof. Hall was wanting to render his results exceedingly valuable. Capt. S. R. Franklin of the United States Steamer Mohican, saw three protuberances upon the moon's disc, but no more. B. A. G.

2. *On the flight of a remarkable meteorite across the Western portion of Ohio near Forest*, lat. 40° 50′ and long. W., 84° 40′; by J. LAWRENCE SMITH, Louisville Ky.—A few minutes before three o'clock on the morning of Oct. 27th, the citizens of Forest, and for miles around, were suddenly aroused by a terrific sound in the upper regions like the report of a heavy siege gun, followed by two or three reports in quick succession, resembling the reports of field pieces fired by section, and ended in a peculiar and rather

metallic, rumbling or roaring sound, dying away in the distance. The interval between the first and successive reports was two or three seconds; the other intervals about one second. The firmest houses were shaken, windows rattled, and thousands of sleepers were roused in an instant, bewildered at the unusual and appaling noise; and many were the conjectures as to what it was; some thought it was an earthquake, but to the greater part it was simply an inexplicable phenomenon. Persons who happened to be awake at the time, were first startled to see the night suddenly lighted into day, and soon relapsed again into the usual darkness. While wondering what caused it, the stunning sound broke in upon them and greatly increased the mystery. The time between the going out of the light and the report is estimated by citizens here at from one-half to one minute.

Mr. Pierson of Patterson, a village about one mile, a little west of south of Forest, states that he saw the meteor coming directly toward him, from a direction about S. 35° W.; that it was a ball of fire apparently as large as a bucket, exceedingly bright and dazzling and had a luminous tail apparently thirty feet long and three feet wide; that it vanished or exploded, as he thought, directly overhead. At Finly, twenty miles northwest of Forest, the statement is, that there at about 3 o'clock on Wednesday morning, Oct. 28th, the inhabitants were aroused by a terrific explosion somewhere in the upper regions.

The night was one of clear moonlight, and exceedingly cold for the season. The night watchmen had witnessed it; and one says the he first saw it in the southeast, in size, seemingly, as large as a beer keg, and of intense brightness; that it descended, leaving a luminous streak behind, and that when near the earth, it exploded, with a terrific sound, and fierce brightness; that the light, after the explosion, took a southerly course, and disappeared. Another watchman reports that at the time of the explosion, it appeared as large as a load of hay, and that the sound of the explosion was stunning, not like a quick sharp report of thunder, but, as he termed it, more like the coming together of railroad cars, but much louder, and that the light was brighter than that of the sun. The direction of the meteor from Finley, as given by the watchman, with the bearing of the meteor's path, as described by Mr. Pierson of Patterson, and the fact, that to many the sound seemed nearly overhead, would indicate that it exploded or terminated its course in the vicinity of Forest; yet a careful investigation might prove its terminus to be many miles from that place. The sound seems to have been heard for perhaps fifty miles around, if not more. The stones or fragments that have fallen, may never be found, owing to the fact that the explosion was at night, and the consequent difficulty of determining its exact locality. In Kenton, Ohio, the phenomena are said to have occurred a few minutes before 3 o'clock and consequently they were not well observed; many persons saw the light but not the meteor, and all were sensible of the shock and sound. The meteor did not pass this place nearer than twenty miles, and the best judges give its duration at from two to three minutes from the flash to the explosion. The sound was of such force as to shake the houses and many believed it to be an earthquake.

These are all the statements I have been able to obtain in regard to the appearance of this meteor and its accompanying phenomena. It was beyond all doubt a meteorite, and I am using all possible means to discover any fragments that may have fallen. I must acknowledge the assistance afforded by Mr. Moore and Mr. Thomson, of Ohio.

3. *Elements of Asteroid* (109); by Wm. A. Rogers. (From a letter dated Alfred Observatory, Alfred, N. Y., Dec. 21st, 1869.)—I herewith communicate for the Journal the elements of (109) which I have computed from the following observations:

Date, W. M. T.				Place of observation.	α			δ		
	h.	m.	s.		°	′	″	°	′	″
Oct. 9,	13	26	32,	Hamilton Coll. Obs.,	14	00	45·9	+9	37	15·0.
Oct. 31,	8	44	33,	Hamilton Coll. Obs.,	9	11	27·8	9	54	47·8.
Oct. 31,	8	44	33,	Alfred Observatory,	9	11	32·4	9	54	48·0.
Nov. 28,	7	15	57,	Alfred Observatory,	8	7	49·2	11	9	3·17.

Epoch Oct. 9·0, W. M. T.

$$M = 339^\circ\ 16'\ 02''\!\cdot\!0$$

$$\text{Mean Eq. } 1869\cdot0 \left\{ \begin{array}{lrrr} \pi = & 55 & 22 & 43\cdot3 \\ \Omega = & 4 & 57 & 35\cdot2 \\ i = & 8 & 4 & 10\cdot6 \end{array} \right.$$

$$\begin{array}{lrrr} \varphi = & 17 & 16 & 40\cdot7 \end{array}$$

$$\mu = 804\cdot8304''$$

$$\log a = \cdot4295348$$

For computing an ephemeris, I find:

$$\log x = 9\cdot9999681 + \log\sin(v + 145^\circ\ 19'\ 47''\!\cdot\!4)$$
$$\log y = 9\cdot9307966 + \log\sin(v + 55\ \ 45\ \ 21\cdot1)$$
$$\log z = 9\cdot7181204 + \log\sin(v + 54\ \ 11\ \ 42\cdot2)$$

V. MISCELLANEOUS BIBLIOGRAPHY.

1. *Exercises in Practical Chemistry*, by A. G. Vernon Harcourt, M.A., F.R.S., Sec. C. S., and H. G. Madan, M.A., F.C.S. Series 1st, Qualitative Exercises. 335 pp. 12mo, Oxford, at the Clarendon Press, 1869. London, Macmillan & Co.—This little volume is designed as the beginner's *vade mecum* in commencing the study of practical chemistry in the laboratory. No attempt at a systematic presentation of the elements of the science is here made. The novitiate is presumed to be equally innocent of nomenclature, symbols and philosophy and is led into the laboratory much as an apprentice to a trade, and is therefore first made familiar with his tools, and how to use them in the most simple operations before even attempting the preparation of oxygen and other gases. It is illustrated by sixty-five wood cuts, mostly new and many of them very effective. The nomenclature is that of Williamson. Symbols

expressing the more important reactions are given at the foot of the pages where needed. We look with interest for the second series on quantative chemistry.

2. *On Aniline and its derivatives. A Treatise upon the manufacture of Aniline and Analine Colors*, by M. Reimann, P.D., L.A.M., to which is added, *The report on the coloring matters derived from coal tar, shown at the French Exhibition*, 1867; by Dr. A. W. Hofmann, F.R.S., MM. G. de Laire, and Ch. Girard. The whole revised and edited by William Crookes, F.R.S. &c. 8vo, pp. 164. (John Wiley and Son, Astor Place, New York, 1868.—Dr. Reimann's account of aniline and its derivatives is a fine example of the union of exact science, with practical skill, and as such teaches an important lesson beyond its special theme. It could not have had a more valuable supplement than in the admirable report of Dr. Hofmann and his associates, on the coal tar colors shown at the French Exhitition of 1867. The book, though published by Messrs. Wiley, was printed in London, by Mr. Crookes at the office of the Chemical News.

3. *A short Course in Qualitative Analysis, with the New Notation;* by J. M. Crafts, Prof. of General Chemistry in the Cornell University. 133 pp. 12mo, with five Tables. N. Y., 1869. (J. Wiley & Son.)—Prof. Crafts is well known to our readers from his frequent valued chemical contributions. He has, in the small volume before us, attempted the solution of a problem which every chemical instructor must meet whose duties call him to impart to a mixed class of academic students a maximum of knowledge in a minimum of time. A considerable portion of the first two chapters is devoted to an explanation of the theory of chemical reactions and nomenclature. The student is at once inducted into the notions of modern chemistry and familiarized with atomicity and the present chemical nomenclature. It is certain that under a good teacher any faithful student will master the main points of qualitative analysis by the time he has gone through the second part of this useful little volume. Only 34 of the 64 radicals known to chemists are treated of in this book. This brevity sometimes mars symmetry and renders the work of the student almost too simple, as when, for example, strontium is left out of group II. The tables IV and V, intended to record in a compact form the facts of analytical chemistry, are ingeniously devised by Mr. Perkins, to give the student exact ideas and methodical habits.

4. *The Fruits and Fruit trees of America, &c.;* by A. J. Downing. *Second revision and correction, with large additions*, by Charles Downing. pp. 1098, 8vo. New York, 1869. (John Wiley & Son).—Those cultivators of fruits who have been accustomed to handle the small duodecimo volume, which was left us by its lamented author under the above title, will hardly recognize their old acquaintance now grown to such portly dimensions. But the same rural flavor and discrimination are found in the work which Mr. Charles Downing has given us, that rendered the original edition of this work by his gifted brother a universal favorite on both sides of the Atlantic. But under Mr. Charles Downing's editorship

this work has become in fact a pomological encyclopedia, embracing a vast number of varieties of various fruits, which are of interest to the curious reader rather than the cultivator. For example, not less than 2800 varieties and synonyms of the pear are mentioned, while few cultivators in the Eastern United States plant over twenty or thirty sorts. Fortunately for the novice in pomology, Mr. Downing adds well considered select lists of the various fruits, that of the pear containing about seventy varieties, which serve to redeem the planter from chaos. The chapter on the grape, is enriched by well drawn figures of the most approved sorts of American origin, most of which have been produced by cultivation since 1845, when the first edition of Downing was published. It would have been well if the general introduction to this fruit had been rewritten or extended to embrace important new matters, as the sulphur treatment for *Oidium Tuckeii*, the later researches on the insects infecting the vine, and the considerable experiences which twenty years have given us, relating to vineyard culture and American vines.

The title of this book reminds us that its contents do not correspond to what it calls for. To one familiar with fruit culture as it exists on the Pacific coast of the United States, Mr. Downing's book appears more foreign than any European work on horticulture can to a cultivator living upon the Atlantic shores of the continent. It would have given a completeness to the work, if it had briefly sketched the main peculiarities of the most remarkable fruit-growing region of North America.

5. *Agricultural Qualitative and Quantitative Chemical Analysis*, (after E. Wolff, Fresenius, Krocker and others), by G. C. Caldwell, Professor of Agricultural Chemistry in the Cornell University. 300 pp. New York; (Orange Judd & Co.).—This work, prepared from the best sources in a thoroughly conscientious and judicious manner, supplies a want now beginning to be seriously felt in this country.

Prof. Caldwell's book is intended to serve as a complete manual of chemical analysis for the use of agricultural students. The titles of its chapters are—I, Reagents. II, Analytical Operations. III, Reactions and Methods of Quantitative Estimations. IV, Special methods of Analysis, viz: Course in Qualitative Analysis, including the elements encountered in Soils, Plants and Agricultural Products, Estimation of Water, Organic Matter, Sulphur and Chlorine in Organic Compounds, Separation and Estimation of the Alkalies, Alkali-Earths, Alumina, oxyds of iron and manganese, silica and phosphoric acid. V, Analysis of Soils, Rocks and Marls. VI, Analysis and Valuation of Fertilizers. VII, Ash-Analysis. VIII, Analysis of Fodder and Food. IX, Wool and Bark. X, Beverages. XI, Tables. We trust that this volume will be studied and used by every student in our Agricultural Colleges, for the knowledge that can be acquired only by following and by applying its methods is not only of the utmost importance to the individual farmer, but bears most seriously upon the development and conservation of our greatest national resource, the productive power of our soil. J.

6. *Lithologie des Mers de l'ancien monde, par* M. Delesse.—We have received from the author two beautiful charts, illustrating the above work, which we propose to notice in another number.

7. *Hydraulic Motors;* translated from the French cours de Mécanique appliqué, par M. Bresse, Professeur de Mécanique a l'Ecole des Pronts et Chaussées, by F. A. Mahan, Lieutenant U. S. Corps of Engineers. Revised by D. H. Mahan, LL.D., Professor of Civil Engineering, &c., United States Military Academy. 165 pp. 8vo. 1869. New York, (John Wiley & Son.)—A convenient and thorough treatise on the subject of Hydraulic Motors.

8. *Weisbach's Manual of Mechanics*, vol. i, part ii, 8vo. New York, (D. Van Nostrand).—The second part of the first volume of Weisbach's Mechanics, noticed in our last volume (p. 449), has just been received.

9. *A Practical Treatise on Metallurgy, adopted from the last German Edition of Kerl's Metallurgy*; by William Crookes, F.R.S., and Ernst Röhrig, Ph.D., M.E. In three large volumes, 8vo. London, (Longmans & Co.; New York, John Wiley & Son, 2 Clinton Hall, Astor Place.)—These volumes take up the subjects in the following order: Vol. I, Lead, Silver, Zinc, Cadmium, Tin, Mercury, Bismuth, Antimony, Nickel, Arsenic, Gold, Platinum, Sulphur. Illustrated by 207 wood engravings. 724, pp. 1868. Vol. II, Copper, Iron. Illustrated with 273 wood engravings. pp. 876, 1869. Vol. III, Steel, Fuel; Supplement. Illustrated with 145 woodcuts. 803, pp. 1870. We have no space at this time for a further notice.

10. *Reliquiæ Aquitanicæ;* by Messrs. Lartet & Christy, edited by Prof. Thomas Rupert Jones. Part ix, of this beautiful work on the Archæology of Southern France, has been published.

Lea on the Genus Unio. Index to Vol. xii and supplementary index to Vols. i to xi of Observations on the Genus Unio, together with Description of new species of the Family Unionidæ and description of new species of Melanidæ, Paludinæ, Helicidæ, etc. Isaac Lea, LL.D., etc. Philadelphia, 1869.

Bulletin of the National Association of Wool Manufacturers. Oct., 1869. Boston.

Address delivered on the Centennial Anniversary of the birth of Alexander von Humboldt under the auspices of the Boston Society of Natural History, by Louis Agassiz. Boston, 1869. 108 pp. 8vo.

Mammalia of Massachusetts, by J. A. Allen. 112 pp. 8vo. Bulletin of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass. No. 8. 1869.

Alexander von Humboldt, eine wissenchaftliche Biographie, by Dr. Carl Bruhns, aided by various Savans of Germany, is soon to be issued in two large volumes by F. A. Brockhaus of Leipzig.

Obituary.—Thomas Graham, author of the excellent "Elements of Chemistry," and since 1855 Master of the Mint, died Sept. 16th, in his 64th year.

Axel Joachim Erdmann, Director of the Geological Survey of Sweden, and eminent in Mineralogy as well as Geology, died at Stockholm on the 1st of December last, at the age of 55 years.

O. L. Erdmann, the distinguished Chemist, and editor of the Journal für praktische Chemie since 1843, died on the 9th of October at Leipzig, aged 65 years.

Michael Sars, the eminent zoologist of Norway, and one of the first in Europe, died at Christiania on the 22nd of October last, at the age of 65 years. The last of his remarkable memoirs is noticed in the last volume of this Journal, (p. 142).

THE

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JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XVII.—*Photometric Experiments.* PART FIRST.—*On a simple form of Photometer for determining the amount of Light reflected by Metallic Surfaces at different incidences;* by OGDEN N. ROOD, Prof. of Physics in Columbia College.

THE fundamental idea of many forms of photometer is based on the comparison of two illuminated surfaces placed in immediate juxta-position, and the judgment of the experimenter is called on to decide when the brightness of these appears equal. If the difference of the two rival surfaces be considerable, the inequality will be perceived even by an unpracticed eye, but as the disparity is diminished the observer becomes less confident, and, passing into a doubtful and dissatisfied frame of mind, is at one moment inclined to consider the balance as gained, and at the next equally certain that it has been lost. It hence follows that no photometer of this form is adapted for investigations at all aiming at a refined character.

Bunsen, to whom Physics as well as Chemistry is under so many obligations, employs in the instrument which bears his name another principle: here, it is not the equality in the brightness of two adjacent surfaces, but the actual disappearance of a "spot" on an illuminated ground, the powers of the observer being now taxed to a much less extent, as it is merely a question of the visibility or invisibility of the "spot." The great superiority of this latter principle is evident from the mere statement, and it only remains to investigate how the proposed advantage can best be realized in actual practice. It is, I believe, generally admitted by those who have used Bunsen's photometer, that with it, the end proposed is only approximately attained, the spot never becoming absolutely invisible, but merely assuming what may be termed an appearance of maximum faintness. Dove* has proposed a form of photometer, the general idea of which is not unlike that of Bunsen; it consists of a compound microscope provided with a microscopic photograph on glass, which serves as the "screen," the intention being to illuminate it from both sides, in such a way that the photograph should assume the same degree of brightness with its own border or ground, and hence become invisible after compensation. In a former number of this journal† I described a form of photometer partly based on this suggestion of Dove's, and at the same time detailed a few rough experiments that were made by its aid. Since then I have followed up the subject, and have examined somewhat, with the help of a more refined apparatus, the practical application of the physical and physiological principles involved in its more perfect construction and use.

The microscope and microscopic photograph I dispense with, gaining thus not only in economy but also in delicacy, while the plan of employing a silver-collodion film on glass was retained and proved of great value, and, as will be shown in the second part of this paper, the idea of a total disappearance of the "spot" was at length realized to the fullest extent, when the precaution of using an absolutely unvarying illumination was observed. I proceed now to describe the instrument, and will afterward add some determinations that were made by its help.

Description of the instrument. "The Screen."—If we take a plate of glass covered by an opaque preparation, with a surface of some reflecting power, but without polish, and laying bare a small portion of the glass, say $\frac{2}{10}$ of an inch square and having backed it up by a plate of ground glass, illuminate it front and rear by two lights, it is evident that a mass of light will be reflected to the eye of the observer from the opaque pigment, while light coming from the second source will traverse the naked portion of the plate and reach the same destination. This second portion, or that coming from the "spot," will have commonly a greater or lesser degree of intensity than that *reflected* from the adjacent regions of the plate, so that we shall have either a bright spot on a dark ground, or a dark spot on a light ground, and an attempt to equalize the two illuminations, so as to effect the disappearance of the "spot," will be successful only under the following conditions:

First. All portions of the "spot" must be equally illuminated, and it must have no texture, that is, must not perceptibly consist of minute grains mingled with others having a less or greater degree of brilliancy.

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* Pogg. Annalen, Band cxiv, p. 145.

† Vol. xxxvi, July, 1863.

Second. All those portions of the ground surrounding and in contact with the "spot," must be equally illuminated, and the texture of the ground must likewise be uniform.

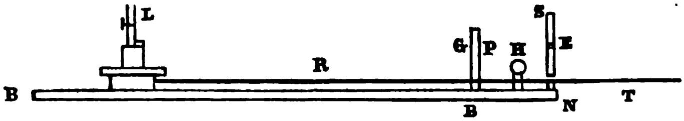
Third. The transition from the ground to the "spot" must be perfectly sharp and sudden, so that not the faintest border can be seen surrounding it.

Fourth. If it be required to render the invisibility of the "spot" more than momentary, it is equally essential that the ratio subsisting between the two sources of illumination should be truly constant.

If either of the first three precautions be neglected, the disappearance of the spot becomes entirely impossible, while it is only by a peculiar arrangement of apparatus that the fourth can actually be realized. (See second part of this article.)

Screen.—A plate of colorless glass of good quality is coated with photographic collodion and immersed for a few minutes in a solution of nitrate of silver, ("bath,") as though it was the intention to take a picture, ("negative"); it is then exposed for a minute or less to ordinary daylight, and a solution of photo-sulphate of iron poured over it. This produces a dense opaque deposit of silver in the substance of the film, when the plate is to be washed well in plain water and dried. Its surface will be of a grey tint, and will vary somewhat in its power of diffuse reflection according to the sample of collodion used. A small amount of light is also regularly reflected by the upper surface of the collodion film, and in using the plate it is always so placed that this latter portion shall produce no effect on the result. The collodion film is now removed neatly, by the aid of a needle, from a portion of the plate, so that a square with sides $\frac{2}{10}$ of an inch is laid bare, care being taken to leave the edges clean and well defined, in which there is no particular difficulty, provided the collodion was originally of the proper quality to cause it to adhere well. If the "spot" is made much smaller than the above mentioned dimensions it becomes an annoying object for observation, while if it be larger, it is difficult to illuminate it uniformly. This plate is seen at P, in figure 1, the collodion side being turned toward the eye of the observer, and the other side, except just opposite the spot, covered by a coating of lampblack mixed with weak shellac varnish, so as to leave a black, non-reflecting surface. At the distance of an inch from the collodion plate, there is fastened, parallel to it, a plate of colorless

1.



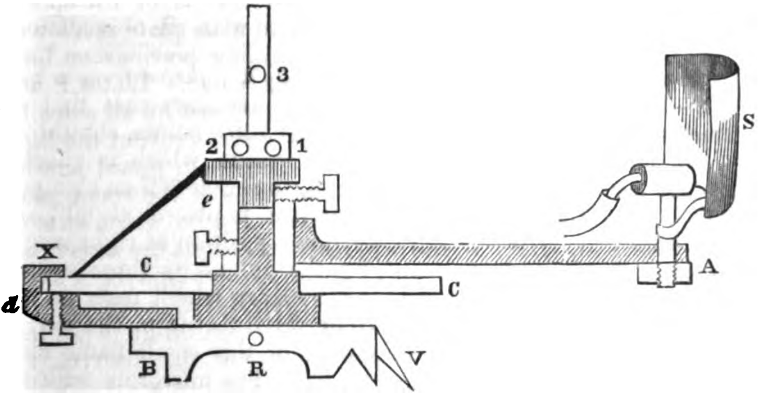
glass, G, finely ground on each side; this is destined to receive the light from L. If only one surface of the glass be ground, the texture becomes plainly visible, particularly when, as in the experiments detailed in the second part of this paper, the spot is magnified. Only so much of the ground glass plate is allowed to remain bare as is necessary, the remaining portions on both sides are covered by black non-reflecting paper. Plates P and G are then as it were roofed over, and enclosed on all sides by a small well fitting blackened case, destined to prevent the light from penetrating between them, and finally, to guard against possible errors by reflection from the walls of the room, plate G is provided with a projecting blackened tube which cuts off stray reflected light. P and G, constituting thus the screen, are fastened at the end of a pair of long parallel iron bars, B, B, made like the ways of a lathe, and provided with a scale graduated in millimeters. The length of this iron frame is six feet.

Source of illumination.—At E is the eye of the observer, the face being protected from light by the blackened screen, S. E, the center of the "spot," and the center of the mirror experimented on, all lie in the same line, which is of course at the same time the axis of the instrument. At H are two small gas flames issuing from circular apertures, and destined to illuminate the collodion plate on the side next to the observer; both are fed from the same source. The gas-burner at H consists of two thin brass tubes, half an inch in diameter and one inch long, connected together by a glass tube; the circular apertures for the flames are placed at equal distances on either side of the spot, and as far from it as is found most advantageous in any particular set of experiments; their distance from each other is seven inches. The direct light from the two flames is prevented from reaching the eye through the observing aperture, by small blackened screens, the same means being employed to arrest it in its course toward L. If instead of two, only a *single* gas flame is employed at H, the ground around the spot will be unequally illuminated, and exact observations become impossible. Of course the direct light from these two burners which penetrates through the "spot," must not be allowed to reach that portion of the ground glass opposite it; the distance of the flames apart must be so chosen that this becomes impossible.

The light from the movable burner on the other side of the screen at L will be used direct and reflected. The small *single* movable burner which supplies it, is similar to those already described, and to it is attached a flexible india-rubber tube, which is supplied from the same source that feeds H; it is connected by a *wooden* column, (for the sake of insulating the heat,) to the arm A, fig. 2.

I pass now to the joint support of the arm, A, and of the mirror to be experimented on. It consists of a block of brass,

2.



B, four inches square (see figure 2, which is one-third of the real size); its under surface is cut in such a way as to fit the two parallel iron bars and to slide on them easily but steadily, and this foundation block is farther provided with a vernier, V, to read off the distances on the millimeter scale. C is a graduated circle six inches in diameter, and is provided with a clamp at *d*. The hollow massive cylinder, *e*, supports the arm A, and also carries the axis of the support of the mirror. The screws, 1, 2, 3, serve to bring the mirror into its correct position; it is pressed against them by a band of india-rubber attached to the edges of the mirror. It will be seen that owing to this arrangement, all the different parts have motions quite independent, and yet by the clamping screws can at any moment be connected. Finally, attached to this stand is a long light rod of wood, R, reaching to the observer, and enabling him, by varying the distance between this movable piece and the fixed screen, to effect compensation.

Flexible Gas tubes.—It occurred to me that by splitting a stream of gas and sending one portion to L, fig. 1, the other to H, it would be possible to secure a uniform ratio between the two illuminations, as it would seem that any cause which increased the pressure in one of the branches of the tube ought to be equally operative in the other, so that after a compensation had been effected it should be permanent. In practice this was not found to be the case; the compensation point gradually shifted its position in the course of the evening away from H, sometimes to the extent of 50 or 60 millemeters. This difficulty was remedied by a suggestion from Mr. Lewis Rutherfurd, who suspected that the trouble came from the unequal length of the two branches of the supply tubing, in point of fact, on giving them equal lengths and diameters, and making the compensations slowly, so that the branch attached to L was not much shaken or set in oscillation, this source of error became so greatly reduced as in no way to interfere with the observations.

Mode of measuring the amount of light reflected from a mirror.—To adjust the apparatus, it is first necessary that that diameter of the circle joining the 0° and 180° points, should be made parallel to the axis of the instrument, which is effected by the use of the vernier at X, fig. 2, the circle is then clamped. Next, the vernier attached to the mirror is placed at 90°, and the mirror itself is made to assume a position at right angles to the axis of the instrument, by the aid of its three screws, and a small gas flame placed on a support which rests on the parallel bars at a distance of two or three feet from the mirror, it being so contrived that the center of this small flame shall just lie in the axis of the instrument. The mirror is adjusted till it reflects the light of this small flame back to the eye through the flame itself, securing thus the collimation with the desired accuracy. The arm A is then set at any desired angle with the mirror, and the two clamped together, when it is easy to arrange matters so that the reflected light shall be sent from the mirror along the axis of the instrument to the fixed screen at G. The small shade at S, fig. 2, prevents the *direct* light from reaching the same destination. After a compensation has been effected with reflected light, the arm A is made parallel with the axis of the photometer by the aid of its vernier, and another compensation is made with the *direct* light, the small shade being now placed behind the flame so as to be out of the way and to prevent light from reaching the mirror.

Mode of registering the observations.—These were always registered with a sharp point on a slender fillet of paper, attached to the long wooden rod R T, fig. 9, used for moving the mirror. This point was one end of a small lever placed at N, fig. 1. In consequence of this, at the end of a set of experiments two groups of dots were found on the paper, admitting of the most exact measurement on the following day. Before removing the paper from the rod two dots were always made on it in the neighborhood of these groups, the corresponding positions of the vernier V, fig. 2, being at the same time noted. A glass slide, ruled with millimeter lines, was then to be placed over the detached fillet of paper, and the observations recorded in the note-book. In the determinations given below, the observations on the direct and reflected light were made alternate, so as to avoid errors due to the shifting of the compensation point.

Observations on the amount of light reflected by a glass plate silvered by Liebig's process, the *silver* side being used. The light was reflected at an angle of 45°.

1.

Distance when mirror was used.	Distance with free flame.
946	1297
949	1301
950	1302
951	1302
952	1304
955	1305
956	1306
957	1308
958	1309
961	1309
961	1310
962	1312
965	1312
966	1315
967	1318
967	
	15)19610
16)15323	
	1307·3
957·68	105·
Correction 191·	
	1202·3
1148·68	

Result 91·26 per cent reflected.

2.

Distance when mirror was used.	Distance with free flame.
936	1286
940	1287
941	1288
942	1288
943	1290
944	1292
946	1292
946	1293
947	1295
948	1296
949	1297
950	1298
952	1298
953	1301
954	1303
955	1303
956	1304
958	1305
959	1307
960	1307
961	1307
962	1308
965	1312
	1312
23)21867	1312
	1315
950·7	
Correction 191·	26)33796
1141·7	1299·8
	Correction 105·
	1194·8

Result 91·3 per cent reflected.

These figures are taken of course directly from the notebook, and in making an examination of them it is to be remembered that the corrections, 191 and 105, (for false positions of the vernier), are to be applied to each of the distances in the respective columns, before a correct judgment of the closeness of individual observations can be made.

When the same mirror was used at 5°, i. e., the light being reflected from it nearly perpendicularly, the results given below were obtained.

1.

Distance when mirror was used.	Distance with free flame.
955	1297
958	1304
958	1304
958	1305
960	1305
962	1307
962	1308
963	1310
963	1310
965	1313
965	1313
969	1313
970	1314
971	1314
972	1315
973	1318
977	1318
977	1328
980	
983	18)23596
983	1310·8
21)20324	105
967·8	1205·8
191	
1158·8	

Result, 92·35 per cent reflected.

2.

Distance when mirror was used.	Distance with free flame.
954	1298
955	1301
956	1302
957	1303
957	1303
958	1304
958	1305
961	1306
961	1307
962	1308
962	1308
963	1309
963	1310
964	1312
965	1312
967	1313
968	1315
969	1316
972	18)23532
19)18272	1307·4
961·6	105
191·	1202·4
1152·6	

Result, 91·88 per cent reflected.

In the second part of this paper I shall detail the results of several sets of experiments made with a truly constant illumination, in other respects the mode of experimenting being quite similar to that used in obtaining the above results, when it will be seen that about two-thirds of the scattering in the figures was due to the shifting of the compensation point during the experiments, and not to any defect in the screen.

In the same way experiments were made on another mirror silvered by Liebig's process, the glass side being used, with the result that out of a hundred rays 78·01 were reflected, the angle being 45°; while an amalgam mirror tested at the same angle reflected only 44·58 per cent.

ART. XVIII.—*Contributions to the Chemistry of Copper;* by T. STERRY HUNT, LL.D., F.R.S. PART 1.

[**Read before the American Association for the Advancement of Science at Salem, August 25, 1869**]

§ 1. THE resemblances between silver and copper in its cuprous form have already attracted the attention of chemists. The ordinary chlorid of silver (argentic chlorid) and the dichlorid of copper (cuprous chlorid) have many properties in common. Both of these chlorids are white, readily fusible, and blackened by exposure to light; both of them are insoluble in water but dissolve in ammonia and in aqueous solutions of other chlorids, in which however the cuprous is far more soluble than the argentic chlorid. A saturated solution of chlorid of sodium holds at 90° Centigrade, 16·9 per cent of cuprous chlorid, at 40° C., 11·7, and at 11°, 8·9 per cent. A solution containing fifteen per cent of chlorid of sodium retains at 90° C., 10·3 per cent of cuprous chlorid, at 40°, 6·0 per cent, and at 14°, 3·6 per cent; while a solution with only five per cent of chlorid of sodium holds of the cuprous chlorid at 90°, 2·6, and at 40° only 1·1 per cent. These determinations are from single observations and therefore require verification. From the sparing solubility of the cuprous chlorid in dilute solutions of chlorid of sodium it follows that the denser saturated solutions are copiously precipitated by dilution with water, which causes the separation of white cuprous chlorid in a crystalline condition.

§ 2. The aqueous solutions of the chlorids of calcium, magnesium, zinc, manganese, cobalt, ferrosum and cupricum, also freely dissolve cuprous chlorid, and it is probable that this property is shared by other soluble chlorids. The strong affinity of cuprosum for chlorine enables cuprous oxyd to decompose all the chlorids just named, with the exception of those of sodium and calcium, with separation of the corresponding oxyds and formation of cuprous chlorid. In the case of zinc and manganese, insoluble oxychlorids of these metals are formed at the same time. These reactions require further study, and the same may be said of the cupric and cobaltic chlorids with cuprous oxyd. I have, however, partially investigated the behavior of cuprous oxyd with magnesic and ferrous chlorids, and obtained the results about to be described.

§ 3. The cuprous oxyd for these experiments was prepared by gently heating a solution of sulphate of copper mixed with cane sugar and an excess of caustic soda, until the whole of the copper was thrown down as a bright dense cinnabar-red powder which was carefully washed and dried. A concentrated solution of chlorid of magnesium dissolves this oxyd in the cold, and more readily when heated, with separation of hydrated oxyd of magnesium and cuprous chlorid, which latter is held in solution by the excess of magnesic chlorid. By filtering the liquid while hot, and washing with a strong solution of chlorid of sodium, the hydrate of magnesia may be separated, and the dissolved copper subsequently precipitated by metallic iron from the colorless filtrate, ferrous chlorid being formed. Experiment shows that the reaction between the red oxyd of copper and chlorid of magnesium may be represented as follows: $Cu_2O+MgCl=Cu_2Cl+MgO$.

§ 4. A solution of magnesian chlorid nearly saturated when hot with cuprous oxyd, and allowed to cool in contact with the precipitated magnesian hydrate, deposits a portion of orange colored oxyd, or perhaps an oxychlorid, which disappears as often as the solution is heated. The solid cuprous chlorid is moreover decomposed when digested with water and magnesia, hydrated cuprous oxyd and magnesic chlorid being formed. The double chlorid of cuprosum and magnesium is however stable, even in the cold, in presence of magnesian hydrate, provided a considerable excess of magnesian chlorid be present. From a filtered solution of cuprous oxyd in chlorid of magnesium water precipitates a large portion of the cuprous chlorid, in this case colored orange-yellow from adhering oxyd, due to the reaction of a little magnesia, which remains dissolved or suspended in the concentrated solution even after filtration. A solution of magnesian chlorid of specific gravity 1·23, retains in solution at 12° Centigrade, about 7·10 per cent of cuprous chlorid. A solution of magnesian sulphate with chlorid of sodium may be employed to dissolve cuprous oxyd. This, like all similar solutions of cuprous chlorid, rapidly absorbs oxygen from the air and deposits a pale green cupric oxychlorid.

§ 5. With ferrous chlorid and cuprous oxyd it might be expected, from analogy with the magnesian salt, that we should obtain cuprous chlorid and ferrous oxyd, but the reaction is complicated by the tendency of the latter to pass to the state of ferric oxyd. When ferrous chlorid in solution with chlorid of sodium is heated with a sufficient quantity of cuprous oxyd, the whole of the iron is precipitated as ferric oxyd, mingled with metallic copper, while cuprous chlorid remains in solution. Experiments made with an excess of ferrous chlorid show that one third of the copper is reduced, while two thirds are dissolved as dichlorid. This reduction may be effected directly by ferrous oxyd; if to a solution of cuprous chlorid in chlorid of sodium, we add hydrated ferrous oxyd recently precipitated by an alkaline base and still suspended in the liquid, it is at once converted into ferric oxyd, with precipitation of metallic copper. The first stage in the action of ferrous chlorid on cuprous oxyd may be represented as similar to that of magnesic chlorid: $Cu_2O + FeCl = Cu_2Cl + FeO$. In the second stage $Cu_2Cl + 3FeO = Cu_2 + FeCl + Fe_2O_3$. It follows from this that one-third of the cuprous chlorid formed in the first stage is reduced to the metallic state, and the final result may be represented as follows: $3Cu_2O + 2FeCl = 2Cu_2Cl + 2Cu + Fe_2O_3$.

A similar result is obtained if ferrous chlorid is added to an unfiltered solution of cuprous oxyd in chlorid of magnesium. The suspended hydrate of magnesia in this case liberates an equivalent of ferrous oxyd, which reduces to the metallic state one-third of the dissolved cuprous chlorid, in accordance with the second reaction given above.

§ 6. The reducing power of ferrous oxyd is also shown with cupric chlorid, which is at once converted by it into cuprous chlorid in accordance with the equation, $2CuCl + 3FeO = Cu_2Cl + FeCl + Fe_2O_3$. The further action of ferrous oxyd will, as we have seen, reduce the cuprous chlorid to the metallic state: in fact, $2CuCl + 6FeO = 2Cu + 2FeCl + 2Fe_2O_3$. If recently precipitated hydrated ferrous oxyd or ferrous carbonate be added to a solution of cupric chlorid in the proportions indicated by the last equation, the whole of the copper is separated in the metallic state, mingled with ferric oxyd, while ferrous chlorid is found in solution. The reaction with ferrous carbonate, which requires a gentle heat, is accompanied by a violent disengagement of carbonic acid gas. This experiment is best made by dissolving in water ferrous sulphate and sodic carbonate or sodic hydrate in the proportions required, and adding thereto a solution holding the proper amount of cupric chlorid. Under certain conditions the cuprous precipitate is brownish-black in color, like that obtained by heating ferrous chlorid with cuprous oxyd, but more generally it is of a bright red color, and often coats the glass with a mirror-like film. A warm solution of cupric chlorid with chlorid of sodium at once converts the metallic copper of the precipitate into cuprous chlorid, which is dissolved, leaving behind only hydrated ferric oxyd. When a solution of ferrous chlorid with chlorid of ammonium and excess of ammonia is added to a solution of a copper salt the precipitated films of metallic copper sometimes possess considerable brilliancy and show a bluish translucency. It is to be remarked that although the cupreous precipitate thus obtained is bright red in color, that which is produced by boiling cuprous oxyd with ferrous chlorid is nearly black.

§ 7. It was long since shown by Levol that hydrated ferrous oxyd will reduce cupric to cuprous oxyd, and this, as we have already seen, can separate from its combinations ferrous oxyd, whose reducing power may be still further exerted upon the cuprous combination thus formed. These facts serve to explain the results obtained by E. Braun (*Zeitschr. Chem.*, 1867, p. 568, cited in *Jahresbericht* for 1867), which were not known to me at the time of making these experiments. He found that by digesting cupric hydrate or cupric carbonate with ferrous sulphate in solution there was obtained a reddish mixture of basic ferric sulphate with cuprous oxyd, formed apparently in accordance with the equation,

$$2FeO, SO_3+2CuO=Cu_2O+Fe_2O_3, 2SO_3.$$

This, when boiled with a further portion of ferrous sulphate, became black in color, and from the small amount of oxygen present was supposed to contain metallic copper. By adding a large excess of carbonate of ammonia to a mixture of ferrous and cupric sulphates, Braun succeeded in obtaining solutions in which all the copper was present in a cuprous form, and even in reducing portions of it to the metallic state, a process which we have seen is complete when the requisite amount of ferrous oxyd is brought in contact with the chlorids of copper.

§ 8. In this Journal for March, 1867, page 308, I described briefly the reaction between cupric oxyd and ferrous chlorid, according to the equation, $3CuO+2FCl=Fe_2O_3+Cu_2Cl+CuCl$. I was not then aware that the same had been shown by Meyer (Berg. und Hutt. Zeit., 1862, 182, cited by Kerl).* Further studies of this reaction have given me interesting results. The black oxyd of copper, even after ignition, is attacked by ferrous chlorid in the cold, but the insolubility of the resulting cuprous chlorid retards the action. If however the ferrous chlorid be mingled with a strong solution of chlorid of sodium, and heat applied, the cuprous chlorid is readily dissolved, and the reaction is rapid and complete, the whole of the iron separating as a bulky reddish-brown precipitate, provided three equivalents of cupric oxyd have been taken for two of ferrous chlorid. The greenish solution thus obtained readily dissolves precipitated metallic copper, in virtue of the cupric chlorid which it contains, and, unless a large excess of chlorid of sodium be present, deposits white crystalline cuprous chlorid by cooling or by dilution. When digested at a temperature of 50° Centigrade with carbonate of lime, the greenish solution deposits one-third of its copper as a pale green insoluble cupric hydro-carbonate, while the colorless filtrate retains the remaining two-thirds in the form of cuprous chlorid. If a solution of ferrous chlorid with chlorid of sodium is digested with a sufficient excess of cupric oxyd the cupric chlorid formed unites with the latter to form an insoluble cupric oxychlorid, and only cuprous chlorid remains in solution.

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* Metall. Huttenkunde, xi, 588.

§ 9. For the ferrous chlorid in the experiments in § 5 and § 8, a solution of ferrous sulphate with chlorid of sodium may be substituted. When cupric oxyd is heated with an excess of ferrous chlorid, a small portion of ferric oxychlorid is produced. The red-brown precipitate may be washed free from cupric, cuprous and ferrous chlorids by a strong solution of chlorid of sodium, but will then yield to pure water a portion of soluble ferric oxychlorid. By careful desiccation in a water-bath and subsequent washing with dilute alcohol the ferric precipitate may be obtained free from chlorid of sodium, and completely insoluble in water; but its composition appears to be variable. Of two preparations the first contained one equivalent of chlorine for eleven, and the second, one for twenty equivalents of iron. In another experiment where fine oxyd of copper from the calcination of malachite was dissolved in an excess of a mixture of ferrous sulphate and chlorid of sodium at a boiling heat, it was found that for thirty equivalents of copper dissolved there were precipitated twenty-one equivalents of iron, instead of twenty as required by the formula given in § 8; the additional equivalent being separated as ferric chlorid in union with the ferric oxyd. The production of a small and variable amount of ferric chlorid in the above conditions is apparently due to a secondary reaction between cupric and ferrous chlorids in the presence of ferric oxyd; $2CuCl+2FeCl = Cu_2Cl+Fe_2Cl_3$. This point however requires further investigation.

§ 10. The facility with which cupric chlorid parts with one-half of its chlorine and passes into the more stable cuprous compound is shown by its well known power to chloridize not only metallic copper, but metallic silver and even sulphid of silver. Its action on cuprous sulphid is not less remarkable. A strong solution of cupric chlorid mingled with chlorid of sodium rapidly attacks pulverized copper-glance, even in the cold, sulphur being separated and cuprous chlorid formed; $2CuCl+Cu_2S = 2Cu_2Cl+S$. Chalcopyrite, on the contrary, is but slightly acted upon by such a solution, which, however, slowly takes up a portion of iron, forming ferrous chlorid with a corresponding amount of cuprous chlorid.

ART. XIX.- -*Notice of a recent Land-slide on Mount Passaconaway*; by GEO. H. PERKINS, Ph.D., Prof. Zoology and Geology, University of Vermont.

THE name Passaconaway is given to a somewhat extended mountain in Grafton county, New Hampshire, twenty miles northeast of Plymouth, and about the same distance southwest of Mount Washington. It consists mainly of three conical peaks of nearly equal height, which form a group at the southern end, and of a high ridge running several miles northeast. A much smaller ridge stretches in a southerly direction. During the great rain of October 4th, there was an unusually large land-slide upon the southwestern slope of the most southern of the three highest summits. The light-colored streak down the mountain side, which marked the course of the slide, could be distinctly seen for more than fifty miles. It was at this distance that I first saw it, two weeks after its occurrence, and with my friend Rev. M. T. Runnels of Sanbornton, set out to examine it. At Campton we were joined by Mr. Chas. Cutter, to whose knowledge of the region and general kindness, much of our success was due. From Campton a ride of ten miles in a northeasterly direction up Mad River valley brought us to Waterville; thence we proceeded on foot. After walking two or three miles we reached a level clearing of fifty acres through which Mad river runs, here only a few yards across. This space was covered with great heaps of logs, some of them very large, brought down from the débris of the slide during the freshet that attended or followed it. That they came from the slide was very evident, for nearly all were broken as if suddenly snapped in two, many had one or both ends crushed to splinters so fine that they seemed like great brushes, and all were entirely stripped of foliage and of most of the smaller branches.

These logs were mainly spruce, and some were fifty to sixty feet long, and one to two feet in diameter. They were piled up in great confusion to the height of fifteen or twenty feet. Subsequent investigation showed that this was at least three miles below the terminus of the slide. Nowhere else was there such a mass of timber as here, nor any where else was the ground so favorable for such an accumulation; for elsewhere the banks of the stream were high and rocky, and not more than eight to twelve rods apart.

The whole mountain is covered to the very top with a forest of spruce, and to reach the slide most easily we followed the bed of the stream along its side. At short intervals were piles of logs more or less broken, by which the stream had evidently been in several places completely dammed.

Trees on the banks twenty feet or more above the water were scarred, rubbed, and even uprooted, by those borne on the current of the swollen stream. A toilsome walk brought us to the foot of the slide which lay directly in the bed of the stream. This side of the mountain is quite steep, and in outline regularly triangular. Its height is about four thousand two hundred feet.

The slide commences forty rods from its summit, and a little to one side of the highest point. At the outset it is very narrow, being not more than a rod wide; a narrow tongue runs above this, however, for a short distance. For fifty or sixty rods the increase in width is very gradual. The inclination is very steep, appearing almost perpendicular and probably not less than fifty to sixty degrees. For the next hundred rods the width increases more rapidly. A hundred and thirty rods from the top the widest part is reached. Here the sides bend gradually outward and the width is twenty-five to thirty rods. From this point the sides begin slowly to approach each other, and thirty-six rods below, a hundred and fifty-six from the top, the width is nearly seventeen rods. The course from the top to this point is in a direct line, but here a curve toward the northwest begins and ends eighty rods below, nearly at a right angle with the main axis. The whole length is nearly two hundred and forty rods, and the outline is fusiform, with the lower end curved to one side. Directly across the line of the main axis a few rods from the foot of the mountain runs a high ridge. Instead of striking this, as would have been expected, the slide begins to turn nearly a hundred rods above, and when within twenty rods is almost parallel with it. There seems to be no reason why sand, rocks and débris of all sorts should not have been thrown against this ridge; indeed there is every reason why they should have been. Yet the space between it and the slide is singularly free from such material. There is undoubted evidence that one or more watercourses ran down the mountain before the slide, and probably did much to cause it. These streams, which form part of the source of Mad river, must have been very inconsiderable, and yet the mass of rock and sand seems to have been guided by them in its downward course. It appears incredible that so great a mass moving with power and velocity enough to snap off hundreds of great trees, crushing many of them to splinters, and piling up such heaps of débris, could be directed by such small watercourses, but the facts indicate it.

There has for a long time, been the track of a former slide down a part of the surface covered by the more recent one, but it was very small. Contrary to our expectations the side of the mountain over which the slide passed, was not bare rock stripped of surface material. Only in two places, both small in extent, was the rock foundation of the mountain exposed. One of these was at the top where the slide seemed to have started from a ledge, the other was a little less than half-way down, and reached entirely across the track of the slide. With these exceptions the whole surface was covered with a loose, coarse sand or gravel, consisting entirely of comminuted rock, increasing in depth from top to bottom and very loosely compacted. The thickness of this loose material was shown along the sides of several small streams that were running down the slide, which, at the time of our visit, the 20th of October, had cut entirely through it, and ran over the solid rock beneath. Near the top the ground was moist but there were no streams; these began twenty or thirty rods farther down. At the top of the slide, the surface sand was only a few inches deep; below the second exposure of rock it was between one and two feet; at the widest part, a hundred and thirty rods from the top, it was from six to eight feet; a hundred and fifty rods, it was nine to ten feet, and at the bottom fifteen to twenty-five, and in some places, even thirty feet deep. At this point the slide seemed to have suddenly stopped, for there was no gradual diminution in thickness below it.

How much of this coarse gravelly sand originated with the slide, being ground by it from larger rocks; and how much existed before as surface soil disintegrated from the solid rock, I could not estimate. The sand was pure syenite and contained no trace that I could find, of any vegetable mold. From an examination of the banks of Mad river below, and of the soil by the side of the slide, I am convinced that the greater part was produced by the disintegration and falling to pieces of the mountain's rock, and so existed before the slide, and was carried along and heaped up by it. The mountain is, so far as I could learn, wholly composed of light gray, rather coarse syenite, which appeared to disintegrate very readily, as I saw large masses that could easily be pulverized. A mile below the slide there were extensive layers of black hornblendic rock, and in one place the syenite was crossed by trap dikes, from an inch to a foot in thickness. Some of these dikes forked several times, others crossed each other in the form of a letter X, and some varied greatly in thickness along their course. A few broken crystals of rose-purple quartz, an inch or more in diameter, were found among the débris. But the only rock in place along the course of the slide was the syenite.

Scattered all over the surface of this loose, sandy material, were masses of syenite, of various sizes though none were very large. These were angular, and appeared to have been recently broken. They were undoubtedly fragments of large masses broken by the slide. Besides these, there were a few well worn boulders of syenite, quartz, and hornblende rock, some of which were near the top.

All the trees that had stood on the ground now occupied by the débris of the slide, were carried away or buried up, only a very few bare logs remaining in sight.

Along the sides of the slide the forest was full of uprooted, bruised, and broken trees.

The upper part of the slide was much more rugged and covered with a greater mass of broken rocks than the lower. Over the widest portion there were few rocks on the surface, which was smooth and level as if rolled. Below this it was rougher but not as much so as above.

The appearance of the surface was the same in all parts. Its color was a light yellowish brown, and at a little distance it closely resembled a field lately ploughed and harrowed.

It is the contrast of light color with the dark green of the spruce forest around it that causes the slide to be so distinctly visible at long distances. As is stated above, the upper portion of the slide is very steep, but after the first fifty rods the angle of inclination is less, and just above the widest part it is not more than twenty-five degrees. Below it is not more than fifteen degrees.

ART. XX.—*On the Silver Mines of Santa Eulalia, State of Chihuahua, Mexico*; by JAMES P. KIMBALL, Ph.D.

THE silver mines of Santa Eulalia were among the earliest mineral discoveries of the Spaniards in Northern Mexico. Don Jesus Inocente Irigoyen of Cusihuiriachic, a good antiquarian authority, states that the year of discovery was 1591. The only available official register of their performance, however, goes back no further than 1705, but mentions their discovery in 1703—twelve years after the city of Chihuahua was founded, according to the date given by Dr. Wislizenus. From 1705 to 1737, they produced 6,583,500 marcs, or an average of 1,938,903 dollars of silver per annum. Up to 1791, during a period of eighty-six years, their acknowledged production of silver, of which the *quinto*, or king's fifth, was paid to the royal exchequer, was 11,903,126 marcs, or nearly *one hundred and twelve millions of dollars*, and their entire production from one-fifth to one-third more. At this period the district had a population of 6000, and supported sixty-three reduction establishments with one hundred and eighty-eight smelting furnaces of the type known as the Mexican *horno*, and sixty-five cupelling furnaces; while a number of similar establishments in the city of Chihuahua were also kept running on ores from this district. The depredations of the savages, which until comparatively quite recently have always seriously interfered with industrial pursuits in Northern Mexico, became so grievous during the last five years of the past century that the district was gradually abandoned.* Indian hostilities were soon followed by political troubles, including the war with Spain and the proscription of the Spaniards. During the present century, operations at the hands of the Mexicans have never been fully resumed. Yet shallow workings have continued to furnish ores to a small number of furnaces, which to a considerable extent have also made use of the débris of former operations. At present ten blast furnaces, each of a capacity of 2500 to 3000 lbs. of ore per day, are in operation, their small supply of ore being drawn from the older as well as the newer mines. The fresh ore from the mines as charged to the furnace, is eked out with the settlings of the old slag heaps, and coarse refuse from old workings, extracted from the dry bed of the creek which in time of rain courses through the mining village of Santa Eulalia. As in old times, for the sake of mutual protection, the reduction works of the district are still all collected here, along with the dwellings of the miners and smelters, who number with their families some 700. Recollections with the old, and traditions with the young, still fill the minds of the dwellers in this narrow valley with dread of the Apache, who even now is occasionally found lurking among these mountains, and of whose hiding all have been taught to be wary when moving about alone.

The village of Santa Eulalia is fifteen miles east of the city of Chihuahua, across the expansive champaign valley of Tabalopa. This city, the capital and social center of the state of the same name, and containing some 20,000 inhabitants, attained its importance mainly through the silver industry of Santa Eulalia, in the period of whose prosperity it had a population of 70,000. Chihuahua afforded—what have ever been lacking at Sta. Eulalia—water and space for the dressing and reduction of ores; and to the city therefore was brought and smelted a large portion of them. Immense heaps of slag, twenty in number, on the outskirts, attest the extent of work done there during the last century, while the imposing, and in some respects admirable, cathedral of the city is a monument to the mines of Santa Eulalia, it having been built out of a fund raised by the tax of one real per marc of silver coined, and which, continued for sixty-two years, ending in 1789, amounted to 800,000 dollars.*

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* Manuscripts and certificates in No. 65, folio 36, state archives. See also Ward's Mexico, 1st edit., vol. ii, p. 129, (2d edit., i, p. 454).

The Sta. Eulalia mountains are a portion of a long range trending N.E.—S.W., one of a system of parallel ranges, which, separated by champaign valleys or valley-plains, characterize the topography of the sloping margin of the Grand Sierras, and as much at least of the state of Chihuahua as lies north of the 28th parallel, and east of Concepcion—beyond which limits my experience does not extend. The range lies between the broad and fertile valley of the Conchos on the east, and the narrower valley-plain of Tabalopa on the west. Access to the village and mines of Sta. Eulalia is from the latter. The village lies two-and-a-half miles from the plain up the mountain stream of the same name, at the foot of the higher hills, in which, at distances of two to five miles, the mines are situated, and which are crossed only by bridle-paths.† The mining ground is embraced within the area of an uplift of the Cretaceous fossiliferous limestone, imparting toward the center of it gentle quaquaversal dips. This is a prolongation of the narrow continuous elevation of this formation, reaching to the Rio Grande in an axis parallel to the Conchos river, and a part of the same formation which in Texas comes to the surface in the Rio Grande basin, at least, between the 28th and 31st parallels, except in mountainous localities, or in the case of stratigraphical depressions, where an upper and metamorphosed member of the same formation (a porphyritic quartzite) known in Mexico as *cantera*, caps the summits.‡ In Mexico, under similar conditions of superposition of the latter, the

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* State archives. Ward erroneously states this tax to have been collected during nine years pending the last *bonanza*, ii, p. 581. (2nd edit., p. 305).

Accustomed, as of late, we have become to the far higher returns of not larger groups of silver mines, it is easy to pass over the full significance of the comparatively low figures of early operations in northern Mexico, unless we are mindful that it is to the availableness of mechanical appliances, that our great production of silver is more to be ascribed than to the number or superiority of recent discoveries. What account of the Comstock lode by this time should we have were its mines, as in the case of those of northern Mexico during the period of their activity, without other than the rudest iron or steel implements, without powder, except in rare cases of a foreign stinted supply, with only the *adobe horno* for smelting, and the little *caso* for amalgamation; and continually harrassed by Indians? Although supplies are no longer quite out of reach, the mines to-day are worse off than during the last century, for most of them have reached a point where have become necessary mechanical appliances, with which the Mexicans are entirely unacquainted. They continue to suffer the want of what *adobe*, raw-hide, and wood will not supply, following the practice of the Spaniards, which in their hands has rather retrograded than advanced. The silver industry of Chihuahua at present amounts to little more than cleaning up the rubbish of former workings.

† For a popular and graphic account of this picturesque mining district, see an article by Gen Lew. Wallace in Harper's Monthly, Nov., 1867.

‡ See notes on the Geology of Western Texas and Chihuahua, by the writer, this Jour., xlviii, p. 378.

Cretaceous fossiliferous limestone, with local lithological differences, is the prevailing formation of the Rio Grande and Conchos basins, where, as seems likewise to be the case in Texas, within the development of the cantera it sustains throughout a metalliferous character. The mineral deposits of Sierra Rica, Cuchillo Parado, the Chupaderos, and the Chorreras in Chihuahua are all contained in it. In the Santa Eulalia mountains is its most westerly development in any great prominence above the valley plains, though seventy-five miles still further west, a limestone is said to form the low base of the Sierra de Magistral, where it is likewise metalliferous, and where, going west, it is last seen. In the Santa Eulalia mountains, the same as in other localities named, the *cantera* caps the higher elevations. This name is applied in Northern Mexico to the bleached portion of an essentially alumino-siliceous rock, generally more or less metamorphosed and ferruginous, and occurring in a great variety of colors.* As elsewhere explained, it is to the disintegration of this rock that the accumulation of soil in the valleys is due, as well as their peculiar conformation.†

Allowing for erosion, the mines of Santa Eulalia may strictly be said to be grouped in a single great boss of the Cretaceous limestone strata, gullied or scored by water-courses which impart to all portions of it a rugged and precipitous configuration. There, as elsewhere throughout the development of the fossiliferous limestone, the water-courses have cut bold, almost perpendicular, escarpments in which the stratification is very plainly marked. In this district the dips are from 5° to 15°, and from a point near the Vieja mine are quaquaversal. The ravine, Arroyo de Dolores, which takes its name from the old mine of Dolores, at its head, has been cut through the crest of the limestone uplift, and thus exhibits in steep mural escarpments a partial thickness of the formation of 400 to 500 feet. Its true thickness, however, has never been revealed. The oldest and most extensive mines are in this ravine, including the deep ones of Dolores, Vieja, Aguada, and the shallow or cavernous workings of Parcionera, San José and San Matias. The distance of Arroyo de Dolores from the village is some three miles, horizontal, from four to five topographical.

As the limestone uplift or boss expires, the summit cantera, a few remnants of which cap the limestone hills, sets in, and within a couple of miles, becomes the main formation, and forms the body of the range. No limestone appears in the Puerto de Dolores, seven miles to the north, where the San Diego and Chihuahua road crosses the Sta. Eulalia range; and in the village of Sta. Eulalia, it has already declined below the surface. The overlying cantera presents a greater development in the surrounding hills, which rise to the height of some 800 feet above the plain.

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* This Journal, xcviii, p. 380.

† Ibid, p. 381.

A curious lithological phenomenon, though in a less conspicuous way not uncommon in other localities which I visited in Chihuahua, is a formation of conglomerate which is found on all the slopes of the district, and which incases the lower and middle portions of the hills like a shell.* This has evidently been formed by carbonated calcareo-magnesian infiltrations, springing from the summit cantera; and, penetrating the fine and coarse detritus of the surface alike, have cemented it, and thus produced all the gradations from a fine friable sandstone to a coarse breccia. The village rests upon it. In the main street it is distinctly bedded, and cleaved by joints. Nowhere is it found entering into the interior structure of the hills, and no traces of it are found in steep places. Yet cursory observation might dispose the traveler to assume the main body of the mountains to be made up of this formation.

The mineral deposits of Santa Eulalia are unique. The only instance of a vein formation brought to my notice in the region is in the Santo Domingo mine. All the rest of the deposits are more or less irregular, and in a variety of modes of occurrence, are contained in the nearly horizontal fossiliferous strata (Cretaceous). All the strata above water level are exceedingly cavernous. In nearly all of the workings, caves, entirely shut off from the surface have been encountered. Some of these are of enormous size. The great cave of the Parcionera and San José mines, is said to be large enough to hold the cathedral of Chihuahua. Though unable to explore its height, or to illumine its roof, I am disposed to believe this. Drusy cavities or vugs of all sizes, are the smaller exhibitions of the same prevailing cavernous character. These latter yield excellent pockets of ore. Rich *bonanzas* have been got from chambers in the walls of the large caves. The ores which are mainly the chlorid and sulphids of silver, argentiferous galena and salts of lead, together with, (though of rarer occurrence,) the chloro-bromid (embolite) and iodid (iodyrite) of silver, are very ferruginous, to which circumstance they owe their friable character, and also, to a considerable degree, the spaces in which they have been deposited. Courses of ore are always marked by ferruginous stains, which, properly considered, are segregations of mineral matter, sometimes following cleavages and joints, and sometimes planes of bedding, and, again, sometimes reticulating solid beds, and in all these modes of occurrence, without order or defined limits. A bed, or a number of beds, of the limestone, in places may be thoroughly webbed with such segregations of so decided a character as to impart a concretionary appearance, or, as if limestone breccia were cemented by ferruginous ore. In such places the beds, are *not* brecciated, but thus strikingly indicate the energy of disintegrating and segregating action under the decomposition or oxydation of iron salts (probably proto-carbonate) originally contained in the limestone, together with diffused salts of silver. These ferruginous portions always afford good mining ground. As some of them are very extensive, and as their distribution is irregular, their excavation results in large and rambling chambers, generally ranging through a number of the heavy beds of limestone. Such chambers are often continuous with natural caverns, together forming underground spaces scarcely less imposing than the most noted caves that excite the wonder of tourists.

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* This Jour., xlviii, p. 382.

The mines of Santa Eulalia are scattered all over the great limestone uplift, and along the deep ravines which have scored it. All are comprised within an area of some five square miles. In the hillsides they consist mostly of horizontal workings, and occupy different strata from top to bottom. Shafts and deep workings, which are few and ancient, are located both in the hills and ravines. Accessibility of the mines is determined by the topographical configuration of the mining ground, the primary form of which, before modified by erosion, it is important to keep in view. As they are reached from the village by three different trails, I will describe them in as many separate groups, as follows: the *Santo Domingo group*, comprising the workings in the same cañon as the Santo Domingo mine, and of which this mine is the principal; the *Dolores group*, or those in the cañon, at the head of which is the old Dolores shaft; and the *Guadalupe group*, comprising the workings on the summit and S. W. flank of the limestone ridge, which forms the divide between the Dolores Cañon on the northwest, and on the southeast—the waters of the *arroyo* in which is the village of Santa Eulalia.

SANTO DOMINGO GROUP.—The Santo Domingo and neighboring mines are two miles north of the village, up near the head of a deep ravine, in both sides of which, at different elevations, they have their entrances. As this locality is about one-and-a-half miles to the southeast of the axis of the boss, the dip of the formation is seen rapidly declining in the ravine toward its mouth, and the overlying cantera is thus brought down so as to form the body of the hills. The limestone altogether disappears from above the bed of the ravine within a few hundred yards south of the Santo Domingo mine, where the cantera sets in and forms the surface. The town is really on the horizon of this formation, though the surface is immediately overspread by the cemented rubble above described. The hills on either side of the defile in which the village is situated, rising as they do some 700 feet above it, are both topographically and stratigraphically the highest elevations in the district. They are made up wholly of cantera distinctly and nearly horizontally stratified, and in different zones weathering with a variety of colors. They have mesa tops, and present to the broad valley-plain of Tabalopa bold escarpments flanked by low foot-hills.

The *Santo Domingo*, old and present, are deep workings entering the limestone boss on the west side of the ravine. The old mine, now abandoned and dismantled, consisted of a shaft located so high on the hillside that it must have pierced one hundred feet of cantera before striking the limestone. The present mine is some 500 yards further up the ravine, and, at an elevation above its bottom of some 120 feet, goes down in a bed of fossiliferous limestone some 30 feet from the top of the formation, the division being plainly indicated by a ledge of cantera above the entrance of the mine. Its depth is about 400 feet. This is reached by an irregular descent, the only thoroughfare, in which passage is laboriously effected partly by means of ladders, and partly by footholds in the rock. This main-way occupies a vertical crevice in the limestone, or a series of cavernous partings more or less filled out with workable ores. The widest portions are some 12 feet, the most contracted, not more than six inches. Many of the former are of the nature of drusy cavities lined with quartz and gypsum. The narrower portions have generally yielded paying ore of a decomposed and ferruginous character. The vertical crevice, which has been followed westerly into the hill has been pretty thoroughly wrought. The most extensive workings, however, follow rich partings between planes in the limestone beds, thus giving rise to lateral excavations opening into the main-way. Rich pockets of galena are found both in the crevice and in the horizontal deposits, and these seem to be increasing in depth. Indeed it is chiefly for galena and other plumbiferous ores (*plomosos*), that this mine is at present wrought; and it appears that its ores of all grades (*ayudas*) have always been prized less for their argentiferous qualities, than for the property of facilitating the smelting of the more refactory ores (*resecos*) of the district.*

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* Although over 300 feet below the bed of the ravine, which is dry except in the rainy season, there is no water, or even sensible moisture in the bottom of the mine; and though the deepest mine at present wrought in the region, it is, like all the others, without mechanical appliances of any sort. The ore is spalled underground and brought to the surface on the backs of men and boys whose burdens vary from 100 to 125 lbs., and who make during the day five trips to the bottom. The mine is so excessively warm and badly ventilated as to be almost suffocating; while its passages are so worn and polished by the hands and feet of the miners as to afford the most precarious footing. The carriers (*tenateros*) are surprisingly muscular, nimble and sure of foot, running with swiftness up and down the single

The other workings of this group are on the east side, and further toward the head of the ravine, occupying different beds in the limestone and pursuing productive courses of ore. The principal are *Chiquihuite*, *Rosario*, *Gertrudis* and *S. Lazaro.*

DOLORES GROUP.—The cañon of Dolores is a deep gorge cutting almost perpendicularly the axis of the limestone uplift or boss. The cliffs thus formed, expose on either side a perfect section of bare limestone strata. Its course is very nearly westward, and its head opposite and very near that of the Santo Domingo arroyo. Together the two water-courses describe a triangle, and as they border the mining area impart this shape to it. A lofty ridge capped with the summit cantera divides the two waters. Owing to the comparatively rapid declination of the limestone strata in this direction, the workings toward the head of the cañon are by means of shafts going down just above its dry bed. They are the Tiro Dolores, present Dolores, the "Gauo" (Aguado) and the Vieja shaft.

The *Tiro Dolores* is a vertical shaft starting in the cemented rubble, but soon striking the fossiliferous limestone which, from 200 to 300 yds. below, is uncovered in the bed of the cañon. The shaft is 327 ft. vertical—at which depth a slanting passage, now flooded with rain-water, carries it some 100 feet deeper.

The *Aguado*, like the Dolores, is now inaccessible. It is an irregular sinking of about the same age and depth as the latter, and communicates with it by deep workings. Its mouth is near the top of the limestone formation.

The *Dolores*, present working, is a sinking of the same description as the Aguado, with the lower workings of which, and thereby with those of the older Dolores, it connects. The upper and more accessible workings are still wrought in a small way for plumbiferous ores. The *Vieja*, some 200 yards still further down the arroyo is also an irregular sinking carried to the depth of about 165 ft. It still affords desirable silver ores.

All these mines were formerly diligently wrought—the shafts by means of horse-whims. From a watch-tower on the highest point overlooking the whole district, timely warning of the approach of the savages could be given. Ruins of stone dwellings built strong for defense, indicate a former settled establishment in the bottom of the defile, which, as in the case of all the others in the region, contains water only during rains. The water in the mines appears to have got in from the surface, no percolation being sensible even in the deepest.

In the immediate vicinity of these old workings, near the

notched stem of a ladder, and the shelving, slippery rocks in lieu of steps, but laboring for breath, and their bare bodies bathed in perspiration. No laborers in the world have a more arduous task. The cost of raising the ore in this manner is 25 cents per 350 lbs. This could be greatly reduced, and the mine improved by an opening from the ravine.

head of the arroyo, the dip of the limestone beds to the southeast is near 45°. The vertical axis of the boss is in the eroded neighborhood of the Vieja, at the confluence of another arroyo from the southwest. From this point in every other direction the dips (quaquaversal) are gentle, coming down gradually to not more than five degrees. But the steeper dips toward the outside of the limestone boss have brought up a great thickness of this formation (400 ft.) above the arroyo, and thus west of the Vieja the same strata are above its bed, as could only be entered by shafts east of this point. This is an explanation of the fact that below the Vieja all the workings are above the bed of the ravine, their openings being in the bluffs. These workings are all approximately horizontal: that is, they follow the stratification which on either side being slightly inclined *from* the ravine, gives them all something of a descent *into* the body of the hills.

The *San José* enters the south bluff a quarter of a mile below the Vieja, some 30 ft. above the bed of the arroyo. Its workings extend to several beds, the excavation of which has caused great chambers, while a number of natural caves also have been opened. The ores here are mainly diffused through the limestone strata in ferruginous and the more decomposed portions. They are also found in courses leading from stratum to stratum, but never in the form of a vein.

The *Parcionera* opens near the San José some 70 feet higher up the bluff—its workings, however, descending so as to connect with those of the latter, and thus excavating several beds. It may be described as a series of caverns, both natural and artificial, the largest in the district. It is here that is to be seen the immense one already mentioned. The openings extend about 500 yds. into the hill, in which distance they fall some 150 ft. The pursuit of courses of ore whither they might lead, has caused very irregular passages. The ores of the mine are highly prized, and at the time of my visit were being extracted by twenty miners for the supply of the furnaces of Don Emanuel Escobar. The ores then coming out were plumbiferous. Their mode of deposit does not differ from that of the San José ores.

The *San Matias* is in the north bluff, directly opposite to the San José and Parcionera, and going in on the same level. It is one of the more cavernous, as well as one of the largest and oldest, mines in the district, having been wrought southeastward so as to connect by descending passages with the workings of the Vieja, a quarter of a mile off. It is still wrought by Mateos & Co. Its ores are excessively ferruginous (*colorados*), their color being that of red hematite. According to the owner they are now yielding 12 ounces to the carga ($103 to the ton.)

Several openings in the north bluff have been made at higher levels. One, the *Cuartillera*, is at the height of some 300 feet above the bottom of the cañon. It furnishes a non-ferruginous ore of a drab color.

GUADALUPE GROUP.—It is convenient thus to designate the numerous workings in the same hill with the Parcionera, situated, topographically speaking, some above these mines, and some on the opposite north and west sides, wherever the surface is not too steep to afford easy access.

The *Guadalupe* mine is directly over the Parcionera and San José, and opens from the plateau which marks the top of the limestone formation. Its workings progress southeasterly, descending through several very ferruginous beds in which silver ores seem to be concentrated in "pockets"—the average ferruginous courses being filled with segregated quartz, and generally barren. A drusy cavity is said to have been struck here in 1865 which gave in one day sulphids of silver worth $5000; and five months afterwards another, yielding $1200.

The *Aragon* is a similar working in still higher limestone strata, and very near the junction of this formation with the cantera, here a buff, porphyroidal quartzite, a fine outlier of which rises some 150 ft. above the limestone plateau. Both this mine and the Guadalupe steadily yield plumbiferous ores, carrying, mostly in an invisible form, chlorid, bromid and sulphids of silver.

The other mines of this group are all on the left flank of the limestone ridge. They are the Santa Rita, San Francisco, Purisima, Negrita Grande, Negrita Chiquita, and the Carmen.

The *Santa Rita*, one of the oldest and more reputable mines, is a shelving excavation, starting in fossiliferous limestone, some 350 ft. above the bed of the Dolores arroyo. A large burrow of ferruginous material indicates the extent of former workings. The main opening is said to be asphyxiated, and is now closed, though containing, according to all accounts, ores running as high as four marcs to the carga ($250 to the ton).

The *Purisima*, occupies nearly the same level as the Santa Rita, going down some 60 ft. in heavy bedded limestone, fossiliferous at the surface. The fossils, as usual in this locality, are a coral, *Radiolites*, and fragmentary *Pecten* and *Inoceramus*. The mine, though now vacant, was worked three years ago, and is said to have proved satisfactory.

The *San Francisco* is a sloping excavation in the hillside, and some 100 ft. lower in the limestone than the Santa Rita. The workable portions of the limestone beds closely resemble those of the workings in the Dolores arroyo. The mine is now supplying highly prized ores to the Chihuahua Company's hacienda.

The *Negrita Grande* is an old, now inaccessible, shaft, more than 250 ft. deep, which depth about corresponds to the level of the arroyo immediately below the mouth of the mine, and from which it would be practicable to reach its workings. The shaft was formerly worked by horse-whims (*malacates*)—probably by the distinguished Bustamente who is reputed to have had a *bonanza* from it of $60,000, said to have been in the form of concentrated chlorid of silver.

The *Negrita Chiquita* is a newer open working, now operated by Don Jesus Mateos, and occupying strata but a little lower than the mouth of the shaft of the *Negrita Grande.* The *Carmen* occupies beds somewhat higher, and is a similar cavernous excavation.

It will be understood that notwithstanding wide differences in topographical level, all the workings above mentioned are embraced within the same set of beds whose combined thickness does not exceed 450 ft.; that the shaft workings at the head of Arroyo de Dolores, the mines of the Santo Domingo arroyo, and the workings on the top of the plateau forming the surface of the limestone uplift, *all have their entrance at about the same stratigraphical horizon; that is, near the top of the limestone.* And it is to be borne in mind that stratigraphically the lowest workings are *not* those which topographically are the lowest, but the San José and San Matias instead of the Dolores, Vieja and the Aguado.

Notwithstanding the number and size of the excavations in the mining ground of Sta. Eulalia, and the large returns which these have afforded, its future prospects seem scarcely impaired by the achievements of the past. By the modern scale of mineral industry, these might pass for only a thorough exploration—in its assurances worth all that it has yielded. The location of the mines has been determined by the accidents of the surface rather than by promising outcrops—a foothold upon the surface seeming to have been all that was necessary. Besides the great amount of unbroken ground left in and amidst the established mines, a large body of the limestone strata remains untouched, especially to the north of the Dolores arroyo, where the inner ends of the workings, proceeding from the bluff on that side, show no deterioration or diminution of the ores. It is not venturing too much to predict that the past record of Sta. Eulalia will be far surpassed at some future time by its development prosecuted by an enlightened practice.

Yield.—Extracted according to the judgment of the miners who are very expert in the detection of familiar ores, the ores after being spalled, run from 4 to 6 oz. to the carga ($34·46 to $51.60 per ton of 2000 lbs.) Four-ounce ores are abundant in all of the mines, but scarcely pay for working by the present smelting practice. By care in selection, this grade is easily brought up to five and six ounces to the carga. This is the lowest working yield of all the smelting operations—the average being something above this. Don Jesus Mateos, one of the most experienced operators in the district, by using a mixture of the 12 oz. ores from the San Matias and 6 oz. ores of the San José, obtains never less than 6 oz. to the carga, generally as high as 8 oz., and sometimes 9 and even 10 oz.

Cost of ore.—The cost of ore delivered at the mouth of the mine varies according to the expense of raising. At the Parcionera, which may be taken as the type of horizontal or sloping workings, this cost, which includes all mining expenses, is stated at $1.50 per carga. At the Santo Domingo, the cost is greater on account of the laborious raising. The ores are delivered at the furnace for 20 to 37½ cents per carga, the donkey-load being one carga (300 lbs.)

Reduction.—The furnace employed in the district is the common Mexican *adobe horno*, a blast-furnace 47 inches high, 18 in. wide at the top, slightly tapering toward the bottom, and 16 in. across. The blast is supplied by hand bellows, the nozzle of which is in the back, 8 in. from the bottom. In the better constructed establishment of the Chihuahua Co. the bellows are set in motion by mules. The charge of the furnace varies according to the notion of the smelter as to the requirements of the different ores. Mateos uses 75 lbs. of spalled ore to 20 lbs. of litharge, and 12 to 25 lbs. of old slag (*grasa*) by way of flux. The charge of litharge varies with the ore. Plumbiferous ores, like those of Santo Domingo, Santa Gertrudis, Dolores and San Antonio, give an excess of litharge, and hence are in especial favor for mixture; while the excess of litharge obtained is sold out of the district at the rate of 8 to 16 dollars per carga ($2.67½ to 5.32½ per cwt.) The cupellation is done in the ordinary *adobe vaso*, one serving three blast furnaces, or treating 20 cargas of argentiferous lead per week.

Fuel.—The question of fuel is one of paramount importance to the industry of the district. The *mesquite* root is the only indigenous fuel of the immediate section of country, forest trees being entirely unknown east of the humid belt, 50 miles to the west of Sta. Eulalia, except the cotton wood (*alamo*) which is cultivated for shade. Yet nothing could excel this root as a fuel, either as it comes from the ground, or after conversion into charcoal. A single shrub generally gives near a cord of heavy root. In the neighborhood of Sta. Eulalia, the *mesquite* has long been exhausted by the draft upon it from there, and from the city of Chihuahua. Oak charcoal is brought 30 leagues to Sta. Eulalia from Mapula, and sold at the rate of 75 cents to one dollar per quintal (100 lbs.)—the lower price prevailing whenever the roads are favorable. Oak wood is likewise brought thither, and sold at the rate of one dollar (copper) per carga (80 sticks, 28 by 4 in.). The same prices prevail in the city of Chihuahua. Mesquite charcoal from the east side of the Sta. Eulalia Mts. is delivered at the village for $1 to $1.50 (copper) per quintal. These prices, already high, would doubtless steadily advance under a larger and more pressing demand, such as would be created by an extensive smelting industry depending upon a *certain* supply and *limited* price of fuel, without control over either.*

With a cheaper mode of reduction, the cost of production would be very considerably lessened by rendering available ores which, though cheaply and largely broken, are now rejected —ores yielding as high as $34 to the ton. This condition, rather than the introduction of mechanical appliances, would necessarily bring this cost below the cost of extraction of silver ores from expensive workings in Nevada, though now far exceeding that of most of the deep mines of the Comstock lode. But the cost of reduction is even more excessive and out of proportion to the value of the ores. Though the Mexican smelting practice is attended with a smaller loss of silver, the fact that Nevada ores returning no more than $15 per ton can be worked by amalgamation with some profit, more than offsets the loss of 20 to 25 per cent to which all are subjected, as a large and regular business generally depends upon the availableness of the ores of low grade—always predominating in extensive deposits. Yet the Mexican furnace by no means extracts the whole of the silver as may be seen by picking up from any ancient or fresh slag heap fragments containing numerous globules of argentif-

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* Expressing in familiar units the above rates imparted to me by Don Jesus Mateos, we have in a form for comparison with working results elsewhere obtained by different modes of reduction the following exhibit:

Cost of Production.

Cost of ore at mouth of mine, per ton, at $1.50 per carga,	$10.00.	
Transportation to furnace, " " 0·37½ "	2 62½.	
		12.62½

Cost of Reduction.

Charcoal in blast furnace, per ton of ore, at $1 per quintal,	$10.62½	
Wood " *vaso*, cupelling, " " " $1 per carga,	2·66½	
2 Smelters at $1. per diem, 1 Refiner at 1. " - 6 Helpers at 0.37½ " } Labor,	4.92	
Litharge, repairs, etc.,	1.00	19.21½
Total cost of production and reduction,		$31.84

Having adopted the mean of variable rates, no allowance is made for the discount of copper currency (at the time of my visit, in the winter of 1869—33⅓ per cent; but now much greater) in which are stated the prices of fuel, especially as the above exhibit bears out the representation of Don Jesus; namely—that in the failure of the 50 cargas of ore smelted weekly by one furnace to yield a total of thirty marcs of silver, that is (taxes paid) $32 per ton, loss is incurred.

erous lead, and otherwise indicating imperfect reduction. Fuel is far more costly at Virginia, Neveda, than anywhere in Chihuahua, but this difference follows from the higher price of labor there—the supply of fuel really being greater than at Santa Eulalia.*

Thus will be seen the mistake of treating the ores of Santa Eulalia by a practice which is so costly as to render unavailable the great bulk of them, and to absorb almost the whole value of even the choice ores in their reduction, when by a cheaper practice the whole run of the ores could be treated with profit, and the industry improved and expanded in all respects. I allude to amalgamation as practiced in Mexico itself, and which the climate, labor and facilities of the country especially favor. In 1846, it was estimated by Mr. John Phillips, seven-eighths of the silver produced in Mexico was obtained by amalgamation.

The lack of surface for *patios* at Sta. Eulalia, together with the scantiness of water, are circumstances sufficient to account for the prevalence there of smelting, notwithstanding the ores are of a nature to yield readily to patio amalgamation. That these difficulties have never been surmounted is to be ascribed to the isolated condition of this section of country, and its lack of facilities for extralimitary supplies. In the last century, during the period of its prosperity, as I learn from a manuscript in the state archives, amalgamation both by *patio* and *cazo*, was carried on at Sta. Eulalia and Chihuahua to the extent of keeping in operation in the two places 72 drag-mills (*tahones*), and 6 stamp-mills (*morteros de agua y caballerias*). The reduction works, just erected, of the new Chihuahua Co. have not in the least departed from ancient models. Nor would a change of practice be warranted by anything short of an extensive undertaking. In view of the scarcity of water and fuel at Santa Eulalia, it must be seen that present operations without modification can scarcely be extended beyond their present scope. As long as they are thus limited, they are favored by the choice of ores, and by cheap labor. By dispensing with mechanical appliances for dressing ores any considerable outlay is avoided.

The silver deposits of Santa Eulalia, however, are so superior and extensive as to warrant their extraction and reduction on a large scale. This will be practicable by having recourse to the facilities afforded by one or both of the two plains on either side of the Sta. Eulalia mountains.

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* Notwithstanding the rates of labor in Nevada are more than treble those of Santa Eulalia, the ores of the Comstock lode are extracted for a half to a third less, and reduced for less than a half cheaper—their average yield ranging in different mines from $28 to $43. To properly contrast the wide difference between the economical conditions of the industry taken as a whole in the two localities of Sta. Eulalia and Virginia City, it is necessary to consider that of the latter as involving an immense outlay for milling and deep mining, altogether beyond the requirements of the former locality. It is also obvious that a high cost of fuel would, the same as at Santa Eulalia, preclude the practicability of working by smelting any but *selected* ores, and thus confine the Comstock industry within comparatively narrow limits.

West of the mountains, superior facilities for the dressing of ores, and for patios, are to be had at Tabalopa on the Sacramento river, at the distance of some eight miles from the mouth of the Arroyo Dolores. Ores could be delivered at this point by wagon at near the same rate that they are now freighted on mules over the mountains to Santa Eulalia. This way out to the plain would be by the ravine, and thence the whole way to Tabalopa by a down grade.

Having extended my observations but a little way east of this gorge, I am not prepared to determine the question of an exit on that side of the Sta. Eulalia range. Should it be found practicable to cheaply deliver ores in the Conchos valley, this side would, on the whole, present superior conditions for reduction works, provided a good water supply can be had, which is probable, as the plain is already thoroughly irrigated. Fuel (mesquite) is far more abundant here than on the Chihuahua side, and the position is nearer by two days to all supplies drawn from Texas.

New York, Jan. 1, 1870.

Art. XXI.—*Machinery and Processes of the Industrial Arts, and Apparatus of the Exact Sciences;* by Frederick A. P. Barnard, LL.D., United States Commissioner to the Paris Universal Exposition.

If the imperial decrees of June, 1865 and subsequent dates, which gave origin to the Paris Exposition that opened on the first of April, 1867, were grand and comprehensive in their scope and intention, they were equally illustrious in the success and completeness of their execution. More than fifty thousand exhibitors brought together on that occasion several millions of objects, all different and all deserving attention. These were gathered from some forty nations and countries, covering the entire civilization of the globe. Taken together they formed a representation of the art, the industry and the inventions of mankind, and of every ideal yet realized for the comfort, the elegance, the operative power, and the progress of the human race. From the material to the implement the completed structure or the machine; from the staple to the fabric; from the massive and gigantic—the products of Cyclopean forges or Titanic engines—down to the infant's toy and the gossamer vestment; from the ponderous gearing to the tiny time-piece all were spread out, an accumulation of series upon series, and every series showing an almost complete and continuous gradation.

The field of the Exposition was the parade ground, or Champ de Mars, easy of access, and supplemented by annexing the island of Billancourt in the Seine, together comprehending an area in excess, by a few acres, of one-fourth of a square mile. Spacious grounds outside the building, or *palace* as it was called, but without much very palatial in its aspect or construction, were allotted to 'structures that ranged from the nomadic hut of an Esquimaux to the gilded palace of the Sultan,' and to such living, or agricultural, or other objects and groups, as could not suitably be admitted within. In deference, we suppose, to French propensities, the number of groups was just ten; but, by a marvellous aberration, the number of subdivisions or classes was but ninety-five. Seven of these so classified groups were arranged in seven oval galleries, one story high, successively surrounding one another, and divided systematically into commodious apartments, the entire assemblage of which comprised an area of forty acres and a circuit of almost a mile. In the center was a pavilion holding sets of the various national coins, weights and measures, and surrounded by a florally ornamented space from which sixteen avenues radiated to the outer space or circumference of the palace. In the sixteen sections thus formed the various nationalities had their products and groups arranged in a series, and all the series corresponding to one another from circumference to center. By passing around the galleries the spectator would come in succession upon similar groups of the different nationalities; while by passing along the avenues he would take successively the various groups of any and every nationality, each through its own entire series. Accordingly a tour of ten miles judiciously disposed, was enough to bring to an observer's attention the entire aggregate of groups within the building. By such means therefore, the best practicable facilities were supplied to each nationality to exhibit its own products to advantage, at once in union and in comparison with the like of other countries, while also the one 'special jury' and the ninety-four 'juries of classes,' for awarding testimonials and degrees of merit—the *prizes*, the *medals* and the *honorable mentions*—and, with them, the ten 'juries of groups,' for revising these awards under the final revision of a 'superior council,' all had the most ready and commanding observation of the objects composing the entire exhibit.

Were it a possibility to continue in permanence such an exhibit—a single area into which all the national arts were gathered and concentrated, like differently colored lights in the focus of an object-glass—there might thus be afforded to visitors an ever accessible telescopic vision, as it were, of the various civilizations of the earth, although ever so remote or scattered. But, even then, the visitors, however great their multitude, could never be expected to comprise more than a small fraction of the thinking and inquisitive classes of society. The best substitute therefore which the case admits of plainly seems to be of the sort adopted and carried out by the Federal Government for the use and satisfaction of these United States; that is to say, a well appointed and sufficiently numerous commission to witness, examine and report upon the Exposition and the groups and classes of objects therein represented; and the Reports to be printed by the Government for general distribution. Besides the primary 'General Survey of the Exposition,' from the pen of Charles B. Seymour, chairman of the Committee, instructed for that purpose by the Department of State, and its preface by N. M. Beckwith, U. S. Commissioner General, and President of the Commission, the Reports are before us, so far as their publication has been completed. By far the most voluminous, various and extended of the number is that of President Barnard, the title of which is employed above as an introductory heading to these remarks. It is an octavo of 650 pages, filled with descriptions, illustrations and theoretic determinations, illustrated with numerous clearly executed cuts on the pages, and concluded by eight elaborate and finely wrought engravings. The finished artistic execution of this and of all the volumes or reports is obviously due, in no small measure, to the taste, good judgment and accuracy of Prof. W. P. Blake, who superintended the publication, as its editor. It sets off to advantage the intrinsic value and eminent adornments of the report,—both the matter and the author's style of rendering it. Next to the privilege of one's own presence amid the immense and diversified assemblage of objects in the Exposition, is the experienced satisfaction and advantage of finding one's self in presence of a large collection of the most select and suggestive of the number, brought into one view through the medium of luminous, graphic and illustrated descriptions. The author not confining himself to mere delineation, as he passes from object to object, takes pains to instruct us in their subsisting mutual connections and dependencies, and to explain in each the distinctive uses of parts, the modes of general operation, and the rationale of effects. Often indeed, if we will stay to go deeper, he puts at our disposal his well understood powers of analysis, dissipates the fallacies which becloud the mechanician's conceptions, and reconciles those paradoxes which, to the popular mind, often carry the semblance of an inevitable discord between those two wedded and indissoluble partners, Theory and Practice. And we attend to these all the more earnestly that our author's opening chapter had impressed us, as at the vestibule and entrance of our survey, with the intimacy and imminence of 'The relation of invention to industrial progress.' That model chapter traces and exhibits with unwonted distinctness the dependence of art upon other art, and of art upon its own created auxiliaries: thus the steam engine first put within the workman's reach those operative and refined tools to which the steam engine itself owes its progressive improvement and existing completeness. In following out the like connections and dependencies we are also led to perceive, not indeed for the first time but with intensified vividness, how deeply art and invention project their influences into economic and social life, into trade and commerce, into civilizing effects abroad, into the habits of thinking and feeling at home, and even into politics and the nations's destiny. Thus, the cotton-gin expanded itself into a prime mover not only of all-pervading activities in the manufactory and on the ocean, but even of the vast combinations and issues themselves of war and peace. Here is no exaggeration; still we conceive, with our author, that not the individual inventor alone is to be recognized as the originating instrumentality of all this, except as one which the age itself had brought up into preparation for his invention, and in whom the invention sprang up because the age had created a demand for it.

The dependencies of industrial art may be spoken of in various senses,—*massively*, for instance, as resting upon force, its modifications and applications,—or *quantitatively*, as resting for the abundance and multiplication of its products, upon facile machinery, manipulation and skill,—or *inductively*, as resting upon knowledge and ingenuity for its origination and progress. A glance at the table of contents and headings of the chapters of the report, will suggest how considerably some such natural classification, whether or not so intended, has influenced our author in the selection of his subjects and in their arrangement. Of the eighteen chapters, five are occupied with the subject of *power* in its various mechanical relations; as, first, the prime movers, steam, air, water, electricity and their enginery; next, the transmission of power, then its accumulation, then its measurement and registration, and, lastly, its massive applications. The eight middle chapters are taken up mainly with the appliances, the methods and the instrumentalities of ingenuity, tact and sagacity, as used for the purpose of *production*, and acting through all the range and agencies of heat, cold, excitation, pressure and motion,—sometimes the production of improved material, and sometimes the production of improved effects, or of articles and implements for ornament and use, in their variety, their finish, and their aptitudes, as well as the rapidity and multitude of their manufacture. The concluding four chapters are occupied with the 'exact sciences' considered in their mechanical and practical relations. We read with wonder of the success with which measurements are carried to an exactness well nigh rivalling, to all intents, the mathematical itself,—of rods ascertained to a millionth of an inch in length, and of balances that perform to a twenty-millionth part of their burden. We are delighted to trace anew the progress by which the human eye has become enabled, through the perfection of object-glasses enormous in dimension, on the one hand, or of minute sizes on the other, to penetrate into the universe outwardly, and into the bosom of nature inwardly. We are let into the surprising secret by which mechanism is made to render numerical computations with greater exactitude and with immeasurably greater promptness than the mental faculties of the computer himself could realize. We are instructed how to make sound itself a thing of visible undulations, and how to apply a measuring scale to their continuance. Not much, it is true, of the Exposition, as the author himself is careful to say, was absolutely new, as respects *elementary* inventions; and, therefore, not much of that quality and essence could have place in any faithful account of it. The report is simply, and admirably, a representation by just and varied selections, of the stage of progress which industrial art and invention had reached in the year 1867. Although necessarily partial it still embraces a collection ample for its purpose; that is to say, for giving a near birdseye view of the Exposition, and, like the walks and avenues of the Exposition itself, diversified by occasional fragments of history and relics of legendary lore. Taken together it forms a resort where the engineer may obtain reassurance, the mechanic experience, the inventor impulse guidance and correction, and the man of intellect or of letters choice entertainment. The report is printed as a public document, and has not been ordinarily obtainable otherwise than by and through members of the United States Senate, each of whom, as we are informed, had the distribution of some fifteen entire series, each corresponding to the one of which this report is a constituent.

The United States was not largely represented, as respects numbers, in its own section of the Exposition. The country was too much occupied with new and extraordinary domestic questions. Industry was unsettled in many respects. The subject had not attracted the earliest attention; and the field was very distant. Yet our national exhibit, although not extended in scale, was excelled in quality only by France itself—and that in no very considerable degree—while the percentage of awards relative to the number of exhibitors was double the corresponding percentage for England and her colonies.

No extracts which our space would allow could do justice to the report, especially if not accompanied by the cuts or engravings; and if we were to instance and enlarge upon passages and descriptions that appear specially important, it would convey no conceptions to the reader comparable with what he will obtain by a reference to the work itself. We only add, in justice to the distinguished author, that, while he is thoroughly American in a due anxiety to set forward the claims of his countrymen, it is with an obvious impartiality toward all the competitors, and with a diligence whose regulating principle is truth and an endeavor for the benefit of mankind in general.

ART. XXII.—*On Norite or Labradorite Rock;* by T. STERRY HUNT, LL.D., F.R.S.

[Read before the American Association for the Advancement of Science, at Salem, August, 1869.]

THE various rocks composed essentially of a triclinic or anorthic feldspar, with an admixture of hornblende, pyroxene, hypersthene or diallage, have by lithologists been designated by the names of diorite, dolerite, diabase, hypersthenite and gabbro, among others. The latter name has by many been regarded as synonymous with euphotide. I however pointed out many years since that the true euphotide is not a feldspathic rock, but consists of a mixture of diallage with saussurite, a white heavy silicate apparently identical with zoisite. By an admixture of labradorite or an allied feldspar, however, euphotide passes into the so-called gabbro, which I have defined as a diallagic diabase (this Journal, II, xxvii, 336), and which is closely related to norite. The name of hypersthene rock or hypersthenite (sometimes contracted into hyperite), was given by MacCulloch* to a rock consisting of labradorite, or a related feldspar, and hypersthene, found by him in the Western Islands of Scotland, and subsequently recognized by Emmons in the Adirondack Mountains of northern New York. By both of these observers it was regarded as an erupted rock. In 1851 I detected it among the Laurentide hills of Canada, where, as in New York, it extends over considerable areas. Farther examinations of this rock in place showed that though hypersthene, generally in very small proportion, is a frequent element, it is often replaced by a green granular pyroxene, and still more often both of these are wanting, so that we have a rock composed almost entirely of a triclinic feldspar, whose composition is generally near that of labradorite, but varies in different examples from that of andesine to near that of anorthite. To these rocks I provisionally applied the name of anorthosites, the pure feldspathic type being regarded as normal anorthosite, associated with which, however, were to be found hypersthenic and pyroxenic varieties. Red garnet, epidote, a black mica, and more rarely dichroite and quartz, are all occasionally found sparingly disseminated in these anorthosites of New York and Canada, which cannot be distinguished from those first observed by MacCulloch in the Isle of Skye, as I have convinced myself by an examination of the specimens there collected by him, and now preserved in the collections of the Geological Society of London. Titaniferous iron ore (menaccanite) also frequently occurs in grains and masses in these rocks, both in Skye, and in North America, where it sometimes forms beds or masses of considerable size. Details as to the chemical and mineralogical characters of these rocks will be found in the L. E. & D. Philos. Magazine for May, 1855, and also in the Geology of Canada, 1863, pages 588–590.

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* MacCulloch, Geology of the Western Islands, i, 385–390.

The subsequent investigations of Sir William Logan have shown that these anorthosites in Canada belong to a great series of stratified crystalline rocks which by the geological survey of Canada have been designated the Labrador or Upper Laurentian series, and which repose unconformably upon the older or true Laurentian gneiss and limestones. The area of the Labrador formation most examined lies in the counties of Argenteuil and Terrebonne, to the north and northwest of Montreal, and has a breadth of more than forty miles. It is however met with on the northeast shore of Lake Huron, according to Dr. Bigsby,* and at several points below Quebec, notably in the parish of Château-Richer, at Bay St. Paul, and around Lake St. John on the Saguenay, where it occupies a large area. Proceeding northeastward along the left bank of the St. Lawrence, Mr. Richardson has lately observed it at the mouth of Pentecost river, about 160 miles below the entrance to the Saguenay, and I have found it forming the shore of the Bay of Seven Islands forty miles farther down. This area is probably connected with the wide extent of this rock observed by Prof. Hind on the river Moisie. In all of these regions it appears to be surrounded and limited by the ordinary Laurentian gneiss. Bayfield, moreover, describes a rock with a base of labradorite as forming the coast for several miles near Mingan. Finally, it is widely spread on the coast of Labrador, where its characteristic mineral was first found, and from whence it takes its name.

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* Geology of Canada, 1863, page 480.

Prof. A. S. Packard, Jr., has given us valuable information with regard to the occurrence of labradorite rocks at some points on the Labrador coast.* One of its localities is at Square Island, just north of Cape St. Michel, where the rock consists chiefly of crystalline labradorite smoky gray in color, translucent, and opalescent with greenish reflections. This feldspar often shows cleavage planes two inches broad, and is associated with a little vitreous quartz and with coarsely crystalline hypersthene, which appears in relief on the weathered surfaces. This labradorite rock, according to Prof. Packard, is surrounded by and probably rests upon Laurentian gneiss. At Domino Harbor he found domes or bosses of a similar labradorite resting upon strata which consist in great part of a slightly schistose quartzite, having for its base a granular vitreous quartz, and enclosing grains of black hornblende, more rarely hypersthene, black mica, and red garnet. Feldspar is generally wanting, but in some parts these quartzites become gneissic, and they were nowhere seen in uncomformable contact with the Laurentian gneiss of the vicinity. These quartzose strata Prof. Packard refers, with some doubt, to the Huronian system. The minerals which they contain are not however met with, so far as known, in the Huronian quartzites, and on the contrary, are very characteristic of the quartzites of the Laurentian system, which attain a great thickness in many parts of its distribution. The overlying domes of labradorite rock, which Prof. Packard was inclined to regard, in this case, as erupted through Huronian quartzites, are probably nothing more than outlying portions of the newer Labrador formation resting upon the Laurentian strata, as already observed by him at Square Island. Along the western coast of the island of Newfoundland Mr. Jukes observed at Indian Head and at York Harbor dark colored rocks composed of labradorite and hypersthene, and others of albite (?) and hypersthene, which may probably be found to belong to the Labrador series.

Rocks composed chiefly of labradorite or a related feldspar greatly predominate in the Labrador series, but these, at least in the area near Montreal, which is the one best known, are interstratified with beds of a kind of diabase in which dark green pyroxene prevails, with crystalline limestone similar in mineralogical characters to that of the Laurentian system, and more rarely with quartzites and thin beds of orthoclase gneiss. I have more than once insisted upon the rarity of free quartz and the general basic character of the rocks in this series, an observation with which I am credited in Dana's Manual of Geology (p. 139), where it seems to be applied to the whole of the rocks there classed as Azoic, including the Laurentian, Labradorian and Huronian systems. It is, in fact, remarkable that the silicated rocks of the latter two consist chiefly of labradorites, diorites and diabases; gneissic and granitic rocks being exceedingly rare among them, though quartzites abound in the Huronian. In the Laurentian system, on the contrary, though basic silicated rocks are not wanting, orthoclase gneisses, often granitoid in structure, and abounding in quartz, predominate.

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* On the Glacial phenomena of Labrador and Maine. Mem. Bost. Acad. Nat. Hist., vol. 1, part ii, pp. 214–217.

The anorthosite rocks of the Labrador series present great variations in texture, being sometimes coarsely granitoid, and at other times finely granular. They not unfrequently assume the banded structure of gneiss, lines of pyroxene, hypersthene, garnet, titanic iron ore or mica marking the planes of stratification. Probably three-fourths of the anorthosites of this series in Canada, whether examined in place or in the boulders which abound in the St. Lawrence valley, consist of pure or nearly pure feldspar rocks, in which the proportion of foreign minerals will not exceed five hundredths. Hence we have come to designate them by the name of labradorite rock. The colors of this rock are very generally some shade of blue, from bluish-black or violet to bluish-gray, smoky gray or lavender, more rarely purplish passing into flesh red, greenish-blue, and occasionally greenish or bluish-white. The weathered surfaces of these labradorite rocks are opaque white. The anorthosites which occupy a considerable area in the Adirondack region, as described by Emmons in his report on the Geology of the Northern district of New York, and as seen by me in hand specimens, closely resemble the rocks of the Labrador series in Canada.

In all of these localities the coarse or granitoid varieties often hold large crystalline cleavable masses, generally polysynthetic macles, and frequently exhibiting the peculiar opalescence which belongs to labradorite. Although rocks composed of labradorite or similar feldspars, with hornblende or pyroxene, occur in various other geological formations, both as indigenous greenstones and as erupted masses, they never, so far as my observation in North America goes, exhibit the peculiar character just described; namely, that of a granular or granitoid rock composed of nearly pure labradorite or some closely related feldspar, frequently opalescent, and generally of a bluish color, often violet, smoky blue or lavender-blue. This type of rock seems in North America to characterize the Labrador series.

It may here be remarked as an interesting fact bearing on the distribution of the Labrador series, that two large boulders of labradorite rock, one of the beautiful dark blue variety, are found on Marblehead Neck on the coast of Massachusetts. It does not seem probable that these masses could have been derived from any of the far off localities already mentioned, and the fact that the gneiss of eastern Massachusetts is, as I have recently found, in part of Laurentian age, suggests that an outcrop of the Labrador series may exist in some locality not far removed. In this connection it may be added that I have lately found characteristic labradorite and hyperite rocks in southern New Brunswick, a few miles east of St. John, occupying a position between the Laurentian and the Huronian or Cambrian rocks, which there make their appearance, accompanied by Lower Silurian strata, to the south of the great Carboniferous basin of the region. This interesting locality was recently pointed out to me by Mr. G. F. Matthew of St. John, to whom we are indebted for a great part of our knowledge of the geology of southern New Brunswick. Chester and Bucks counties in Pennsylvania, and the Wichita Mountains in Arkansas, are cited in Dana's Mineralogy as localities of labradorite, but as I have never examined specimens from these places, I am unable to say whether they resemble the characteristic anorthosites of the Labrador series already described.

The name of norite, in allusion to Norway, was given by Esmark to a rock composed chiefly of labradorite, which is found in several localities in that country.* I had already noticed the close resemblance between two specimens of norite obtained from Krantz of Berlin, and the labradorite rocks of North America just noticed, when in 1867 I had the opportunity of examining at the Universal Exhibition at Paris, a collection of Norwegian rocks selected for ornamental purposes, exhibited by the Royal University of Christiania. Prominent among these was a series of the norites, which could not be distinguished from the labradorite rocks of the Upper Laurentian or Labrador series of this continent. In a printed note accompanying this collection from the University it is said that the numerous varieties of rocks consisting of labradorite with hypersthene, diallage and bronzite, have, in the geological map of Southern Norway, published at Christiania in 1866, been designated by the common name of gabbro. This note at the same time suggests that "the name of norite should be preserved for certain varieties of gabbro rich in labradorite, which varieties may in great part with justice be called labradorite rock, since labrador feldspar is their predominent element." With this excellent suggestion I heartily concur, remarking, however that the name of gabbro, as an ill-defined synonym for certain anorthosite rocks, including in part diorite, diabase, hyperite, and even confounded with the non-feldspathic rock euphotide, may very well be dispensed with in lithology.

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* See farther Zirkel. Petrographie, II, 131.

By referring to the geological map just mentioned, it will be seen that these so-called gabbros occupy considerable areas in the Laurentian gneiss region of Norway. By the authors of the maps, Messrs. Kjerulf and Dahl, these gabbros are regarded as eruptive, though they are described at the same time as often assuming the character of stratified rocks. It should however be noticed that these geologists go so far as to regard the whole of the granitic gneiss of the region as unstratified and of plutonic origin.

The specimens of these norites exhibited in Paris were in blocks polished on one side, and as was observed in the note accompanying them, presented a curious resemblance to certain varieties of marble. It is worthy of remark that Emmons in his report on the Geology of the Northern District of New York, suggested the application of the labradorite rocks of Essex county as a substitute for marble (pages 29, 418). An ornamental vase of the same rock turned in a lathe with the aid of a black diamond, has been in the Museum of the Geological Survey of Canada since 1856.

Of the collection of norites from Norway the specimens from Sogndal and Egersund presented fine varieties of grayish or brownish violet tints, while a dark violet norite came from Krageroë and also from the islands of Langoë and Gomoë, and a white granular variety from the gulf of Laerdal in the diocese of Bergen.

It is only in rare cases that the cleavable feldspar of these norites exhibits the peculiar opalescence which distinguishes the finer labradorite found in some parts of the coast of Labrador. Opalescent varieties of this feldspar are however occasionally met with in the area near to Montreal, and in northern New York. In the Paris Exhibition of 1867 there were exhibited from Russia, large polished tables of a beautiful violet colored granitoid norite, portions of which exhibited a fine opalescence. This rock, I was informed, comes from a mountain mass in the Government of Kiew, but of its geognostical relations I am ignorant.

These peculiar labradorite rocks, presenting a great similarity in mineralogical and lithological character, have now been observed in Essex county, New York, and through Canada at intervals from the shore of Lake Huron to the coast of Labrador. They are again met with in southern New Brunswick, in the Isle of Skye, in Norway, and in southwestern Russia, and in nearly all of these localities are known to occur in contact with and apparently reposing like a newer formation upon the ancient Laurentian gneiss. Giekie in his memoir on the geology of a part of Skye,* appears to include the norites or hypersthenites of that island with certain syenites and greenstones, which he describes as not intrusive, though eruptive after the manner of granites (loc. cit., p. 11–14). The hypersthenites are represented in his map as occurring to the west of Loch Slapin. Specimens in my possession from Loch Scavig, a little further west, and others in MacCulloch's collection from that vicinity, are however identical with the North American norites, whose stratified character is undoubted. I called attention to these resemblances in the Dublin Quarterly Journal for July, 1863,* and Haughton, who in 1864 visited Loch Scavig, has since described and analyzed the rock of that locality, which consists of labradorite, often coarse grained, with pyroxene and menaccanite, and is evidently, according to him, a bedded metamorphic rock (Dublin Quar. Jour., 1865, p. 94). He, it may be remarked, designates it as a syenite, a term which most lithologists apply to rocks whose feldspar is orthoclase.

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* Quar. Jour. Geol. Soc., xiv., p. 1.

I desire to call the attention of both American and European lithologists to this remarkable class of rocks, of which the norites may be regarded as the normal and typical form, in the hope that they may be induced to examine still farther into the question of the age and geognostical relations of these rocks in various regions, and to determine whether the mineralogical and lithological character which I have pointed out are geological constants.

ART. XXIII.—*On the cause of the color of the Water of Lake Leman, Geneva*; by AUG. A. HAYES, M.D., Assayer to State of Massachusetts.

THE traveler, who enters Switzerland at Geneva, always has his attention arrested by the beautiful azure color which the water of Lake Leman presents, especially when, as one looks into its depths, the color is in contrast with the white reflection of clothing below the surface, at points where the laundresses pursue their avocations.

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* I, at the same time, called attention to the Laurentian aspect of the crystalline limestones of Iona, which I found in MacCulloch's collection. Limestones not unlike these occur in Skye, intermixed with serpentine, and are, according to Mr. Giekie, associated with the protruded syenites of that region. With all deference to the authority of that eminent geologist, I cannot help suggesting that a re-examination of the district would show that the highly-inclined metamorphic crystalline limestones, holding serpentine, and associated with syenitic rocks, belong to an older system (probably Laurentian), and are thus distinct from the nearly horizontal fossiliferous liassic limestones near by, which are only locally altered by intrusive rocks. American geologists will at once recall the mis-conception which led most of our best observers during many years to look upon the old Laurentian limestones of New York and New Jersey as altered portions of the overlying paleozoic strata.

Many will remember the expression of Sir H. Davy, that "this color is doubtless due to some compound of iodine," and not only chemists, but geologists have speculated on the cause of this coloration, quite frequently with opposite opinions.

Both in composition and general characters, this water belongs to a class I have been accustomed to describe as "White waters," or colorless waters, which, chemically considered, always hold a salt called crenate of lime in solution, and mark in their color the existence of lime salts, in their course as rivers, or in contact with them, as reposing in true lakes. At many points in Switzerland these white waters are found, and indicate clearly the occurrence of calcareous matter, more extensively diffused than surface observations of the rocks would allow in some districts.

Among the Tyrolese Alps, too, lakes and streams present the same cerulean hue, which gives character and beauty to the water of Lake Leman, and I have seen even small collections of water there apparently as highly colored as the more noted lakes. "White waters," when clear, always present this hue, and when turbid, a green hue, due to reflection from particles of solid matter. In the summer of 1858, I commenced a careful analysis of the water of Lake Leman at Geneva, and had found a general accordance with the published statements of composition; but the special search for coloring matter, was prevented by the inopportune recurrence of rain, and subsequent disappointment at a point on the Rhone above the lake.

Since that time this water has been analyzed by distinguished chemists, such as Will, Dafour, and Blanche, whose results only show the presence of more salts in number in the solid matter, and the slight variations in proportions which we should expect would occur, under variations of volume and temperature, in the water at different seasons.

These analyses, I learned, were made in the usual way of evaporating the water for the solid products, and driving off and collecting the gases, a method which enables us to answer many questions, but does not permit the nicer determinations of organic bodies, if present, in a natural or an altered state. Analysis so conducted would not allow us to answer the question in relation to the cause of color, unless it could be traced to some known or unknown fixed compound. Nor would such an analysis determine the weight accurately of the whole amount of matter, naturally dissolved in the water, as it is easy to prove that a loss of matter takes place during the evaporation, even if conducted at a moderate temperature—the great point of state of hydration of the organic salts, and to some extent that of the saline matter, is ignored entirely when we proceed in this way to determine the quantity and kind of constituents.

Nearly all waters contain living organisms, and their germs, these matters, in a decomposing state, organic acids, either with or without bases, in the form of salts, most easily changed by heat or even by concentration. These substances are very important constituents, in connection with the uses of the water, and I could offer many illustrations of damage in manufacturing and unfitness for consumption, traceable directly to the presence of such bodies in waters otherwise desirable, and proper for extended consumption.

The mode adopted in my examination of the water of Lake Leman is that which I have usefully applied in a large number of cases, and with modifications, it is applicable to all waters, in connection with other well known steps of analysis.

A large volume of the water of Lake Leman was mixed with a small portion of a diluted solution of sub-acetate of lead quickly and uniformly diffused. Another portion was treated in the same way, with a water solution of pure soap—both precipitates slowly formed, were collected in a moist state, subsequently examined by the microscope, and were found to contain only the ordinary constituents of white waters, when so treated.

The negative results of these trials in this connection, were sufficient to prove the absence of all coloring or colored bodies in this water—the strong attraction of the basic salt of lead for all coloring matter is well known—its power of removing colored infusoria is equally great. The calcareous soap product formed from the test removes all suspended matter, and permits us to detect organic forms with ease and certainty, after the fatty matters are dissolved on a glass slide.

It is obvious that in cases of complete analysis of waters, these precipitates should be subsequently chemically treated, and their state of hydration found, and engaged compounds separated and weighed.

Chemical analysis, thus conducted, having thrown no light on the cause of color in this water, has proved the absence of coloring substances, and placed it in the list of those waters, which do not exhibit the color seen in this lake; we are, therefore, led to ascribe the origin of its peculiar tint to natural influences, namely, the reflection and refraction of an azure sky in a colorless water.

The sky coloration of this part of Switzerland early engaged the observation of Saussure, who even experimented on its depth of color, while retaining its blue tint. All the conditions favoring almost constant blueness of sky, are present in this, and other parts of Switzerland in a marked degree, and I believe that extended observation will always connect the blue tint of white water with the deep azure hue of the clear sky above it.

By carefully watching the influence of the light from the sky on the water of this lake, I observed here as elsewhere that depth of color and tint in the water were modified greatly, or even lost under varied light from the sky. As the sun declined the azure tint was replaced by a grayish hue, or a light gray color was the closing hue of a series of shades of color. If the clear sky became flecked with small cloud surfaces, the water responded in unequal coloration, as if the water mirrored the sky, under this condition of beauty.

In other countries, there are bodies of colorless water which do not exhibit the coloration, commonly seen in Lake Leman water. Such localities are not favored with clear blue skies through atmospheric constitution, and bluish greenness of tint is the nearest approach to azure hue, which the sky permits, excepting perhaps at rare moments.

The negative results of chemical analysis, and the sufficiency of the effect of reflected and refracted light of the sky, which is over the water of Lake Leman, led me to the conclusion that the cause of color is found in the peculiar hue of the sky, so transmitted to the eye by a colorless water.

Boston, Mass., December 10th, 1869.

Art. XXIV.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School.* No. IX.—*On the Potassio-Cobaltic Nitrite known as Fischer's Salt, and some analogous and related compounds;* by Samuel P. Sadtler.

The composition of the double nitrite of cobalt and potash, known by the different names of "Fischer's Salt" and "Cobalt-yellow," has long been an open question. The following is a brief statement of the views advanced on the subject. Fischer[*] the discoverer of the salt made no analyses of it. St. Evre[†] who rediscovered it gave as a formula for it $K_2\Theta N_2\Theta_4+\text{Є}o\Theta N_2\Theta_4$, but does not give the analytical data. Stromeyer[‡] subsequently showed the incorrectness of this view and wrote the formula $\text{Є}o_2\Theta_3$, $2N_2\Theta_3+3(K_2\Theta N_2\Theta_3)+2H_2\Theta$. This formula is peculiar in making the atom $\text{Є}o_2\Theta_3$ combine with 2 atoms of $N_2\Theta_3$. We shall discuss the formula and results farther on. Erdmann[§] first pointed out a distinction between the salt formed in neutral solutions and the salt formed in acid solutions, which latter he considers as a normal salt. He gives both

$$2\text{Є}o\Theta,\ 3N_2\Theta_3+3(K_2\Theta N_2\Theta_3)+3N_2\Theta,\ \text{and}\ \text{Є}o_2\Theta_3 3N_2\Theta_3+3(K_2\Theta N_2\Theta_3)+3H_2\Theta$$

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[*] Pogg. Ann., lxxii, 477 and lxxiv, 124.

[†] Jr. pr. Ch., 54, 84 and 58, 185.

[‡] Ann. Ch. u. Ph., xcvi, 218.

[§] Jr. pr. Ch., lxxiii, 598.

or as we shall write it $Єo_2 6NΘ_2+6(KNΘ_2)+3H_2Θ$. Braun* the last writer on the subject, passes most of the preceding work in review. He takes up Stromeyer's results, and rejecting his views as to the composition of the salt analysed by him, figures for it a number of ingenious but somewhat complicated formulas. He considers that neither the "neutral salt" nor the "acid salt" of Erdmann can be regarded as anything but mixtures or "poly combinations," and for the first of them considered as such he gives a still more ingenious and complicated formula. We quote it as reduced to its empirical form by Blomstrand:† $Co_{62}K_{59}N_{112}H_{20}O_{454}$.‡ Braun's own results are expressed first by a formula containing both $Єo_2Θ_3$ and $ЄoΘ$, then by one containing $Єo_2Θ_3$, $ЄoΘ$, $N_2Θ_3$ and $N_2Θ_5$ and finally by the following:

$$3(Єo_2Θ_3,\ 3N_2Θ_3+4K_2Θ,\ 4N_2Θ_3+2H_2Θ)+2(Єo_2Θ_3,\ H_2Θ,\ 2N_2Θ_3+K_2Θ,\ N_2Θ_3+3H_2Θ).$$

The experiments made to settle its composition and the results obtained are as follows: St. Evre ignited the salt in a stream of nitrogen. Nitric oxyd was given off and the residue was a black oxyd, which on contact with HCl gave Cl and with $Є_2H_2Θ_4$ gave $ЄΘ_2$. This I cannot regard as conclusive, as mere ignition per se will not break up the $KNΘ_2$, so that the residue was not pure $Єo_2Θ_3$; moreover the $N_2Θ_3$ in breaking up may have given an atom of Θ to form the $Єo_2Θ_3$. Stromeyer found that $NaHΘ$ and $BaH_2Θ_2$ decompose it on gentle warming, even with exclusion of air, separating the brown hydrated $Єo_2Θ_3$ which dissolves in $Є_2H_4Θ_2$ with a brown, and in $Є_2H_2Θ_4$ with a green color. This observation is of undoubted value. He says also that on mixing the neutral solutions, the precipitate is slow in forming, and forms by an absorption of Θ, as is shown by dipping the connecting tube under a cylinder of air or oxygen. Erdmann confines himself to an examination of this last observation of Stromeyer's and finds that the "neutral salt" can be formed in an atmosphere of pure $ЄΘ_2$, and that therefore there can be no absorption of Θ in the case. Braun finds that $KHΘ$ acts very little upon the salt, merely changing its color from yellow to a dirty yellowish-green. $NaHΘ$, $BaH_2Θ_2$, $ЄaH_2Θ_2$ separate at a boiling heat the brownish-black hydrated $Єo_2Θ_3$. This is confirmatory of Stromeyer's observation. Carbonate of silver heated with water and Fischer's salt gave with separation of the hydrated $Єo_2Θ_3$, crystals of what from Braun's description were probably $AgNΘ_3$. KCy did not decompose it.

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* Zeitsch. Anal. Ch., vii, 313.

† Chemie der Jetztzeit, p. 414.

‡ Should be O 508.

My own experiments directed toward the solution of the question of $\mathrm{Ꞓo}\Theta$ or $\mathrm{Ꞓo_2}\Theta_3$ were as follows: I found Erdmann's observations as to the formation of the "neutral salt" in an atmosphere of $\mathrm{Ꞓ}\Theta_2$ correct. I added by means of a funnel tube a solution of $\mathrm{KN}\Theta_2$ boiled to expel the air, to a boiled solution of $\mathrm{Ꞓo\,Cl_2}$, placed at the bottom of a partially closed vessel, into which a strong stream of $\mathrm{Ꞓ}\Theta_2$ had been passing for some time. The salt began nevertheless to form slowly. Wishing to sift the matter more thoroughly, I made a series of experiments in eudiometer tubes over mercury. Into a eudiometer tube filled with mercury and perfectly free from air, I threw up a boiled solution of $\mathrm{ꞒoCl_2}$ and then one of $\mathrm{KN}\Theta_2$. The salt began to form soon at the juncture of the two liquids and then deposited steadily though slowly. After a number of hours not the slightest bubble of air was to be seen. The absence of all chance of oxydation here, either from air or excess of nitrous acid, shows conclusively the protoxyd nature of this "neutral salt." The "acid salt" was formed in the same manner—first a boiled solution of $\mathrm{ꞒoCl_2}$, then acetic acid, and then a boiled $\mathrm{KN}\Theta_2$ solution. A rapid evolution of gas ensues and the salt forms and falls in a layer on the top of the mercurial column. In another instance a small crystal of $\mathrm{ꞒoCl_2}$ was sent up and then acetic acid and then $\mathrm{KN}\Theta_2$. The formation of the salt was almost instantaneous and the evolution of gas exceedingly rapid. Before drawing any conclusions from this, however, the action of acetic acid on $\mathrm{KN}\Theta_2$, out of access of air, was to be studied. Gmelin* states that, when treated with acid out of access of air, nitric oxyd is evolved while the liquid takes up $\mathrm{N}\Theta_2$ and $\mathrm{N_2}\Theta_5$. This evolution was found to take place readily over the mercury and the gas answered to the test of ferrous sulphate and was colored red on admission of air, like the gas evolved in the formation of the salt. The question now is whether the liberated Θ is taken up in the formation of the salt to oxydize the $\mathrm{Ꞓo}\Theta$, to convert the excess of nitrite into nitrate. The question cannot be definitely settled, until we have some delicate test for a small quantity of nitrate in the presence of a large quantity of nitrite. But there is one thing which I regard as significant. Erdmann made one of his preparations of the "acid salt," by filtering the mixture of neutral solutions into pure acetic acid. Now the greater the excess of acid, the quicker would be the conversion of nitrite into nitrate, and we should look for a very small amount of the double nitrite salt. Yet it formed as readily, and analysis proved it to possess the same constitution as the other preparations. The inference from this is, that the sesquioxyd-forming power of the Ꞓo is sufficient to enable it to take liberated oxygen.

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* Cav. Soc. ed. vol. ii, p. 382.

Recourse was now had to analysis as a means of determining the question. Six distinct preparations of the salt in all were made and analyzed. In preparing the salt, the distinction made by Erdmann in regard to the formation of different compounds in neutral and acid solutions, was recognized and the salt was invariably formed in a solution, strongly acidified by acetic acid in the beginning, and kept so until the end by the addition of acid, if necessary. The salt of cobalt used was the chlorid, and, in all but the first two preparations, the solution was boiled to expel air and made acid. A strong solution of $KN\Theta_2$ was then added and the mixture was left to stand: 6 to 12 hours generally proved sufficient, and it was then filtered and washed, according to Erdmann's direction, with a solution of potassic acetate of about 10 per cent which was displaced by 80 per cent alcohol. The salt was then dried, first over the water-bath and then in a water-oven exactly at 100°. I saw no evidence of a decomposition at this temperature alluded to by Erdmann.

The results of full analyses of these six preparations warrant me I think in presenting the following conclusions:—

1st. That Fischer's salt is a *Tri-potassic-Cobaltic-Nitrite*, its essential formula being $Ꞓo_2\Theta_3, 3N_2\Theta_3+3(K_2\Theta, N_2\Theta_3)+Aq$, or $Ꞓo_2 6N\Theta_2+6(KN\Theta_2)+Aq$.

2nd. That it can be formed with $4H_2\Theta$, $3H_2\Theta$, $2H_2\Theta$, $H_2\Theta$, or anhydrous, according to the degree of concentration of the solutions used, passing in color from a light yellow to a dark greenish-yellow.

3rd. That in consequence of such dependence, we can, in in most preparations, fix no absolute point, but are liable to have a mixture of salts of different degrees of hydration.

The analytical methods used in the analyses of the salt were the following:—

The Ꞓo was determined first together with the K, as a double sulphate having the invariable constitution $Ꞓo_2K_6 5Ꞩ\Theta_4$. This sulphate has a distinct fusing-point, and affords an accurate means of determination. It will be discussed farther on. This is then dissolved, the solution made ammoniacal and brought to boiling, when the Ꞓo is thrown down as ꞒoꞨ. This is roasted and, after treatment with aqua regia and sulphuric acid, is weighed as neutral $ꞒoꞨ\Theta_4$. Or the Ꞓo was thrown down from the solution of the double sulphates as peroxyd by acetate of soda and chlorine, and then reduced by hydrogen to the metallic state.

The potash is then determined by difference.

The nitrogen was determined, either together with the $H_2\Theta$ by Gibbs's modification of Bunsen's method, which will be again alluded to, or directly by volume with the Sprengel-pump.

The $H_2\Theta$ either as above, or by combustion with Єu in a stream of $Є\Theta_2$.

The analytical data and results are appended.

Preparation No. 1.

·2011 gr. salt gave ·1732 gr. $Єo_2K_65S\Theta_4$=44·75 p. c. $Єo\Theta+K_2\Theta$ gave also ·0656 gr. $ЄoS\Theta_4$=15·78 p. c. $Єo\Theta$=17·47 p. c. $Єo_2\Theta_3$.
·1626 gr. salt gave ·1403 gr. $Єo_2K_65S\Theta_4$=44·83 p. c. $Єo\Theta+K_2\Theta$ gave also ·0529 gr. $ЄoS\Theta_4$=15·74 p. c. $Єo\Theta$=17·42 p. c. $Єo_2\Theta_3$.
·5147 gr. salt gave 112·7 cc. nitrogen at 15·25° and 483·2 mm. pres. =16·56 p. c. N=44·95 p. c. $N_2\Theta_3$.
·9410 gr. salt gave 167·6 cc. nitrogen at 15·25 and 601·6 mm. pres. =16·76 p. c. N=45·48 p. c. $N_2\Theta_3$.
1·1131 gr. salt gave ·0793 gr. $H_2\Theta$=7·00 p. c. $H_2\Theta$.
1·3418 “ “ ·0937 “ “ 6·98 “ “

Preparation No. 2.

·1082 gr. salt gave ·0955 gr. $Єo_2K_65S\Theta_4$=45·86 p. c. $Єo\Theta+K_2\Theta$ gave also ·0408 gr. $ЄoS\Theta_4$=16·16 p. c. $Єo\Theta$=17·88 p. c. $Єo_2\Theta_3$.
·1222 gr. salt gave ·1074 gr. $Єo_2K_65S\Theta_4$=45·66 p. c. $Єo\Theta+K_2\Theta$ gave also ·0455 gr. $ЄoS\Theta_4$=15·96 p. c. $Єo\Theta$=17·66 p. c. $Єo_2\Theta_3$.
·4192 gr. salt gave 102·83 cc. nitrogen at 18·25° and 462·6 mm. pres.=17·57 p. c. N.=47·70 p. c. $N_2\Theta_3$.
·4376 gr. salt gave
·6328 “ “ ·0346 gr. $H_2\Theta$=5·46 p. c. $H_2\Theta$.
·5494 “ “ ·0296 gr. $H_2\Theta$=5·39 p. c. $H_2\Theta$.

Preparation No. 3.

·6215 gr. salt gave ·5682 gr. $Єo_2K_65S\Theta_4$=47·50 p. c. $Єo\Theta+K_2\Theta$ gave also ·0797 gr. Єo=12·82 p. c. Єo=18·04 p. c. $Єo_2\Theta_3$.
·5838 gr. salt gave ·5349 gr. $Єo_2K_65S\Theta_4$=47·60 p. c. $Єo\Theta+K_2\Theta$ gave also ·0740 gr. Єo=12·68 p. c. Єo=17·83 p. c. $Єo_2\Theta_3$.
·4451 gr. salt gave ·4088 gr. $Єo_2K_65S\Theta_4$=47·71 p. c. $Єo\Theta+K_2\Theta$ gave also ·0574 gr. Єo=12·89 p. c. Єo=18·14 p. c. $Єo_2\Theta_3$.
·5796 gr. salt gave ·5329 gr. $Єo_2K_65S\Theta_4$=47·77 p. c. $Єo\Theta+K_2\Theta$ gave also ·0735 gr. Єo=12·68 p. c. Єo=17·83 p. c. $Єo_2\Theta_3$.
With 1·6634 gr. salt, combustion-tube lost ·3616 gr., of which $ЄaЄl_2$ tube took up ·0710 gr.=4·27 p. c. $H_2\Theta$ leaving ·2906 gr.=17·47 p. c. N.=47·42 p. c. $N_2\Theta_3$.
With 1·5123 gr. salt, combustion-tube lost ·3302 gr., of which $ЄaЄl_2$ tube took up ·0610 gr.=4·03 p. c. $H_2\Theta$ leaving ·2692 gr.=17·80 p. c. N.=48·32 p. c. $N_2\Theta_3$.

Preparation No. 4.

6677 gr. salt gave ·6073 gr. $Ꞓo_2K_65S\Theta_4$=47·26 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·2258 gr. $ꞒoS\Theta_4$=16·36 p. c. $Ꞓo\Theta$=18·11 p. c. $Ꞓo_2\Theta_3$.

·3590 gr. salt gave ·3264 gr. $Ꞓo_2K_65S\Theta_4$=47·24 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·1226 gr. $ꞒoS\Theta_4$=16·52 p. c. $Ꞓo\Theta$=18·29 p. c. $Ꞓo_2\Theta_3$.

With 1·1138 gr. salt, combustion-tube lost ·2461 gr., of which $ꞒaꞒl_2$ tube took up ·0439=3·94 p. c. $H_2\Theta$ leaving ·2022 gr. =18·15 p. c. N.=49·27 p. c. $N_2\Theta_3$.

With 1·2494 gr. salt, combustion-tube lost ·2674 gr., of which $ꞒaꞒl_2$ tube took up ·0407=3·26 p. c. $H_2\Theta$, leaving ·2267 gr. =18·14 p. c. N.=49·25 p. c. $N_2\Theta_3$.

With 1·1587 gr. salt, combustion-tube lost ·2533 gr., of which $ꞒaꞒl_2$ tube took up ·0432=3·73 p. c. $H_2\Theta$ leaving ·2101 gr. =18·13 p. c. N.=49·20 p. c. $N_2\Theta_3$.

Preparation No. 5.

·2895 gr. salt gave ·2633 gr. $Ꞓo_2K_65S\Theta_4$=47·26 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·0382 Ꞓo=13·20 p. c. Ꞓo=18·56 p. c. $Ꞓo_2\Theta_3$.

·3993 gr. salt gave ·3644 gr. $Ꞓo_2K_65SO_4$=47·42 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·0531 Ꞓo=13·30 p. c. Ꞓo=18·70 p. c. $Ꞓo_2\Theta_3$.

·5747 gr. salt gave ·5243 gr. $Ꞓo_2K_65S\Theta_4$=47·40 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·0769 gr. Ꞓo=13·38 p. c. Ꞓo=18·82 p. c. $Ꞓo_2\Theta_3$.

·5959 gr. salt gave ·5443 gr. $Ꞓo_2K_65SO_4$=47·46 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·0768 gr. Ꞓo=12·89 p. c. Ꞓo=18·14 p. c. $Ꞓo_2\Theta_3$.

With 1·2557 gr. salt, combustion-tube lost ·2758 gr., of which $ꞒaꞒl_2$ tube took up ·0459=3·66 p. c. $H_2\Theta$ leaving ·2299 gr. =18·31 p. c. N.=49·69 p. c. $N_2\Theta_3$.

With 1·1464 gr. salt, combustion-tube lost ·2556 gr., of which $ꞒaꞒl_2$ tube took up ·0479=4·18 p. c. $H_2\Theta$ leaving ·2077 gr. =18·12 p. c. N.=49·18 p. c. $N_2\Theta_3$.

Preparation No. 6.

·8154 gr. salt gave ·7644 gr. $Ꞓo_2K_65S\Theta_4$=48·71 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·2823 gr. $ꞒoS\Theta_4$=16·75 p. c. $Ꞓo\Theta$=18·54 p. c. $Ꞓo_2\Theta_3$.

·5453 gr. salt gave ·5110 gr. $Ꞓo_2K_65S\Theta_4$=48·69 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·1885 gr. $ꞒoS\Theta_4$=16·73 p. c. $Ꞓo\Theta$=18·51 p. c. $Ꞓo_2\Theta_3$.

·6805 gr. salt gave ·6373 gr. $Ꞓo_2K_65SO_4$=48·66 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·2358 gr. $ꞒoS\Theta_4$=16·77 p. c. $Ꞓo\Theta$=18·55 p. c. $Ꞓo_2\Theta_3$.

·3712 gr. salt gave ·3480 gr. $Ꞓo_2K_65S\Theta_4$=48·71 p. c. $Ꞓo\Theta+K_2\Theta$ gave also ·1297 gr. $ꞒoS\Theta_4$=16·90 p. c. $Ꞓo\Theta$=18·71 p. c. $Ꞓo_2\Theta_3$.

With 1·5460 gr. salt, combustion-tube lost ·3033 gr., of which $ꞒaꞒl_2$ tube took up ·0116=0·75 p. c. $H_2\Theta$ leaving ·2917 gr. =18·87 p. c. N=51·21 p. c. $N_2\Theta_3$.

Prep. No. 1.

$\mathrm{Ꞓo_2 6N\Theta_2+6(KN\Theta_2)+4H_2\Theta}$.

	Theor. p. cts.	Found p. cts.
$\mathrm{Ꞓo_2\Theta_3}$	17·00	17·45
$\mathrm{K_2\Theta}$	28·94	29·00
$\mathrm{N_2\Theta_3}$	46·69	45·21
$\mathrm{H_2\Theta}$	7·37	6·99

Prep. No. 2.

$\mathrm{Ꞓo_2 6N\Theta_2+6(KN\Theta_2)+3H_2\Theta}$.

Theor. p. cts	Found p. cts.
17·32	17·77
29·48	29·70
47·57	47·22
5·63	5·43

Prep. No. 3.

$\mathrm{Ꞓo_2 6N\Theta_2+6(KN\Theta_2)+2H_2\Theta}$.

	Theor. p. cts.	Found p. cts.
$\mathrm{Ꞓo_2\Theta_3}$	17·65	17·96
$\mathrm{K_2\Theta}$	30·04	31·41*
$\mathrm{N_2\Theta_3}$	48·48	47·87
$\mathrm{H_2\Theta}$	3·83	4·15

Prep. No. 4 *and* 5.

$\mathrm{Ꞓo_2 6N\Theta_2+6(KN\Theta_2)+3H_2\Theta}$.

Theor. p. cts.	Found p. cts.	
17·99	18·20	18·55
30·63	30·81	30·61
49·43	49·24	49·43
1·95	3·64†	3·92†

Prep. No. 6.

$\mathrm{Ꞓo_2 6N\Theta_2+6(KN\Theta_2)}$.

	Theor. p. cts.	Found p. cts.
$\mathrm{Co_2\Theta_3}$	18·35	18·58
$\mathrm{K_2\Theta}$	31·24	31·90
$\mathrm{N_2\Theta_3}$	50·41	51·21
$\mathrm{H_2\Theta}$	0·—	0·75

It will be seen that, while the general coincidence is well sustained, there are several decided deviations. We can only call attention to certain facts in explanation 1st. We probably have in some of these preparations, mixtures of the salts of different degrees of hydration. 2nd. The salt cannot be purified by re-crystallization. 3rd. The acetate of potash with which the salt is first washed is, as Rose‡ remarks, only removed with difficulty by the alcohol.

Having formed from these analytical results our conclusions, let us see if we can find any confirmation of them from other sources. Lang§ states that although the corresponding soda and ammonia salts were probably formed, yet they could not be obtained on account of their solubility. By using a strong $\mathrm{NaN\Theta_2}$ solution,‖ I succeeded in obtaining the new soda compounds, which I am about to describe. To a solution of $ꞒoCl_2$, which had been boiled and then acidified with acetic acid I added an excess of $NaN\Theta_2$ solution. The mixture instantly became of a dark color and a yellowish-brown salt began to form, with brisk evolution of nitric oxyd. After some hours, when the evolution of gas was progressing but slowly, I added some more of the $NaN\Theta_2$ solution, testing also to assure myself that the solution was acid, and in a little while the solution began to assume a yellowish color, and I found a clear yellow salt depositing. This led to the conjecture that there were two distinct soda salts. I accordingly sought to obtain them distinct for analysis. This I found to be difficult. The brown salt I readily got almost pure, but succeeded only partially in obtaining the yellow one distinct. The brown salt continues to form, to a greater or less extent, even after the formation of the yellow one has begun. The preparations were all washed with acetate of soda, which was displaced with alcohol. From analyses of them, and from other and subsequent results, I am led to consider them as salts analogous to Fischer's salt and differing from each other in the number of atoms of $NaN\Theta_2$ in combination with the $Ꞓo_2 6N\Theta_2$. The brown salt I would call a *di-sodio-cobaltic-nitrite*, and the yellow one a *tri-sodio-cobaltic-nitrite*, giving them respectively the formulas:

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* Acetate of potash probably not all washed out by alcohol.

† Unnaccountably high.

‡ Anal. Ch. sechste auflage, Zweiter Band, p. 128.

§ Jr. pr. Ch., lxxxvii, 303.

‖ The $\mathrm{KN\Theta_2}$ and $\mathrm{NaN\Theta_2}$ solutions used in this work were prepared as follows: 1 part of nitre and 2 parts of lead were fused together after Stromeyer's well-known method. After the complete oxydation of the lead, the residue was boiled several times with water. The solution was then filtered to remove the undissolved litharge. Into the filtrate which contained more or less free alkali, the red gas evolved from nitric acid and sawdust was passed until a neutral reaction was obtained This red gas, however, does not consist of the oxyds of nitrogen alone; it also contains some carbonic acid. This throws down as carbonate of lead, the litharge which

$Ꞓo_2 6N\Theta_2 + 4(NaN\Theta_2) + H_2\Theta$. And $Ꞓo_2 6N\Theta_2 + 6(NaN\Theta_2) + H_2\Theta$

The following are the analytical results:

Brown Salt. 1st Preparation.

·5400 gr. salt gave ·4676 gr. $Ꞓo_2Na_4 4Ꞩ\Theta_4$=39·94 p. c. $Ꞓo\Theta + Na_2\Theta$ also gave ·2374 gr. $ꞒoꞨ\Theta_4$=21·27 p. c. $Ꞓo\Theta$=23·54 p. c. $Ꞓo_2\Theta_3$.

·5866 gr. salt gave ·5041 gr. $Ꞓo_2Na_4 4Ꞩ\Theta_4$=39·64 p. c. $Ꞓo\Theta + Na_2\Theta$ also gave ·2615 gr. $ꞒoꞨ\Theta_4$=21·57 p. c. $Ꞓo\Theta$=23·87 p. c. $Ꞓo_2\Theta_3$.

Brown Salt. 2nd Preparation.

·5602 gr. salt gave ·4805 gr. $Ꞓo_2Na_4 4Ꞩ\Theta_4$=39·57 p. c. $Ꞓo\Theta + Na_2\Theta$ also gave ·2425 gr. $ꞒoꞨ\Theta_4$=20·95 p. c. $Ꞓo\Theta$=23·18 p. c. $Ꞓo_2\Theta_3$.

·4671 gr. salt gave ·4027 gr. $Ꞓo_2Na_4 4Ꞩ\Theta_4$=39·77 p. c. $Ꞓo\Theta + Na_2\Theta$ also gave ·2026 gr. $ꞒoꞨ\Theta_4$=20·99 p. c. $Ꞓo\Theta$=23·23 p. c. $Ꞓo_2\Theta_3$.

With 1·7054 gr. salt, combustion-tube lost ·4476 gr., of which $ꞒaꞒl_2$ tube took up ·1156 gr.=6·78 p. c. $H_2\Theta$ leaving ·2920 gr.=17·12 p. c. N.=46·47 p. c. $N_2\Theta_3$.

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had gone into solution. It is now filtered and the filtrate evaporated to an oily consistency, when the saltpetre in it will almost all crystallize out. We filter it again and if we wish it still purer we can give it the treatment with alcohol described by Hampe (Ann. Ch. u. Ph., 125, 335). This frees it completely from saltpetre and concentrates it.

With 1·8558 gr. salt, combustion-tube lost ·4476 gr., of which $ꞒaꞒl_2$ tube took up ·1216=6·55 p. c. $H_2Θ$ leaving ·3260 gr. =17·56 p. c. N.=47·67 p. c. $N_2Θ_3$.

Yellow Salt. 1st Preparation.

·6342 gr. salt gave ·5448 gr. $Ꞓo_2Na_65ꞨΘ_4$=39·22 p. c. $ꞒoΘ+Na_2Θ$ the Ꞓo determination was lost.

·5805 gr. salt gave ·4994 gr. $Ꞓo_2Na_65ꞨΘ_4$=39·28 p. c. $ꞒoΘ+Na_2Θ$ also gave ·2431 gr. $ꞒoꞨΘ_4$=18·55 p. c. ꞒoΘ=20·53 p. c. $Ꞓo_2Θ_3$.

Yellow Salt. 2nd Preparation.

·4197 gr. salt gave ·3700 gr. $Ꞓo_2Na_65ꞨΘ_4$=40·25 p. c. $ꞒoΘ+Na_2Θ$ also gave ·1723 gr. $ꞒoꞨΘ_4$=19·86 p. c. ꞒoΘ=21·98 p. c. $Ꞓo_2Θ_3$.

·4873 gr. salt gave ·4291 gr. $Ꞓo_2Na_65ꞨΘ_4$=40·20 p. c. $ꞒoΘ+Na_2Θ$ also gave ·2012 gr. $ꞒoꞨΘ_4$=19·98 p. c. ꞒoΘ=22·11 p. c. $Ꞓo_2Θ_3$.

Yellow Salt. 3rd Preparation.

·8554 gr. salt gave ·7354 gr. $Ꞓo_2Na_65ꞨΘ_4$=39·25 p. c. $ꞒoΘ+Na_2Θ$ also gave ·3543 gr. $ꞒoꞨΘ_4$=20·04 ꞒoΘ=22·18 p. c. $Ꞓo_2Θ_3$.

	$Ꞓo_26NΘ_2+4(NaNΘ_2)+H_2Θ$.			$Ꞓo_26NΘ_2+6(NaNΘ_2)+H_2Θ$.			
	Theor. p. cts.	Found p. cts.		Theor. p. cts.	Found p. cts.		
$Ꞓo_2Θ_3$—	24·13	23·71	23·21	20·10	20·53	22·04	22·18
$Na_2Θ$—	18·02	18·37	18·70	22·52	20·70	20·31	19·21
$N_2Θ_3$—	55·23		47·07 (?)*	55·20			[20·54]
$H_2Θ$—	2·62		6·66 (?)*	2·18			

Some error connected with the $ꞒaꞒl_2$ tube which I am entirely unable to account for has here caused an error. It will be observed that in each case the total loss of the combustion-tube is very nearly what the theor. per cent of $H_2Θ+N$ would demand, while the increase in the $ꞒaꞒl_2$ tube is out of all proportion. If we examine the results in the yellow salt, we see that all these preparations are more or less mixtures. The last preparation indeed might be put equally under the head of the 2-atom salt, the per cent in bracket showing the per cent $Na_2Θ$ calculated on the other formula. We are fortunately, however, not dependant solely on these analyses for our evidences of the existence of these two salts. On account of their partial solubility, we are enabled to obtain substitution compounds from them, which throw very strong light on the composition of the alkali-cobaltic nitrites. In one case, after the formation and filtering off of some of the brown salt, chlorid of luteocobalt was added to the wine-colored filtrate, which probably held some of both salts in solution. The strong tendency which luteocobalt possesses to form double salts suggested to Dr. Gibbs that some results might be here obtained. A beautiful yellow crystalline salt formed and settled quite readily. I then prepared a considerable amount of it for analysis. It proved to be as good as insoluble in cold water, so that its washing was an easy matter. Its analysis shows very clearly that it was formed from the tri-sodio-cobaltic nitrite, or yellow soda salt, which existed in the solution, and that one atom of luteocobalt exactly replaces the three of $Na_2\Theta$, giving it the formula:

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* The found per centages of $N+H_2Θ$ in the brown salt is the average of two determinations.

$$\text{Є}o_2, 6N\Theta_2+\text{Є}o_2 12NH_3, 6N\Theta_2+H_2\Theta.$$

The following are the analytical results:

·5627 gr. salt gave ·3471 gr. $\text{Є}oS\Theta_4=29{\cdot}85$ p. c. $\text{Є}o\Theta=33{\cdot}03$ p. c. $\text{Є}o_2\Theta_3$.
·2867 gr. salt gave ·1770 gr. $\text{Є}oS\Theta_4=29{\cdot}87$ p. c. $\text{Є}o\Theta=33{\cdot}06$ p. c. $\text{Є}o_2\Theta_3$.
·5834 gr. salt gave 176·88 cc. nitrogen at 3° and 661·29 mm. = 32·79 p. c. N.=19·91 p. c. NH_3 and 44·50 p. c. $N_2\Theta_3$.
1·0866 gr. salt gave with $\text{Є}u+\text{Є}u\Theta+PbCr\Theta_4$ ·3745 gr.=34·46 p. c. $H_2\Theta=32{\cdot}09$ p. c. from $NH_3+2{\cdot}37$ p. c. $H_2\Theta$.

$$\text{Є}o_2, 6N\Theta_2+\text{Є}o_2 12NH_3, 6N\Theta_2+H_2\Theta.$$

	Theor. p. cts.	Found p. cts.
$\text{Є}o_2\Theta_3$	32·87	33·04
NH_3	—20·20	19·91
$N_2\Theta_3$	—45·15	44·50
$H_2\Theta$	— 1·78	2·37

I now tried chlorid of purpureo-cobalt upon some of the same solution and got a roseo-cobalt compound, exactly analogous to that of luteocobalt, its formula being—

$$\text{Є}o_2 6N\Theta_2+\text{Є}o_2 10NH_3 6N\Theta_2+H_2\Theta.$$

The following were the analytical results:

·3186 gr. salt gave ·1974 gr. $\text{Є}oS\Theta_4=29{\cdot}98$ p. c. $\text{Є}o\Theta=33{\cdot}18$ p. c. $\text{Є}o_2\Theta_3$.

$$\text{Є}o_2, 6N\Theta_2+\text{Є}o_2 10NH_3, 6N\Theta_2+H_2\Theta.$$

	Theor. p. ct.	Found p. ct.
$\text{Є}o_2\Theta_3$	—34·02	33·18

This salt is not nearly as insoluble, however, as the luteocobalt salt and but a little of it could be obtained. At first I got it as a yellow and very crystalline salt on the sides of the beaker, after standing some little time. The crystals examined under the microscope were beautifully defined monoclinic prisms with terminal planes. The portion I analyzed had more of the color of the salts of roseo-cobalt and showed under the microscope a star-shaped aggregation of small crystals.

I also formed in the soda-salt solution a yellowish xantho-cobalt compound having probably an analogous constitution but did not get enough of it to analyze. Examined under the microscope, the crystals were seen to be of a peculiar cup-shaped appearance and quite large and pointed.

The ammonium salts were next examined. That one at least existed had been found by Gibbs and Genth,[*] although it had not been analyzed by them. Erdmann[†] had, however, formed an ammonium salt exactly corresponding to Fischer's salt. I succeeded in forming this and another, the two exactly corresponding in constitution to the two soda salts. The circumstances under which the different salts form I am not able, however, to state at all positively, except that in forming the 3-atom salt I had more concentrated CoCl_2 and $\text{NH}_4\text{N}\Theta_2$ solutions. They are both bright yellow and could not be distinguished by their color. We may term them the *di-ammonio-cobaltic nitrite* and the *tri-ammonio-cobaltic nitrite*, their formulas being respectively

$$\text{Co}_2 6\text{N}\Theta_2+4(\text{NH}_4\text{N}\Theta_2)+2\text{H}_2\Theta$$

and

$$\text{Co}_2 6\text{N}\Theta_2+6(\text{NH}_4\text{N}\Theta_2)+2\text{H}_2\Theta.$$

The following were the analytical results:

Two-atom Salt.

·4495 gr. salt gave ·2022 gr. $\text{CoS}\Theta_4$=21·77 p. c. $\text{Co}\Theta$=24·09 p. c. $\text{Co}_2\Theta_3$.

Three-atom Salt.

·2992 gr. salt gave ·1145 gr. $\text{CoS}\Theta_4$=18·53 p. c. $\text{Co}\Theta$=20·51 p. c. $\text{Co}_2\Theta_3$.

·4169 gr. salt gave ·1602 gr. $\text{CoS}\Theta_4$=18·59 p. c. $\text{Co}\Theta$=20·58 p. c. $\text{Co}_2\Theta_3$.

	$\text{Co}_2 6\text{N}\Theta_2+4(\text{NH}_4\text{N}\Theta_2)+2\text{H}_2\Theta$.		$\text{Co}_2 6\text{N}\Theta_2+6(\text{NH}_4\text{N}\Theta_2)+2\text{H}_2\Theta$.	
	Theor. pr. ct.	Found pr. ct.	Theor. pr. ct.	Found pr. ct.
$\text{Co}_2\Theta_3$—	24·20	24·09	20·39	20·55

Lang is more nearly right in the case of these ammonia compounds than in regard to the soda salts, they being much more soluble. I was unable, however, to obtain any substitution compounds. With this additional light we are now prepared to draw some conclusions. We find that our luteocobalt and roseocobalt compounds are exactly analogous to the cobalticyanids of luteocobalt and roseocobalt formed by Gibbs and Genth,[‡] the only difference being that the monatomic radical $\text{N}\Theta_2$ here replaces the monatomic radical Cy. This I think gives us a very strong argument in favor of the exact analogy of the 3-atom soda salt and with it of Fischer's salt, to the cobalticyanid of potassium, from which the salts of Gibbs and Genth were formed. With this view of these salts, we are able to discern yet other analogies. Iridium forms a number of sesquioxyd salts very similar to those of cobalt. We have, indeed, two double chlorids of iridium, exactly analogous to the 2-atom and 3-atom soda or ammonia salts. Their formulas are

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[*] Researches on Ammonia cobalt bases, p. 48.

[†] Jour. Pr. Ch., xcvii, 405.

[‡] Researches on Ammonia cobalt bases, pp. 40, and 18.

$$Ir_2Cl_6+4KCl \text{ and } Ir_2Cl_6+6KCl$$

in which we should expect monatomic Cl to be exactly replaceable by monatomic $N\Theta$, and so we find that it is. Dr. Gibbs has discovered an iridium salt, having the formula

$$Ir_2 6N\Theta_2+6(KN\Theta_2)+2H_2\Theta,$$

an exact analogue of Fischer's salt. Lang[*] also has discovered a rhodium salt, whose formula $\mathrm{Ꞧh}_2 6N\Theta_2+6(KN\Theta_2)$ is precisely analogous. If we place together for a moment the three salts in question, their identity of constitution becomes apparent.

$Ir_2\Theta_3$, $3N_2\Theta_3+3(K_2\Theta,\ N_2\Theta_3)$ or $Ir_2 6N\Theta_2+6(KN\Theta_2)$—Gibbs.
$\mathrm{Ꞧh}_2\Theta_3$, $3N_2\Theta_3+3(K_2\Theta,\ N_2\Theta_3)$ or $\mathrm{Ꞧh}_2 6N\Theta_2+6(KN\Theta_2)$—Lang.
$\mathrm{Ꞓo}_2\Theta_3$, $3N_2\Theta_3+3(K_2\Theta,\ N\ \Theta_3)$ or $\mathrm{Ꞓo}_2 6N\Theta_2+6(KN\Theta_2)$—Sadtler.

If, therefore, we are to place any dependence at all upon analogy, the universal occurrence of the hexatomic $\mathrm{Ꞓo}_2$ atom in all our compounds, is what we should expect, a priori. The analyses of the series of salts I think fully confirms this expectation.

I have now to discuss some related compounds—those formed in neutral solutions. Erdmann first pointed out the distinction between these and the normal Fischer's salt. He obtained and analyzed a yellow "neutral salt." I obtained one corresponding to this, and one of a different ratio. By adding a somewhat dilute solution of $KN\Theta_2$ to a warm solution of $\mathrm{Ꞓo}Cl_2$, I obtained a highly crystalline precipitate, which was shortly followed by a somewhat crystalline greenish salt, which was again followed after some time by a yellowish precipitate, with little or no crystalline character. The appearances were indicative of the successive formation of three protoxyd compounds. This however, I found was not the case. The black precipitate under the microscope was seen to consist of beautifully defined cubes of a very dark green color, and the green precipitate was seen to consist of similar cubes, though much smaller. Analysis afterward proved them to be the same salt in different states of aggregation. On adding a warm concentrated solution of $KN\Theta_2$, however, to a warm concentrated solution of $\mathrm{Ꞓo}Cl_2$ nothing is formed but a flocculent yellow precipitate, which appears to be the same as that which I formed over mercury in the eudiometer tube. The formulas of these salts appear to be as follows. For the black or green salt $2(Ꞓo2N\Theta_2)+2(KN\Theta_2)+H_2\Theta$ and for the yellow salt $Ꞓo2N\Theta_2+2(KN\Theta_2)+H_2\Theta$, and the first may be termed a *potassio-dicobaltous nitrite* and the second a *potassio-mono-cobaltous nitrite.* Appended are the analytical results.

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[*] Royal Swedish Acad. Trans., 1864.

Prep. No. 1.—*Black Salt*, very crystalline.

·5421 gr. salt gave ·5421 gr. $Ꞓo_2K_23S\Theta_4=50·43$ p. c. $Ꞓo\Theta+K_2\Theta$
also gave ·3425 gr. $ꞒoS\Theta_4=30·57$ p. c. $Ꞓo\Theta$.
·7341 gr. salt gave ·7361 gr. $Ꞓo_2K_23S\Theta_4=50·57$ p. c. $Ꞓo\Theta+K_2\Theta$
also gave ·4662 gr. $ꞒoS\Theta_4=30·73$ p. c. $Ꞓo\Theta$.

Prep. No. 2.—*Green Salt* (had not been washed for analysis.

·3003 gr. salt gave ·3063 gr. $Ꞓo_2K_23S\Theta_4=51·44$ p. c. $Ꞓo\Theta+K_2\Theta$
also gave ·1846 gr. $ꞒoS\Theta_4=29·74$ p. c. $Ꞓo\Theta$.
·3221 gr. salt gave ·3286 gr. $Ꞓo_2K_23S\Theta_4=51·45$ p. c. $Ꞓo\Theta+K_2\Theta$
also gave ·1971 gr. $ꞒoS\Theta_4=29·61$ p. c. $Ꞓo\Theta$.

Prep. No. 3.—*Black and Green Salts mixed*, very crystalline.

·6942 gr. salt gave ·6978 gr. $Ꞓo_2K\ 3S\Theta^4=50·70$ p. c. $Ꞓo\Theta+{}_2K\Theta$
also gave ·4410 gr. $ꞒoS\Theta_4=30·74$ p. c. $Ꞓo\Theta$.
·4713 gr. salt gave ·4730 gr. $Ꞓo_2K_23S\Theta_4=50·62$ p. c. $Ꞓo\Theta=K_2\Theta$
also gave ·3004 gr. $ꞒoS\Theta_4=30·84$ p. c. $Ꞓo\Theta$.

Prep. No. 1.—*Yellow Salt.*

·2702 gr. salt gave ·2631 gr. $ꞒoK_22S\Theta_4=50·05$ p. c. $Ꞓo\Theta+K_2\Theta$
also gave ·1235 gr. $ꞒoS\Theta_4=22·12$ p. c. $Ꞓo\Theta$.
·4615 gr. salt gave ·4497 gr. $ꞒoK_22S\Theta_4=50·08$ p. c. $Ꞓo\Theta+K_2\Theta$
also gave ·2103 gr. $ꞒoS\Theta^4=22·05$ p. c. $Ꞓo\Theta$.

$2(Ꞓo2N\Theta_2)+2(KN\Theta_2)+H_2\Theta$. $Ꞓo2N\Theta_2+2(KN\Theta_2)+H_2\Theta$

Theor. p. cts.	Found p. c. cts.			Theor. p. cts.	Found p. cts.
$Ꞓo\Theta$—30·60	30·65	29·67	30·79	22·11	22·08
$K_2\Theta$—19·22	19·85	21·78	19·87	27·77	27·09
$N_4\Theta$—46·51				44·80	
$H_2\Theta$— 3·67				5·31	

In looking over the analytical data of these analyses of protoxyd salts, we notice a striking similarity between the weights of salt taken and weights of double sulphates obtained. The explanation is very simple, and yet I believe there is involved in it a fact of importance. The similarity of weights is due to the similarity of atomic weights of $N_2\Theta_3$ and $S\Theta_3$ (76 and 80) with which the bases are combined. Now in none of the very many determinations made in the "acid salts," do we find such a similarity. On the contrary we find the weight of double sulphates to be invariably less than the weight of salt taken and in a fixed ratio. The explanation of this is that $Ꞓo_2\Theta_33N_2\Theta_3$ by digestion with $H_2S\Theta$ is converted into two atoms of neutral $ꞒoS\Theta^4$ and becomes $2(ꞒoS\Theta_4)$. $2Ꞓo\Theta, 2N_2\Theta_3$ simply assumes

$\text{S}\Theta_3$ instead of $N_2\Theta_3$ and becomes likewise $2(\text{Єo}\text{S}\Theta_4)$. The first loses and atom of $N\Theta_2$ which the second does not. We have here a strong evidence of the totally different habitus of the Єo atom in the "neutral" and in the "acid" salts.

Having now made a complete statement of all my analytical results in the case of Fischer's salt proper, as well as in this series of analogous and related compounds, which I have described in this connection, and noticed the conclusions to be drawn from them, before summing up my argument, I will examine how the results of Stromeyer, Erdmann and Braun compare with my own. In the first case I think the last argument drawn from the similarity of weights of double nitrite and double sulphate in the protoxyd salts and the difference of weights of double nitrite and double sulphate in the sesquioxyd salts, effectually precludes our assigning a formula like that of Stromeyer, in which $2(N_2\Theta_3)$ combines with $\text{Єo}_2\Theta_3$, to the normal Fischer's salt. The salt Stromeyer analyzed was probably a mixture of "acid" and "neutral" salts, as he speaks of adding "a little acetic acid." It is easily seen that a mixture of the "acid salt" and the yellow "neutral salt" would give figures approximating to his. Erdmann's results correspond in the main to the analysis of my second preparation. The only difference then is that I have introduced a generalization, which I think my figures fully justify. With Braun's results, I am hardly able to make a comparison, as he made his preparation in a manner totally different from that followed by either Erdmann or myself. His formulas, as might be inferred from my statement of them in the beginning of this paper, I am still less able to accept.

To sum up briefly, the reasons for regarding Fischer's salt as a potassio-cobaltic nitrite, having the essential formula

$$\text{Єo}_2 6N\Theta_2 + 6(KN\Theta\ ) + aq. \text{ are—}$$

1st. The liberation of Θ at the moment of its formation and the rapidity with which, under these circumstances, the salt forms, would seem to justify the assumption.

2nd. The analyses of six distinct preparations sustain it.

3rd. The existence of corresponding sodic and ammonic salts is proved by analysis.

4th. The formation of substitution compounds exactly analogous to well known salts is proved by analysis.

5th. The analogy of Fischer's salt to double cyanids, chlorids and nitrites proved to contain a similar hexatomic atom.

6th. The difference of ratio between the weights in the protoxyd and sesquioxyd salts is marked and constant throughout.

Accepting these proofs, we will sum up with equal brevity the formulas of the salts analyzed on this view:—

Tri-potassio-cobaltic nitrite,	$\text{Ꞓo}_2 6N\Theta_2+6(KN\Theta_2)+aq.$
Di-sodio-cobaltic nitrite,	$\text{Ꞓo}_2 6N\Theta_2+4(NaN\Theta_2)+H_2\Theta.$
Tri-sodio-cobaltic nitrite,	$\text{Ꞓo}_2 6N\Theta_2+6(NaN\Theta_2)+H_2\Theta.$
Di-ammonio-cobaltic nitrite,	$\text{Ꞓo}_3 6N\Theta_2+4(NH_4N\Theta_2)+2H_2\Theta.$
Tri-ammonio-cobaltic nitrite,	$\text{Ꞓo}_2 6N\Theta_2+6(NH_4N\Theta_2)+2H_2\Theta.$
Luteocobaltio-cobaltic nitrite,	$\text{Ꞓo}_2 6N\Theta_2+\text{Ꞓo}_2 12NH_3, 6N\Theta_2+H_2\Theta.$
Roseocobaltio-cobaltic nitrite,	$\text{Ꞓo}_2 6N\Theta_2+\text{Ꞓo}_2 10NH_2, 6N\Theta_2+H_2\Theta.$
Xanthocobaltio-cobaltic nitrite,	
Potassio-dicobaltous nitrite,	$2(\text{Ꞓo}2N\Theta_2)+2(KN\Theta_2)+H_2\Theta.$
Potassio-monocobaltous nitrite,	$\text{Ꞓo}_2 N\Theta_2+2(KN\Theta_2)+H_2\Theta.$

These it will be seen are capable of ready conversion into atomic formulas, thus: $\text{Ꞓo}_2^{\text{vi}}[K(NO_2)_2]6$ and $\text{Ꞓo}_2^{\text{vi}}[(Na(NO_2)_2)_2\ (NO_2)_4]$ and so on.

In the course of my examination of these salts, I had an opportunity of comparing the relative merits of the different analytical methods employed. A brief statement in regard to them may be of some value.

1st. Determination of Ꞓo and K. The double sulphate method already alluded to, was first proposed by Gibbs and Genth,* and I find it to give extremely close results. Sulphuric acid is added to the weighed portion of the salt in a porcelain crucible, which is then heated carefully, either by a ring gas burner from above, or by radiation while supported in a sheet-iron crucible. After the nitrous acid is all driven off with most of the excess of sulphuric acid, the sulphates begin to fuse and as the last trace of sulphuric acid goes off, they pass into calm fusion. They will then bear a very considerable increase of heat without farther decomposition. I found this method capable of extension to both the soda salts, as well as to the protoxyd salts, the double sulphates formed having of course a constitution depending upon the proportion of the bases in the original salts. The soda sulphates did not appear, however, to possess as perfect a fusibility as the double potash sulphate from Fischer's salt, while the double sulphates from the protoxyd potash salts were also less fusible, in proportion to their greater ratio of Ꞓo. I give an instance in proof of the accuracy of this method. Four determinations of $\text{Ꞓo}\Theta+K_2\Theta$ were made in preparation No. 6 of Fischer's salt in the manner described. The percentages of double oxyds were 48·71, 48·69, 48·66, 48·71. This method was tried by Gauhe† without obtaining good results. Several points are to be noticed however. The salt in which Gauhe sought to verify the composition of the double sulphate was plainly a mixture of "acid" and "neutral" salt. He says, "after some days small amounts deposited." This was before Erdmann's paper had appeared.

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* Researches on Ammonia cobalt bases, p. 49.

† Zeitsch. Anal. Ch., iv, p. 56.

Again, Gauhe will find that Gibbs and Genth never even mentioned the use of $(NH_4)_2\text{Ȼ}O_3$ in this connection, so that his experiments were plainly made under different conditions from those proposed.

2d. Determination of Ȼo. I prefer to determine Ȼo as neutral ȻoS̶Θ_4. We have the advantage here of determining the Ȼo in the form of a compound of higher atomic weight, thus lessening our errors of analysis. The cobalt is precipitated from the solution of the double sulphates as ȻoS̶, washed with hot sulphid of ammonium water, dried and roasted and then converted into sulphate with the aid of aqua regia and sulphuric acid. The results are good, although not so exact as in the case of the fusible double sulphates. I also determined Ȼo as metal by H after its precipitation as hydrated oxyd by Cl and acetate of soda, a method proposed by Popp.[*] Instead of Cl I found it far more convenient in practice to use a strong solution of $Cl_2\Theta_4$, which is a powerful oxydizing agent, and as it can be kept, becomes a useful laboratory reagent. This method, however, does not always give good results, on account of the difficulty of washing the hydrated $\text{Ȼo}_2\Theta_3$ till free from alkali.

3d. Determination of the N. This was done, first by the modification of Bunsen's[†] method proposed by Dr. W. Gibbs,[‡] which space will not permit me to give in detail. The reader is therefore referred to the original paper. Suffice it to say, that for nitrates and nitrites to which it is applicable, it is an excellent method. The loss of the combustion-tube can be nothing but N and $H_2\Theta$. If, therefore, the $\text{Ȼ}aCl_2$ tube retains the $H_2\Theta$ accurately, the remainder is N. Whether, therefore, the $H_2\Theta$ is accurate, or high from hygroscopic moisture, the nitrogen must be accurate, being subject to no source of error.

The nitrogen was also determined by volume, using the Sprengel-pump. This method was first proposed by Frankland[§] for nitrogen in analyses of potable waters, but he did not apply it then to organic analysis. The application, however, had been made in this laboratory by Dr. Gibbs some months before Dr. Frankland's paper was received. A vacuum is first made in the combustion-tube by means of the mercury-pump. The delivery-tube from the pump is then connected with a Simpson's receiver and the vacuum destroyed by heating some $\text{Ȼ}\Theta_2$ placed in the anterior portion of the tube. Any excess of $\text{Ȼ}\Theta_2$ is absorbed by the $KH\Theta$ solution in the receiver. The combustion is now made. A final vacuum is then obtained by the pump and the receiver disconnected. After a short time the gas is transferred to a eudiometer-tube and measured according to the methods of gas-analysis. The results are generally good, but depend upon the vacuum obtained at the beginning and end, on the perfect combustion, and on the perfect transfer of the gas.

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[*] Ann. Ch. u. Ph., 131, 363.

[†] Ann. Ch. u. Ph., 72, 40.

[‡] This Jour., xxxvii, 350.

[§] Jour. Ch. Soc., 1868, p. 90.

In conclusion I would present my grateful acknowledgments to Dr. W. Gibbs, to whose kindness I am indebted for the selection of my subject, for the use of materials, and for many valuable suggestions during the prosecution of my work.

Cambridge, Jan., 1870.

ART. XXV.—*Notice of some Fossil Birds, from the Cretaceous and Tertiary Formations of the United States;* by O. C. MARSH, Professor of Paleontology in Yale College.

The remains of Birds described from European rocks are at present limited in the Mesozoic to the famous *Archæopteryx* of the Jurassic, and apparently a single species only from the Cretaceous; but in the Tertiary period, and especially in the Miocene and Pliocene, this class was very fully represented by many of the modern types, and numerous species have already been described by Owen, von Meyer, Gervais, Hébert, Lartet, Gaudry, A. Milne-Edwards, and other paleontologists. In this country, however, since the discovery that the three-toed footprints in the Connecticut river sandstone were probably all made by Dinosaurian reptiles, no species of birds have been included in our extinct fauna, as, with this apparent exception, none have been described from any North American strata. Recent explorations, however, have shown that remains of this class are not wanting in the later formations of the United States, and that the Cretaceous and Tertiary, especially, contain representatives of many interesting forms.

The bones of birds, as might naturally be expected, are usually the rarest of vertebrate fossils, particularly in marine deposits; but, fortunately, those parts of the skeleton most likely to be preserved are especially characteristic, so that even small portions not unfrequently admit of accurate determination, although their investigation is a work of considerable difficulty, even where ample material for comparison with recent species is available. The specimens briefly described in the present preliminary notice, although nearly all fragmentary, are in an excellent state of preservation; and most of them show strongly marked characters, which indicate at least the near affinities of the species they represent. If the interpretation of them here given be correct, they prove the existence, in this country, even before the close of the Mesozoic, of an interesting and varied group of aquatic birds, and suggest for this period a very rich avine fauna.

REMAINS OF CRETACEOUS BIRDS.

Among the fossils under consideration are specimens indicating five species of Cretaceous birds, the remains of which were found in the greensand of New Jersey, and with a single exception in the middle marl bed. The specimens are all mineralized, and in the same state of preservation as the bones of the extinct reptiles which occur with them in these deposits; and hence are readily distinguished from the remains of Post-tertiary and recent birds, which have occasionally been found near the surface in the greensand excavations of that region. In most instances, moreover, a record of the discovery places the geological horizon of the present fossils beyond a doubt.

Laornis Edvardsianus Marsh, gen. et sp. nov.

The most interesting, in many respects, of the specimens from the Cretaceous is a portion of the shaft and distal extremity of a left tibia, which indicates a species, apparently of a swimming bird, nearly as large as the common wild swan (*Cygnus Americanus* Sharpless). The specimen is well preserved, and shows that the tibia, when entire, was of medium length, and quite robust. The condyles of the distal end are broader anteriorily than deep, the inner condyle being more prominent in front, and the outer one projecting somewhat farther behind. The inter-condyloid space is wider than either condyle. The supratendinal bridge—one of the most characteristic portions of the skeleton, when present—is well ossified, and still uninjured. It is submedial in position, straight, transverse, of moderate width, and spans a deep and well defined canal, which was traversed by the extensor tendon of the toes. The outer margin of the canal is low and obtuse, as in most of the Gallinaceous birds. The inner margin is the more prominent, and just above the upper edge of the bridge it supports an elongated tubercle for the inner attachment of the oblique ligament. Opposite the bridge, on the other margin, there is a large triangular elevation, which is separated by a shallow transverse groove from the exterior condyle. To this prominence the outer end of the oblique ligament was attached. The outlet of the canal above the bridge is transversely elliptical in outline, and the one below more oval, and looking forward, and slightly outward. Just below this aperture, there is a shallow depression in the inter-condyloid space, similar in shape and position to that in the tibia of the wild goose (*Bernicla Canadensis* Boie), although rather deeper, and more oblique. The under trochlear surface is but slightly concave transversely, and has a faint median elevation, as in the tibia of the swan; but in the present specimen this is continued along the entire posterior surface, which is broad, with a slight, transverse concavity. On the lower surface of the inner condylar ridge there is a shallow notch, resembling in shape and position that in the tibia of some of the Gulls, especially the Herring Gull (*Larus argentatus* Brünn.). The ecto-condyloid surface is unusually circular in outline, and has, for the attachment of the external, articular ligament, a low tubercle, a little behind and below its central point. Just in front of this, there is a shallow groove extending directly upward. The ento-condyloid surface is moderately transverse, and has on its anterior portion, for the attachment of the internal ligament, a large irregular tubercle, which is connected by a low ridge with the prominent inner margin of the tendinal canal. A portion of the fibular ridge remains on the upper part of the specimen, and a faint indication of its continuation can be traced nearly to the ecto-condyloid surface, where it passes into a small, acute, elongated tubercle, which is just outside of the triangular prominence, already mentioned. Between these two elevations, there is a deep groove for the tendon of the short tibial muscle, and in its inner margin a small elongated foramen, leading obliquely downward and inward.

The portion of the shaft preserved is robust, and somewhat flattened in an antero-posterior direction. In the lower fourth of the bone, the transverse diameter gradually increases, and reaches its maximum at the extremity of the distal condyles. The shaft curves forward slightly just where it begins to expand above the lower condyles, closely resembling in this respect the tibia of the turkey; and it has at this point little of the marked inward curvature, characteristic of the swimming birds, but is so straight, that its median plane, if continued, would divide the trochlear surface nearly equally.

The dimensions of the specimen are as follows:—

Length of portion preserved,	70·mm·
Width of condyles in front,	23·
Depth of outer condyle,	19·1
Width of bridge at center,	5·2
Transverse diameter of upper outlet,	5·
Transverse diameter of lower outlet,	4·4
Transverse diameter of shaft where broken,	11·8
Antero-posterior diameter of shaft where broken,	9·6

A consideration of the characteristic points of this interesting fossil leads to the conclusion that it should be placed in the order *Natatores*, but additional remains will probably be required to determine its exact affinities. It shows a strong resemblance in several respects to the *Lamellirostres*, and also to the *Longipennes*, but differs essentially from the typical forms of both these groups. For the extinct genus evidently represented by this specimen the name *Laornis** is proposed. The

* Λᾶος, *stone*, and Ὄρνις, *bird*.

species may very justly be named *Laornis Edvardsianus*, in honor of M. Alphonse Milne-Edwards, of Paris, whose great work on Fossil Birds, now in course of publication, promises to place this hitherto neglected branch of paleontology on a firm foundation.*

This unique specimen was found in the greensand of the upper, Cretaceous marl bed, at Birmingham, New Jersey, in the pits of the Pemberton Marl Company, and was presented to the Museum of Yale College by the superintendent, J. C. Gaskill, Esq.

Palæotringa littoralis Marsh, gen. et sp. nov.

The present collection contains, also, some remains of wading birds from the Cretaceous, and among them are specimens indicating two species, apparently belonging to the *Grallæ*. Each of these is represented by a portion of the shaft, and the lower extremity of a left tibia, and probably by some other fragmentary, but less characteristic parts of the skeleton. The tibia of the larger species resembles in size and general form that of the European Curlew (*Numenius arquata* Linn.). The condyles of the distal end are expanded anteriorly, but in their posterior extension continue upward as acute, prominent ridges. The inner condyle is somewhat narrower than the outer. The intercondyloid space is smooth, with the exception of a faint transverse groove, and is wider than either condyle. The trochlear surface is broad, slightly concave transversely, and its median portion nearly flat, especially at the extremity. Its upper, posterior surface projects slightly beyond the face of the adjoining shaft. The ecto-condyloid surface is smooth, and somewhat concave. The supratendinal bridge is narrow, very thin, transverse, and has its outer edge on the median line. The tendinal canal is very broad and deep, and its floor nearly flat. In its general features it resembles the canal of the Herring, or Silvery Gull. The aperture below the bridge is oval in outline, and looks forward, and slightly inward and downward. On the outer margin of the canal, just above the exterior condyle, there is a triangular rugose surface, to which the oblique ligament was apparently attached. This surface projects outward, so as to produce a slight swelling at this point on the side of the shaft. The interior margin of the canal is the more acute, and just above the bridge has on its inner edge a small elongated and inflected tubercle for the attachment of the upper end of the oblique ligament.

The shaft of the tibia is slender, and has its narrowest part at the beginning of the lowest fourth, or a little above where the margins of the tendinal canal begin. From this point downward, the shaft curves slightly forward, and also expands transversely, but much more gradually than in most wading birds. The lower half of the tibia is unusually flat in front, and very convex behind. A transverse section of the broken end is broadly oval.

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* Recherches sur les Oiseaux Fossiles de la France. 4to. Paris, 1867–70.

The admeasurements of this tibia are as follows:—

Length of portion preserved, - - - -	43·5mm.
Width of condyles in front, - - - - -	9·7
Depth of outer condyle, - - - - -	8·6
Width of bridge at center, - - - - -	1·6
Transverse diameter of upper outlet, - -	2·
Transverse diameter of lower outlet, - -	1·9
Transverse diameter of shaft where broken, -	4·7
Antero-posterior diameter of shaft where broken,	3·8

The remains on which this species is established are from the Cretaceous greensand of the middle marl bed. They were discovered by Nicholas Waln, Esq., in his marl pits, at Hornerstown, New Jersey, and by him presented to the Museum of Yale College.

Palæotringa vetus Marsh, sp. nov.

The present species is indicated by the lower portion of a right tibia, which is of peculiar interest, as it apparently was the first fossil bird-bone discovered in this country. It is mentioned by Dr. Morton in his Synopsis, as "the tibia of a bird belonging to the genus *Scolopax*;"* and subsequently by Dr. Harlan, who regarded it as the femur of a bird allied to the same genus.† The specimen has since remained in the collection of the Academy of Natural Sciences in Philadelphia, but no description of it has been published, probably owing to the fact that it has generally been regarded as a recent species, accidentally introduced into the deposit where found. It is, however, thoroughly fossilized, and in precisely the same state of preservation as the remains of birds, since discovered in the same region, which are known to be of Cretaceous age, and hence its antiquity cannot now be fairly called in question.

This specimen indicates a species about half the size of *P. littoralis*, or about as large as the Hudsonian Godwit (*Limosa Hudsonica* Swain.). In general form and proportions it closely resembles the tibia just described. This is especially evident in the very gradual, transverse expansion of the lower end of the tibia; and in the broad concave trochlear surface, with no indications of a median elevation, or of the peculiar flattening of the inferior surface, which is seen in many of the Snipe family.

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* Synopsis of the Organic Remains of the Cretaceous of the U. S., p. 32, Philadelphia, 1834.

† Medical and Physical Researches, p. 280, Philadelphia, 1835.

One of the most important differences between them, however, is in the supra-tendinal bridge, which in the present specimen is broader and stouter, and the outlet below looks more directly forward. The inter-condyloid space, also, has a distinct transverse depression, which is but faintly indicated in the former specimen. The rugose surface on the outer margin of the canal is here replaced by three tubercles, the largest of these, to which the oblique ligament was doubtless attached, being nearly opposite the bridge, and the other two above and outside of it. The anterior face of the tibia above the bridge is nearly flat, with a faint median rising, which throws the true tendinal canal against its inner margin. In the previous specimen this elevation does not exist, the corresponding space being transversely concave. The ecto-condyloid surface in this species is almost flat, with a slight elevation just below its central point. The ento-condyloid face is quite concave posteriorly, but near the middle of its anterior projection has a very small tubercle for the attachment of the lateral, articular ligament.

The principal dimensions of this tibia are as follows:—

Length of portion preserved, - - - -	18·5$^{mm.}$
Width of condyles in front, - - - -	7·2
Depth of outer condyle, - - - - -	4·9
Depth of inner condyle, - - - -	6.6
Width of bridge at center, - - - -	1·8
Transverse diameter of shaft where broken, -	3·4

The differences noted above between this specimen and the tibia of *P. littoralis* render it probable that additional remains would show that they belong to distinct genera; but for the present they may very properly be placed together in the genus *Palæotringa*,[*] and the present species be named *Palæotringa vetus*. The specimen, according to the authorities already cited, was found in the marl at Arneytown, New Jersey, which would imply that it was from the lowest, Cretaceous marl bed.

Telmatornis priscus Marsh, gen. et sp. nov.

The most important of the remains on which this species is founded is the lower half of a left humerus. The specimen is in perfect preservation, and, fortunately, a very characteristic part of the skeleton. In general appearance it resembles the humerus of some of the Rail family, and the species it represents is probably related to this group of birds. Evidence of this is seen in the unusual flattening of the lower part of the shaft and distal extremity; in the small articular condyles; in the diminutive protuberance for the attachment of the extensor muscle of the hand; and in the oval impression of the anterior brachial muscle. The latter has, however, in this

[*] Παλαιός, *ancient*, and Τρύγγας, *a shore bird*, mentioned by Aristotle.

specimen a more oblique position than in most of the *Rallidæ*. It resembles the corresponding impression in the woodcock, (*Philohela minor* Gray), but is situated higher up, and its superior margin is less distinctly defined. The distal end of this humerus resembles in its main features that of the Green Heron (*Butorides virescens* Bonap.), but differs widely from it in being much more compressed, and in having the imprint of the anterior brachial muscle much shorter, and more oblique.

The most important dimensions of this humerus are as follows:

Length of specimen preserved, - -	$25^{\rm mm.}$
Vertical diameter of distal end, - -	11·2
Transverse diameter through radial condyle,	5·6
Depth of shaft where broken, - - -	4·4
Transverse diameter of shaft where broken, -	3·6
Length of impression of anterior brachial muscle,	5·8

These remains apparently indicate a genus of birds distinct from any yet described, for which the name *Telmatornis*[*] is proposed. The present species, which was about the size of the King Rail (*Rallus elegans* Aud.), may be called *Telmatornis priscus*. The remains of the species at present known were found in the Cretaceous greensand of the middle marl bed, in pits of the Cream Ridge Marl Company, near Hornerstown, New Jersey, and were presented to the Museum of Yale College by John G. Meirs, Esq.

Telmatornis affinis Marsh, sp. nov.

This species is also based, essentially, on the lower portion of a right humerus, which was found at the same locality, and in the same stratum as the one just described. It closely resembles that specimen in most important particulars, but a careful comparison shows some points of difference which appear to justify its separation. The most apparent of these are the following:—The notch between the radial and ulnar condyles is somewhat deeper; the elongated tubercle, on the inner surface behind the notch, which confines the upper tendon of the triceps muscle, is larger; the impression of the anterior brachial muscle on the outer surface is higher up, and more shallow; and the epitrochlear elevation is more prominent. The specimen indicates, moreover, a species considerably smaller than the one mentioned above. Its principal measurements are as follows:—

Length of portion preserved, - - - -	$12^{\rm mm.}$
Vertical diameter of distal end, - - -	10·4
Transverse diameter through radial condyle, -	4·8
Vertical diameter of shaft where broken, -	5·6
Length of impression of anterior brachial muscle,	5·8

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[*] Τέλμα, *marsh*, and Ὄρνις, *bird*.

These remains, also, were found by John G. Meirs, Esq., near Hornerstown, New Jersey, and by him presented to Yale College, in behalf of the Cream Ridge Marl Company.

REMAINS OF TERTIARY BIRDS.

The few remains of birds hitherto discovered in the Tertiary deposits of the United States naturally show a much closer resemblance to recent species than those from the Cretaceous. In the specimens examined by the writer during the present investigation, which include, it is believed, all the remains of importance known from the formation in this country, no characters implying genera distinct from existing birds are apparent, and some of the fossils seem to indicate species nearly related to those now living. Future discoveries, however, will doubtless disclose more important differences between faunæ so remote in time.

Puffinus Conradi Marsh, sp. nov.

The collection of the Academy of Natural Sciences in Philadelphia has for many years contained the distal half of a left humerus, and the lower portion of a right ulna, of an aquatic bird, which were discovered in the Miocene of Maryland by T. A. Conrad, Esq. A brief mention of these specimens, and of some other ornithic remains from the United States, has already been made by Professor Leidy,* but no description of them has yet been published. The specimens are so well preserved, and so characteristic, especially the humerus, that the affinities of the species they indicate can be determined with tolerable certainty. The most marked feature of the humerus is the transverse obliquity of its shaft and distal extremity. Both are much compressed, and so turned that the common plane of their longer diameters, instead of being nearly vertical, as in the brachium of most birds, is here highly inclined inward and downward. Among the other characters of importance may be mentioned, the unusually small size of the ulnar condyle, the very deep, oval impression for the attachment of the anterior brachial muscle, and the presence of an elongated, compressed apophysis, extending outward and upward from the exterior margin of the distal end, just in front of the radial condyle.

This humerus has the following dimensions:—

Length of portion preserved, - - -	49· mm.
Vertical diameter of distal extremity, -	13·2
Transverse diameter of radial condyle, - -	8·6
Transverse diameter of ulnar condyle, -	3·8
Length of impression of anterior brachial muscle,	5·6
Breadth of impression of anterior brachial muscle,	3·8
Longer diameter of shaft where broken, -	7·4
Shorter diameter of shaft where broken, - -	5·

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* Proceedings Acad. Nat. Sciences, Philadelphia, 1866, p. 237.

A comparison of the present fossils with the corresponding parts of recent birds readily shows that the nearest allies of this extinct species must be sought in the Auk family, or among the Petrels; as it is only in these groups of birds, that the peculiar obliquity of the humerus, noticed above, exists. In the *Alcidæ*, however, this oblique compression is greater than in the present specimen. The latter has, moreover, on its outer edge above the radial condyle, the long, pointed projection, which is not seen in the Auks, although present in the Petrels, Gulls, and some of the wading birds. The difference in size between the ulnar and radial condyles, and the remarkably deep, oval, impression for the attachment of the anterior brachial muscle show unmistakably that this humerus belongs to one of the Shearwaters, and apparently should be placed in the genus *Puffinus*, with which it corresponds in all essential particulars. In size and general features it apparently resembles most nearly the brachium of the Cinereous Petrel (*Puffinus cinereus* Gmelin, sp.), of the Pacific coast, but there are some points of difference between them which clearly imply that the species are distinct. The flat apophysis on the outer edge of the distal extremity is in the fossil specimen more pointed; the impression, on the lower surface, of the anterior brachial muscle is deeper, and its outline more sharply defined, which is also the case with the small epicondylar depressions for the attachment of the muscles of the forearm. The bone indicates, moreover, a somewhat smaller bird.

The distal half of the right ulna, which was found with the humerus, apparently belonged to a bird of the same species, although its size would seem to indicate that it pertained to a smaller individual. The species represented by these remains may appropriately be named for its discoverer, T. A. Conrad, Esq., who has been so long and honorably identified with American paleontology.

Catarractes antiquus Marsh, sp. nov.

Among the other bird remains in the Museum of the Philadelphia Academy, is a very perfect left humerus, from Tarborough, Edgecombe county, North Carolina, which was presented by Dr. Booth. The geological position of the specimen is not definitely known, but its state of preservation and mineralization render it extremely probable, at least, that it is from the Tertiary deposits of that region. This humerus shows the same transverse obliquity which characterized the specimen just described, and so strongly resembles in other respects also the same bone in the Auks, that it should evidently be referred to that family. It approaches most nearly the humerus of the Guillemots, especially those now included in the genus *Catar-*

ractes, with which it appears to coincide in all important characters.

The principal dimensions of this humerus are as follows:

Total length, - - - - - -	96·2$^{\text{mm.}}$
Transverse diameter of proximal end, - -	21·
Vertical diameter of articular head, - -	14·2
Transverse diameter of articular head, - -	8·2
Vertical diameter of distal end, - - -	13·8
Longer diameter of shaft at center, - -	8·4
Shorter diameter of shaft at center, - -	5·

The present specimen shows so many points of resemblance to the humerus of the Thicked-billed Guillemot (*Catarractes lomvia* Linn.), that it will probably find in this species its nearest living representative. It indicates, however, a somewhat larger bird; and on carefully comparing the humeri of the two, some marked differences may be detected, which are quite sufficient to prove the species distinct. The most important of these are the following:—The head of the humerus in the fossil specimen is more obtusely rounded, both transversely and vertically; the two grooves, on the inner surface of the distal extremity, for the tendons of the triceps muscle, are of nearly equal width, the upper depression being somewhat wider than the other, but in *C. lomvia* Linn. the lower groove is much the broader; the ulnar condyle has, moreover, on its interior surface, a small obtuse tubercle, extending upward and inward, as in the humerus of the Great Auk (*Alca impennis* Linn.), while this projection is wanting in the Thicked-billed Guillemot; the lower face of the same condyle also differs from that of the latter species in forming a much narrower crescent; and in several minor points of structure a similar want of correspondence may be seen.

A right humerus, closely resembling the preceding specimen, and evidently belonging to the same genus, was presented to the Philadelphia Academy a few years since by Dr. A. C. Hamlin, who obtained it in the Post-tertiary clays, near Bangor, Maine, at a depth of forty-seven feet below the surface. It appears to be distinct from the above species, as well as from *C. lomvia* Linn., and will be described at an early day by the writer, in a paper now in course of preparation on the remains of some Quaternary birds.

Grus Haydeni Marsh, sp. nov.

The various explorations that have been made in the Tertiary deposits of the Upper Missouri region, so remarkably rich in mammalian remains, have, strange to say, brought to light but a single fragmentary specimen which can with certainty be referred to the class of birds; although the material collected there has been carefully examined for these fossils by several paleontologists. The only specimen hitherto detected was obtained by Dr. F. V. Hayden, several years since, in the later Tertiary beds of the Niobrara river. It is the distal extremity of a left tibia, one of the most common, and most characteristic parts of fossil birds, and indicates unmistakably a large species of the genus *Grus*, or Cranes. Although the specimen is somewhat injured, the most important parts are well preserved, and appear to exhibit good distinctive characters. The inner articular condyle is partially broken away, but was evidently much narrower than the other, and is continued far backward as a very sharp ridge. The outer condyle is somewhat flattened below, and its posterior extension projects beyond the surface of the shaft, although not so far as that of the inner condyle. The trochlear space is rather narrow, and has its deepest part inside of the median line. It is slightly concave transversely below, and deeply so posteriorly. The ecto-condyloid surface is concave, but has a low tubercle just in front of its central point. The supratendinal bridge is very broad, internal, transverse, and its surface is concave vertically. It spans a very narrow, but deep, internal canal. The lower outlet is subtriangular in outline, and looks obliquely forward and downward. The superior aperture is broadly oval, and the upper edge of the bridge is continued slightly above it on either side of the canal. The lower opening has its upper, straight margin slightly rounded, and its lower edge is formed by a sharp ridge, which separates it from the nearly flat intercondyloid space. External to this aperture is a prominent tubercle, which has its inner edge on the median line, and is connected above by a low ridge with the outer elongated tubercle for the attachment of the oblique ligament. A more prominent crest extends obliquely downward, and unites it with the external condyle. The inner margin of the canal is bounded by a well defined ridge, which, just above the bridge, is inflected over the edge. Externally, the margin of the canal is low, and indistinct. The groove for the tendon of the short tibial muscle is well defined, and bounded outwardly by a low ridge, which causes a projection on the exterior surface of the shaft.

The principal dimensions of this specimen are as follows:—

Length of portion preserved, - - -	31· mm.
Depth of external condyle, - -	19·2
Width of supratendinal bridge, - - -	7·2
Width of upper outlet, - - -	2·4
Width of lower outlet, - - -	3·6
Transverse diameter of shaft where broken, -	12·6
Antero-posterior diameter of shaft where broken,	8·2

This specimen, also, belongs to the Academy of Natural

Sciences in Philadelphia, and the extinct species it indicates is named for Dr. F. V. Hayden, whose explorations have added so much to our knowledge of the geology of the Upper Missouri, and Rocky Mountain regions.

Graculus Idahensis Marsh, sp. nov.

A collection of Tertiary fossils from Idaho, lately received by Professor Newberry, of Columbia College, contained the greater portion of the left metacarpal bone of an aquatic bird, which he has kindly loaned to the writer for examination. The specimen is in perfect preservation, and has such marked characters that it will evidently admit of at least approximate determination. Among the most prominent features of the fossil, at its proximal extremity, are, the great anterior projection of the radial apophysis, which is also elevated, and its superior portion considerably compressed; the deep anterior carpal fossa, which extends into the base of the carpal articular surface; the very deep and narrow posterior carpal fossa, which is auriform in outline, and extends obliquely inward and forward; and the internal and oblique position of the smaller metacarpal bone. The pisiform tubercle on the inner surface is of medium size, and its summit nearly flat. A sharp ridge extends from its anterior edge directly upward to the margin of the articular surface. The groove in front of this tubercle is very deep and broad. The fossa for the attachment of the inner lateral ligament of the wrist is also deep, and has, apparently, a small pneumatic opening near its center. The smaller branch of the metacarpal bone was slender, and but little separated from the larger one. Its outer edge at its superior attachment is on the median line, and opposite to this point, on the outer posterior edge of the large metacarpal, there is a small tubercle, to which the superior flexor muscle of the hand was attached. The lower extremity of the specimen has, unfortunately, been lost.

The principal dimensions of this metacarpal bone are as follows:—

Length of portion preserved, - - - -	45·mm·
Transverse diameter of proximal extremity, -	16·2
Diameter through pisiform tubercle, - - -	8·1
Width of carpal articular surface, - - -	7·2
Length of radial apophysis, - - - -	12·
Distance from inner superior edge to union of metacarpals, - - - - - - - -	18·6
Greater diameter of large shaft where broken,	5·4

The species represented by this fossil appears to have been related to the Cormorants, and may be placed provisionally in the genus *Graculus;* the metacarpal of which the present specimen resembles in nearly all important particulars. The most marked difference between them is the presence in the latter of the anterior carpal fossa. This interesting specimen, the only fossil bird bone yet found west of the Rocky Mountains, is from a fresh-water Tertiary deposit, probably of Pliocene age, on Castle Creek, Idaho Territory.

In addition to the acknowledgments made in the course of the present article, the writer desires, in conclusion, to express his grateful thanks to Professor Joseph Leidy, of Philadelphia, who generously placed in his hands for examination the various bird remains already mentioned as belonging to the Academy of Natural Sciences in that city. A more complete description, with illustrations, of all the remains here briefly noticed, and a determination of their nearer relations to living species, as well as the conclusions their discovery suggests, are reserved for a future communication.

Yale College, New Haven, Conn., Feb. 1st, 1870.

Art. XXVI.—*Contributions to Zoölogy from the Museum of Yale College.* No. VI.—*Descriptions of Shells from the Gulf of California;* by A. E. Verrill.

Having in preparation faunal catalogues of the extensive collections of mollusca in the Museum of Yale College, collected in the Gulf of California by Capt. J. Pedersen, and at Panama, San Salvador, Peru, and other localities on the West Coast of America by Prof. F. H. Bradley, it seems useful to notice here some of the more interesting and new species. These will hereafter be more fully described and figured in the Transactions of the Connecticut Academy. Most of the following species were obtained by pearl divers near La Paz.

Semele Junonia Verrill, sp. nov.

A large, broad, somewhat oval species; anteriorly much prolonged and broadly rounded; posteriorly shortened, strongly plicated, the extremity broadly obliquely truncate. Surface covered with regular, nearly equidistant, elevated, strong, concentric lamellæ, which are not close, except near the umboes, but beyond the plication become fainter, oblique, and parallel with the truncated posterior edge; interstices broadly concave, smooth, with fine radiating grooves in the larger specimens, and also sometimes with concentric striæ. Epidermis thin, shining, yellowish brown. Beaks but little prominent; dorsal edge of the right valve overlapping the ligament and even more overlapping the edge of the left valve in front of the beaks. Hinge stout and broad, its inner edge very sinuous;

the internal ligament strongly divergent from the margin, its plate obliquely concavely truncate posteriorly; lateral teeth stout, obtuse. Palial sinus large, broadly rounded, reaching about the middle, its surface finely striated and iridescent. The color of the interior is usually a rich pink or deep flesh-color, lighter about the hinge; sometimes yellowish or bright orange; externally generally orange-yellow, with the umboes bright orange-yellow. One large specimen is pale yellow, with bright lemon yellow umboes; the inside yellowish white with the palial sinus and broad concentric bands pinkish.

Length,	**3·15**	**inches.**	**2·55**	**2·35**	**1·44**
Height.	**2·65**	**"**	**2·20**	**2·05**	**1·16**
Breadth,	**1·40**	**"**	**1·12**	**·90**	**·52**
Apex to anterior end,	**2·10**	**"**	**1·86**	**1·60**	**1·00**
Plication to posterior end,	**·80**	**"**	**·60**	**·50**	**·32**
Palial sinus from edge,	**1·85**	**"**	**1·50**	**1·38**	**·85**
Breadth of do.,	**·87**	**"**	**·74**	**·62**	**·32**

Near La Paz,—Capt. J. Pedersen. Six fresh specimens, the valves mostly together. Obtained by pearl divers.

This fine species is allied to *S. rosea* and *S. Jovis*. The latter, as figured by Reeve (Icon., vol. viii, Pl. v, 34), is similar in form and somewhat in color, but the lamellæ are closer, and the plication is nearer the posterior edge. The hinge and palial sinus are not described, and the locality is unknown.

S. rosea (Sowerby, 1832) was described from a single valve found at Tumbez, Peru. It is more orbicular and the lamellæ are much closer.

Semele formosa.

Amphidesma formosum **Sowerby, Proc. of the Committee of Science and Correspondence of the Zoöl. Soc of London, Part II, 1832, p. 199; Reeve, Icon., viii, Pl. iv, fig. 27.**

Semele formosa **Adams, Genera, ii, p. 411.**

Of this lovely species two fresh specimens with the valves together and two fresh odd valves were obtained with the preceding.

The form is much like that of *S. Junonia*, but the anterior dorsal edge is not concave, does not overlap, and the beaks are, therefore, even less prominent. The sides are covered with close, more or less irregular, often slightly oblique, concentric, rounded ribs, which are often furcate, especially at the posterior fold, separated by intervals of about their own width, and become very fine and close toward the apex, and often interrupted and nodulose on the umboes and anteriorly; beyond the strong posterior plication they are stouter, irregularly bent up and crowded, toward the apex rugose. Hinge less stout than in the preceding, the internal ligament more parallel and nearer to the margin, its plate not truncately terminated. Palial sinus large, broadly rounded, iridescently striated.

The color is varied with white, light lemon-yellow, rosy, brown, and purple; these colors being arranged partly in numerous rays, partly in spots and patches. Usually there are many alternating narrow rays of white and rosy, the other colors being more irregularly distributed, but in one specimen there are also regular rays of yellow on one valve. Inside the larger specimens are pinkish white, the middle and umbonal regions bright yellow, the ligament plate deep purple; but smaller specimens are mottled with purplish red and yellow.

The largest specimen is 2·75 inches long; 2·25 high; 1·05 broad; the palial sinus 1·62 long, from edge; ·85 wide.

Semele venusta (A. Adams, 1853) is perhaps only the young of this species. It was from West Columbia.

Callista pollicaris.

Carpenter, Ann. and Mag. Nat. Hist., vol. xiii, p. 312, 1865.
Dione prora var., **Reeve, Conch. Icon., xiv, Pl. x, fig. 45, (*non* Conrad).**

Several specimens of this rare and beautiful species were obtained, which show considerable variation, especially in color. Most of these are more transversely oval and less rounded below than the specimen figured by Reeve. The posterior end is always a little compressed and somewhat obliquely truncate. Ligamentary area long and narrow, slightly excavate, wrinkled. The sides are smooth and polished, finely concentrically striated; posteriorly and anteriorly obliquely corrugately wrinkled in most of the specimens, but in some, especially the smaller ones, the anterior wrinkles are obsolete, and sometimes also the posterior ones. Palial sinus large, broad and oval. Some specimens are white, with only a few small specks or waved lines of orange-brown, and a few stripes of the same color on the ligamentary area, the apex orange, upper part of lunule brown; more commonly the color is yellowish with concentric waved or zigzag streaks and spots of orange-brown, sometimes with imperfect radiating bands of the same color in addition; a large specimen is thus marked for about an inch from apex, beyond which it is white, faintly specked with brown. One large specimen is nearly white with many radiating bands of orange-brown on one valve; on the other diffused orange-brown, lighter above, crossed by many radiating darker brown bands. Another is darker chestnut-brown below, yellowish white toward the umboes, but destitute of bands and spots. Some of the smaller specimens have a purplish brown stain within, crossing the palial sinus; others are pure white.

Length,	2·10	inches.	2·08	1·75	·88
Height,	1·50	"	1·45	1·25	·66
Breadth,	1·00	"	1·00	·80	·44
Apex to posterior end,	1·75	"	1·68	1·35	·68
Palial sinus from edge,	1·15	"	1·10	·95	·46

Near La Paz,—Capt. J. Pedersen.

This species belongs to the subgenus, *Caryatis*, of Römer.

Tivela elegans, sp. nov.

The form is regular, transversely subelliptical, triangular above, nearly equilateral, slightly swollen at the umboes; the beaks but little prominent. Ligament short; lunule elongated, distinctly defined. Hinge narrow, not very strong. In the left valve the anterior tooth is prominent, about its own breadth from the beak; four principal cardinal teeth, nearly equally divergent, rather thin, the posterior one bilobed, the marginal lobe prolonged; in the right valve four principal, thin cardinal teeth, the middle ones most prominent, rugose, palial sinus large, broadly rounded, reaching nearly to the middle.

The La Paz specimens show great variation in color. Some are white with a few posterior oblique lines or spots of reddish brown; others are broadly indistinctly rayed and posteriorly wave-striped with lighter and darker grayish brown, leaving the umboes and two narrow radiating bands yellowish white. One is intricately and beautifully painted with rich reddish brown, in regular, concentric, waved and angulated bands, becoming zigzag posteriorly, with several median interrupted white rays, which differ on the two valves, and the posterior edge stained deep brown; this is deep purple within, except apical and anterior and posterior whitish spots. The lighter specimens are white within, except a spot of purplish about each muscular scar.

The San Salvador specimens present many other variations in color, but are mostly white, more or less profusely painted with chestnut-brown in various concentric, more or less waved and angulated bands, or in interrupted rays; in some the color is yellowish brown; in others the ground-color is reddish brown, with darker bands. The beaks are sometimes, but not usually, purplish; the interior generally white, with a purplish umbonal stain.

		T. elegans.			*T. Hindsii.*
Length,	1·02 inches.	·98	·82	·74	·65
Height,	·78 "	·80	·65	·58	·62
Breadth,	·48 "	·50	·42	·40	·50

La Paz,—Capt. J. Pedersen; Acajutla and Realejo,—F. H. Bradley.

This species varies somewhat in form, but is nearly always more traversely oval than *T. Hindsii*, of which we have typical specimens both from San Salvador and Zorritos, Peru, which agree exactly in form and color with Reeve's figures. *T. Hindsii* is short, triangular, with very swollen umboes, and the posterior end longest and a little produced, its sinus much smaller, and the ligament very short, the hinge stouter, with the lateral teeth nearer the cardinal, and the latter more crowded and stouter. *T. radiata* is less regular in form than *T. elegans*, with much smaller sinus and broader and stouter hinge, a much longer ligament, sharper lateral tooth, and more numerous and more divergent cardinal teeth, the posterior ones being much more elongated.

The name, *Trigona* (Meg., 1811), is not only *later* than *Tivela* (Link, 1807), but was previously used among Hyemenoptera (Jur. 1807). It has also been used in Crustacea (Latr., 1817).

Venus isocardia, sp. nov.

A large, rounded, thick, and swollen species, cordate in front, with a broad deeply excavated lunule; with the sculpture entirely concentric,—the stout, elevated, rather close, slightly recurved and flattened ribs separated by deep interstices in which there are several very thin, crowded, slightly elevated lamellæ.

Umbòes prominent, swollen, the beaks much recurved, not marginal; dorsal outline convex, broadly rounded and a little produced posteriorly; evenly rounded ventrally; anterior end short, deeply indented by the very broad and sunken lunule, which is smoothish and surrounded by a distinct groove, broadly cordate, extending between the beaks. Ligamentary area narrow, smooth on the left valve, the concentric sculpture extending over it on the right valve, which overlaps beyond the ligament. Muscular scars and palial line strongly marked, the sinus of moderate length, tapering to an obtuse point, about as deep as the width of the posterior muscular scar. Hinge stout, the anterior tooth in the right valve elevated and stout; central much larger, slightly bilobed; posterior one much elongated, and strong. In the left valve there is a small, conical, tubercular, anterior tooth; first cardinal elevated, stout triangular; followed by a stout strongly bilobed one; posterior one confluent with the ligament plate, long and curved, less elevated.

Exteriorly more or less stained and blotched with brownish; interiorly tinged with light orange near the umboes.

Length 3·25 inches; height 3·30; breadth 2·50; breadth of lunule ·68; length of palial sinus ·60.

Near La Paz,—Capt. J. Pedersen. Two fresh specimens.

This massive species is allied to *V. rugosa* of the West Indies, which it resembles in form and sculpture, but it has a different and stronger hinge. The posterior tooth of the right valve, especially, is much larger and longer, extending beyond the middle of the ligament. In the left valve the posterior tooth is also much elongated and reaches beyond the middle of the ligament, but it is much less elevated than in *V. rugosa*, and less separated from the ligament plate, there being only a shallow groove between.

It belongs to the typical *Venus*, as restricted by Messrs. H. and A. Adams and most recent authors, but Römer has given the subgeneric name, *Ventricola*, to this group.

Chione tumens, sp. nov.

A stout, thick, often swollen, subtriangular, somewhat beaked species, with broad, swollen, rounded, concentric ridges or undulations, increasing in size from the beaks to the margin, with faint radiating sculpture on the upper side of the ridges. The dorsal outline is nearly straight, the ligament groove broad and deeply excavated, smoothish in both valves, with a short ligament; posteriorly slightly truncate; ventrally broadly rounded; anteriorly rounded and a little produced; the lunule broad-oval, deeply sunken, smoothish, or marked with slight radiating lines. Beaks a little recurved, marginal. The concentric ridges or swellings are 12 or 14 in number, those near the beaks quite small and close, but rapidly becoming broader and more elevated; so that the last three occupy an inch or more in breadth; their upper surface is somewhat flattened and marked by low, crowded, radiating ridges, which are obsolete, or nearly so, on their outer side. The hinge is stout, its inner edge sinuous; the small anterior tooth in the right valve is thin and but slightly elevated; the central one stout, triangular and elevated; in the left valve the anterior tooth is stout, somewhat triangular and acute, as much elevated as the central tooth, but not so stout. Palial sinus quite small. Color externally, whitish, yellowish, or light brownish, variously marked and blotched with dark brown, which is sometimes in large radiating bands or spots, intermixed with narrow angular lines; at other times the brown markings are so numerous and crowded as to nearly conceal the ground color; within whitish, or with a dark purple stain posteriorly.

Length,	1·60	inches.	1·48	inches.
Height,	1·55	"	1·35	"
Breadth,	1·20	"	1·10	"
Apex to posterior end,	1·45	"	1·25	"

La Paz,—Capt. J. Pedersen. Forty-two specimens, mostly odd valves.

This species is very distinct from all others in its form and peculiar broad swollen ridges. It slightly resembles *C. subimbricata*, especially in color, but in the latter the first ridges are lamellar and the later ones much smaller, more numerous, and less swollen, while the shell is less beaked, the umboes less swollen, and the anterior edge is much less recurved and shorter, but the posterior more pointed. The hinge in *C. tumens* is also much broader and stouter, with the inner edge much more sinuous and the teeth longer and stouter.

Chione succincta.

Venus succincta Val., in Humb. Rec. d'Orbs., vol. ii, Pl. 48, fig. 1, p. 219, 1833, (t. P. P. Carpenter).

V. leucodon Sowerby, Proc. Zoöl. Soc. Lond., p. 43, 1835.

V. Californiensis Broderip, op. cit., p. 43; Reeve, Icon., Pl. xi, fig. 35, (*non V. Californiana* Con.).

V. Nuttallii Conrad, Jour. Acad. Nat. Sci. Phil., vii, p. 250, Pl. 19, fig. 16, 1837; Reeve, Icon., Pl. xiii, fig. 49, 1863.

Chione succincta Carpenter, Rep. Brit. Assoc., 1863, pp. 569 and 620.

A fine series of this species is contained in the collection, including about fifty fresh specimens with valves together, and over a hundred odd valves of all sizes. These show considerable variation and confirm the synonymy of Dr. Carpenter, as given above.

In this species the shell, when adult, is thick and heavy, generally white, or nearly so, externally and internally, except that there is usually a deep purple stain posteriorly within, and often two spots or short triangular radiating bands near the apex, which is often purple or brown at tip. The form is somewhat triangular ovate, with the umboes and beaks quite prominent and recurved. The lunule is sometimes brown, generally narrow and ribbed; the ligamentary area is excavate, smooth on the right valve, and often nearly so on the left, though more commonly the concentric ribs extend around upon it in the form of crowded, more or less prominent wrinkles or slight folds; it is generally tinged with light brown or purple, on the right valve sometimes striped transversely with deep brown.

The sculpture is quite variable in the prominence and distance between both the radiating and concentric ribs, especially the latter. Over the umboes the radiating ribs are strong, and either alternate with smaller ones, are arranged by twos or threes, or are nearly uniform for some distance; posteriorly they become obsolete or nearly so, and in large specimens they gradually fade out and disappear at 2 to 2·5 inches from the apex, where the concentric ribs generally begin to become stronger, crowded, and more recurved. The concentric ribs on the umbonal region are generally ·10 to ·15 of an inch distant, toward the apex closer, and toward the base closely crowded, though not always so. The hinge is very strong, the teeth large, and the palial sinus very small.

Some of the larger specimens give the following measurements:

Length,	**2·85**	**inches**	**2·65**	**2·75**	**2·60**
Height,	**2·85**	"	**2·70**	**2·75**	**2·55**
Breadth,	**1·65**	"	**1·60**	**1·70**	**1·60**

La Paz,—J. Pedersen. It appears to live buried in sand or mud with only the posterior end exposed, which is therefore more or less worn and discolored in large specimens.

Chione undatella.

Venus undatella Sowerby, Proc. Zoöl. Soc. Lond., iii, p. 22, 1835; Reeve, Icon., xiv, Pl. 16, fig. 68.
V. neglecta Gray, Voyage Blossom, p. 151, Pl. 41, fig. 8, 1839, (t. P. P. C.).
V. simillima Sowerby, Thes. Conch., xvi, p. 708; Reeve, Icon., fig. 44.
V. perdix Val., Voyage, sur la frégate la Vénus, Pl. 16, fig. 2, 1846.
V. subrostrata Reeve, Icon., xiv, fig. 54, 1863, (*non* Lam.).
(?) *V. bilineata* (*pars*) Reeve, Icon., Pl. 22, fig. 105 a.
Chione undatella Desh., Catal. Veneridæ of Brit. Mus., p. 141.

A large series of this variable species was obtained, which shows that several nominal species have been based on characters that have no constancy. In its most normal condition it is more swollen, less triangular above, with the anterior end less produced, and the beaks much less prominent than the preceding. The sculpture, though variable, is similar, but generally the concentric ribs are closer and more regular. The lunule and ligamentary area are similar, but the latter is perhaps generally rougher on the right valve or both. The hinge is scarcely different, though somewhat variable with age, but the posterior tooth is perhaps generally somewhat longer, and the inner edge of the hinge-plate a little more sinuous. The palial sinus, as in the preceding, is very small. The color is quite variable. It is rarely perfectly white; more commonly externally buff or light cream-yellow, with transverse, waved or zigzag irregular stripes and radiating, often interrupted bands and irregular patches and spots of brown, sometimes also of purplish. Sometimes the brown markings are very light and scarcely distinct, the general color being buff; sometimes the markings are all regularly deeply angulated or zigzag lines, except on the umboes, which are blotched with brown; the apex is often bright brown or purplish; the broad radiating bands are frequently three or four in number and very distinct; many are merely mottled and spotted. Internally there is generally at least a more or less extended posterior purple stain; frequently the whole interior is tinged with a rich reddish purple, deepest outside of the palial line, with the hinge teeth bright red or lilac; sometimes the interior is pure white. The lunule and ligamentary area are usually stained or spotted with brown, the latter often with regular transverse stripes on the left valve.

In the *var. simillima* the radiating ribs are much finer posteriorly, fimbriating the concentric lamellæ, but there is also some appearance of this in the typical form, and some specimens have this form of sculpture for an inch from the apex, beyond which it fades out and the lamellæ are smoothish, or but slightly wrinkled.

Length,	**2·05**	**inches.**	**1·95**	**1·90**	**1·80**
Height,	**1·95**	**"**	**1·85**	**1·55**	**1·65**
Breadth,	**1·30**	**"**	**1·30**	**1·05**	**1·10**

La Paz,—J. Pedersen. With the preceding.

This species is very closely allied to *C. succincta*, and although the ordinary adult specimens are quite different in form and color, there are specimens which are, to a considerable extent, intermediate. It is also nearly impossible to satisfactorily separate young specimens, half an inch in diameter or smaller.

Papyridea bullata Sw. *var. Californica*, nov.

In form and color like the ordinary West Indian specimens, but perhaps a little more elongated than the average Atlantic form. Sculpture also similar in all respects, except that the radiating ribs are more prominent in the middle and especially over the umboes. The hinge shows some differences. In both valves the anterior lateral tooth is less prominent, narrower, and smaller, and the ligament plate is more prolonged and not so squarely truncate, reaching to or beyond the center of the posterior lateral tooth. The color externally is generally yellowish-white variously mottled and spotted with purplish brown; internally white, more or less stained with purple; one specimen is mottled with orange externally, and stained within with the same color, like some of the West Indian. The largest specimen is 1·75 inches long and 1·38 high.

La Paz,—J. Pedersen. Ten specimens.

Cardita Cuvieri.

Broderip, Zoöl. Soc., Proc. of Comm. of Science, p. 55, 1832.
Cardita Michelini Val., Voy. Vénus, Pl. 22, fig. 5, 1846.
Actinobolus Cuvieri Adams, Genera Rec. Moll., ii, p. 487, 1858.

Four large specimens of this rare and interesting species were obtained. They vary but little in form, color, or sculpture. In all the beaks are strongly recurved, umboes very prominent, the posterior depression strongly marked, the ribs very stout, flattened, transversely nodose, with deep square-cut grooves between. The color is deep mahogany brown, varying in shade; within white.

Length,	2·50	inches.	2·10	2·15
Height,	2·45	"	2·20	2·15
Breadth,	2·25	"	1·80	1·65

La Paz,—J. Pedersen. From pearl divers.

Cardita crassa.

Gray, Voyage of the Blossom, p. 152, Pl. 42, fig. 4, 1839, (t. P. P. Carpenter).
Actinobolus crassus Adams, loc. cit., p. 487.

This species appears to be more common than the last. It is more pointed, with the beaks less recurved; the posterior depression less marked; the ribs fewer, less prominent, low and rounded, with the interspaces, broad, shallow, concave, concentrically wrinkled. Color deep reddish brown; sometimes with triangular lateral, or posterior transverse spots of yellow, the beaks white.

Length,	1·90	inches.	1·95	·65
Height,	2·20	"	2·10	·67
Breadth,	1·05	"	1·50	·50

La Paz, with last,—J. Pedersen.

Loripes edentuloides, sp. nov.

Closely allied to *L. edentula* of the West Indies and Gulf of Mexico.

It is subglobose, and much more swollen than *L. edentula.* The apex is more prominent and curved, and the lunular region more deeply excavated. The ligament is shorter and its supporting plate is not so stout, and its inner edge but little elevated above the ligament groove.

Length 1·65; height 1·50; breadth 1·10.

La Paz,—J. Pedersen. One specimen.

This is of special interest as another Gulf of California shell closely allied to a characteristic West Indian species. A large series of specimens might, perhaps, connect the two forms, but at present it seems necessary to keep them separate.

Xenophora robusta, sp. nov.

Shell large, elevated, regularly conical. Whorls seven or more, overlapping, bearing large pebbles and fragments of shells and corals intermingled, exposed surfaces roughly corrugately wrinkled, much crowded. Base broad, concave, densely and finely corrugated, spirally rudely costate by the lines of growth; yellowish brown. Aperture large, the inner lip and columella with a thick, lustrous, deep brown callus, which extends into the shell. Umbilicus closed by the reflexed inner lip.

Height,	2·35	inches.	1·70
Breadth,	2·75	"	2·20
Length of last whorl,	·85	"	·80

Near La Paz,—J. Pedersen. Two fresh specimens.

One of the specimens bears some large fragments of a shell that appears to be *Chione undatella*, the sculpture and color being preserved.

This genus appears to have been previously unknown on the west coast.

Enæta Pedersenii, sp. nov.

Shell small, rather slender, elongated; the spine regularly conical, acute, about two thirds the length of the body whorl; each whorl much flattened below the suture and encircled by a row of rounded tubercles; the body whorl with low, rounded, longitudinal costæ below the tubercles. Whole surface finely longitudinally sulcated or striated, on the upper whorls also transversely striated. Aperture narrow, contracted above, the outer lip much thickened, the edge subreflexed, bearing at about the upper third a stout tubercle, below which it is crenulately toothed within; contracted at base by another small tubercle. One specimen, probably immature, lacks the tubercles and crenulations; columella with about seven plaits, the four lowest largest. Siphon narrow, a little prolonged and recurved, with acute edges. Color fulvous brown, specked with bluish white, with an interrupted band, or spots, of deep brown below the suture, a pale band over the tubercles and another, bordered with brown, below the middle of the body whorl. Length 1 inch; breadth ·50; length of body whorl ·63; length of aperture ·68; breadth ·12.

La Paz,—J. Pedersen. Five specimens.

This species is closely allied to the next, but is more slender, with the spire more acute, smaller tubercles and costæ, a more prolonged and recurved siphon, and more contracted aperture. The surface is not smooth and the color is lighter.

Lyria (*Enæta*) *Cumingii.*

Voluta Cumingii Brod., Zoöl. Soc. Proc. of Comm. of Science, 1832, p. 33.
Lyria (*Enæta*) *Cumingii* Adams, Genera, p. 167, 1858.

Our specimens are rather stout and solid, with large swollen tubercles and costæ, and a smooth polished surface. The best specimen lacks the tubercle on the inside of the outer lip, which is somewhat flaring and much thickened. The siphonal notch is scarcely prolonged and not recurved. The color is dark and rich brown, mottled and spotted with light bluish and flesh-color, with submedian and tubercular interrupted paler bands on the last whorl and an interrupted band of deep brown spots below the suture on the spine; interior salmon-color.

Length 1·33; breadth ·75; length of body-whorl ·85; aperture ·23 broad; ·82 of an inch long.

La Paz,—J. Pedersen. Three specimens.

Among the other species of special interest are *Codakia tigerina* (large and abundant), *Lucina undata* Carpenter, *L. excavata* Carp., in several varieties, *Mactrella exoleta*, *Tellina Cumingii* (abundant and fine), *Raeta undulata* (numerous valves), *Cyathodonta plicata*, *Placunanomia Cumingii* (three large specimens), *Conella cedonulli* (many varieties of color), *Conus* (like *textilis*), *Cypræa pulchra* Kiener (ten specimens), *Cypræa* (*Luponia*) *Sowerbyi* and *L. albuginosa* (both in considerable numbers), *Cassis tenuis* (finely colored), *Harpa crenata* (several varieties of color), *Cassidulus patulus*, etc., all from La Paz and vicinity.

Note to Contributions to Zoology, No. V.—In the last number of this Journal, page 99, the generic name, *Oreaster*, should be substituted for *Pentaceros.* This change was made in the proof, after obtaining the reference to *Pentaceros* in Cuv. and Val., but the correction was overlooked by the printer.

ART. XXVII.—*Notice of Dr. Gould's Report on the Trans-Atlantic Longitude.**

Dr. GOULD's able Report on the trans-Atlantic longitude has at last been published. It is full three years, however, since the field work to which it relates was finished, and more than two, as we learn from a prefatory note, since the paper in its present form (except the last chapter) was ready for the press. Astronomers have waited for it until patience was well nigh exhausted. Yet, thankful now that it is at last within reach, they will scarcely trouble themselves to press the question, why, or by whom, it has been so long withheld, or why it is now published by the Smithsonian Institution, and not by the Coast Survey itself, under whose auspices the work was done. For ourselves, waiving these, and like queries, as probably admitting of satisfactory answers, or, at least, such as would free the author of the Report from responsibility for the unexplained delay, we turn rather to the more welcome task of examining the Report itself, and laying some of its points before the readers of this Journal.

The paper fills a hundred quarto pages of the Smithsonian Contributions, and is one of the most important yet published on telegraphic longitude. For not only was the undertaking to which it relates among the most difficult and delicate, as well as important, of its kind, but the party put upon it brought to their task nearly the sum total of all the experience and practical skill that had then been developed in this special field; their chief having for fifteen years had exclusive charge of the longitude operations of the United States Coast Survey, and his associates, likewise, a long training in the same service. The Report may be taken, therefore, as a good exemplification of the telegraphic method in one of its latest and most difficult applications, and at the same time, as but a sample of the vast store of similar material that has been accumulating under the same hand from the entire longitude work of the Survey—material which embraced, before the war, no less than twenty-four independent determinations in the Atlantic and Gulf States—and which, surely, ought not to be much longer lost to science.

The Telegraphic method, it is now well understood, is distinctively American, and has had its chief development in the work of our great national Survey. That Survey has, in fact, among its many important contributions to science, given to the world the best methods yet devised of determining both latitude and longitude—Talcott's for the one, and the Telegraphic for the other; the former, devised in 1834, giving us in a single night's work with the zenith telescope, or a transit instrument used as such, the latitude to a small fraction of a second, and the latter, the longitude, also, for the first time in the history of geodesy, with corresponding facility and precision—a facility and precision, taken together, wholly unattainable by other methods, and in the case of longitude especially, unapproachable even, particularly in extended operations.

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* The Trans-Atlantic Longitude, as determined by the Coast Survey Expedition of 1866. A Report to the Superintendent of the U. S. Coast Survey. By Benjamin Apthorp Gould, late assistant. Washington City: published by the Smithsonian Institution, 1869. New York: D. Appleton & Co.

It was not till some years after the telegraphic method had been well elaborated here, and in successful use, that it received much attention abroad; and even then, the modes of practice recommended as original, by even eminent astronomers, particularly in France, were only such as had been long employed in our Coast Survey, or superseded by better, and had been published to the world repeatedly, and through various channels.

Almost simultaneously with the invention of the telegraph must have occurred to astronomers the idea of using it in determining longitudes. It was natural enough, therefore, that Arago should, in 1837, as is said, have suggested such a use of it to Morse. But the first actual experiment, so far as we know, was made, on the Baltimore and Washington line, in 1844, by comparison of chronometers at the two termini. The experiment was conducted by Captain Wilkes.

The method had, before this, engaged the attention of Prof. Bache, and he, quick to discern its advantages, in November, 1845, ordered its use in the longitude work of the Coast Survey—then in charge of the eminent astronomer, S. C. Walker; who, the next year, made a successful trial of it on the line between Philadelphia and Washington. From 1848, on, it was the method employed systematically in all determinations between points in the United States. Mr. Walker tried both the method of clock-signals, used in the experiment between Baltimore and Washington, and also the superior one of star-signals, used in the Coast Survey practice, almost exclusively, from the first. Dr. Gould succeeded Mr. Walker in 1851; and in the widely extended operations of the next ten years, brought the method to a very high degree of efficiency, and, with his aids, acquired that thorough mastery of the subject, which so well fitted both him and them for the difficult task, the history and results of which are set before us in this Report.

Dr. Gould's paper contains twelve chapters: 1, Origin of the Coast Survey Expedition; 2, Previous determinations of the trans-Atlantic longitude; 3, History of the Expedition; 4, Observations at Valencia; 5, Observations at Newfoundland; 6, Observations at Calais; 7, Longitude-signals between Foilhommerum and Heart's Content; 8, Longitude-signals between Heart's Content and Calais; 9, Personal error in noting signals; 10, Personal equation in determining time; 11, Final result for longitude; 12, Transmission-time of the signals.

The expedition had for its object a more exact determination of American longitudes, as reckoned from a European zero, than had yet been made. It was important, for the purposes of the Coast Survey, and of astronomical science, that the trans-Atlantic longitude should correspond in precision with those determined telegraphically within our own borders. The previous determinations of that longitude whether by moon-culminations, by eclipses and occultations, or by chronometers, differed very widely from each other. The range of difference was no less than five or six seconds in time. We take for comparison, the longitude from Greenwich, of the Naval observatory at Washington. This depends mainly on that of Cambridge, and this again, in part, on that of New York, Philadelphia, and other places. Between these points, the longitude has been measured telegraphically. That between Washington and Cambridge was found by Walker, in 1848–9, to be $23^m\ 41^s\ 54$, and has never been re-determined; but the link between New York and Jersey City was determined only geodetically, and that between Jersey City and Washington by simple clock-comparisons instead of the better method of star-signals; while another geodetic link was that of $12^s{\cdot}44$, between the Naval observatory and the Seaton station in Washington, to which all the telegraphic longitudes of the Coast Survey are referred. On account of these circumstances, and the omission, besides, in these earlier determinations, of many of the refinements and precautions since introduced, it seems highly desirable, as Dr. Gould suggests, that the longitude between New York and Washington, which he regards as the only weak link in our whole chain of telegraphic longitudes, should be carefully re-determined. Walker's value, however, as here given, is presumed to be very near the truth. Using the above quantities, Dr. Gould finds for the determinations of the longitude of the Naval observatory from Greenwich, which have appeared best entitled to confidence in recent years, the following:—

1. *From Eclipses and Occultations.*—The value adopted in the volume of Observations for 1845 is $5^h\ 8^m\ 14^s{\cdot}64$. Peirce, in 1845, from occultations observed by Bond from 1839 to 1841, gave $5^h\ 8^m\ 13^s{\cdot}9$. Walker, from all available observations between 1767 and 1842, obtained $5^h\ 8^m\ 14^s{\cdot}16$, a value subsequently reduced to $13^s{\cdot}85$, by change in the adopted longitude of Philadelphia, Cambridge, and Washington. A correction of the lunar parallax, deduced by Airy, Walker and others, from various observations, required a still further diminution of all

American longitudes; so that we have at present from eclipses and occultations:

Walker, corrected value from observations before 1843,	$5^{h}\ 8^{m}$, $11^{s}{\cdot}14$
Peirce, from eclipse of 1851, July 28,	11·57
Peirce, from emersions of Pleiades, 1839, Sept. 26,	11·45
Peirce, " " " 1856–1861,	13·13

2. *From Moon Culminations:—*

					h.	m.	s.
Walker,	from	Cambridge	obs'tions,	1843–45	5	8	10·01
Loomis,	"	Hudson	"	1838–44			9·3
Gilliss,	"	Capitol Hill	"	1838–42			10·04
Walker,	"	Washington	"	1845			9·60
Newcomb,	"	"	"	1846–60			11·6 ±0·4
Newcomb,	"	"	"	1862–3			9·8

Walker considered $9^{s}{\cdot}96$ as the most probable value from moon-culminations, and Newcomb assigned $11^{s}{\cdot}1$ as that indicated by the Washington observations from 1846 to 1863, inclusive.

3. *From chronometers transported between Boston and Liverpool.*

	h.	m.	s.
Mean from 373 previous to 1849,	5	8	12·46
Bond's discussion of 175, expedition of 1849,			11·14
Walker's " " " "			12·00
Bond's " " " "			12·20±1·20
Bond's " of 52, 6 trips, expedition of 1855,			13·43±0·19

The new telegraphic determination of the longitude between Liverpool and Greenwich adds $0^{s}{\cdot}06$ to all these values.

The discordance of the most elaborate and trustworthy results obtained by the old methods is thus seen to exceed four seconds. The value employed by the Coast Survey from 1852 to 1859 was $5^{h}\ 8^{m}\ 11^{s}{\cdot}2$; since 1859 it has been $5^{h}\ 8^{m}\ 11^{s}{\cdot}8$.

This great uncertainty of the trans-Atlantic longitude—from 30 to 60 times greater than that of our Coast Survey determinations—required that the earliest opportunity presented by the successful laying of an ocean cable should be seized for determining this longitude telegraphically, and with all possible precision. Measures were early taken to use, for this purpose, the cable of 1858, and, again, that of 1865; but, of course, in vain. In 1866, however, the cable of that year, and the recovered cable of the year before, afforded the desired opportunity. The expedition, organized under authority of the Coast Survey, was directed by Dr. Gould, and composed of officers of the survey, who were among the most skilled in longitude work. Messrs. Gould and Mosman went to Valencia, Ireland, Messrs. Dean and Goodfellow to Heart's Content, Newfoundland, and Messrs. Davidson and Chandler to Calais, Maine; for, the complete solution of the problem in hand, required the determination of three separate longitudes: 1, between Greenwich and Valencia; 2, between Valencia and Heart's Content, and 3, between Heart's Content and Calais, the easternmost station of the series connected by the telegraphic determinations of the Coast Survey. The first was accomplished by the ready co-operation of the Astronomer Royal. The second and third involved the principal labor, and presented the chief difficulties.

And these difficulties were by no means trifling. Wretched climate, defective land lines, unprecedented distance, with other untoward conditions, all conspired to render success by no means easy. Neither Valencia, nor Newfoundland, had, it would seem, any attractions for an astronomer. At Valencia, the observers were informed, on their arrival, early in October, that it had rained every day, without exception, for eight weeks. It was not until the 14th that any instrumental adjustments whatever, could be effected. During the seven weeks of their sojourn "there were but four days on which no rain fell, and there was but one really clear night during the period while the instruments were in position. The observations were, in general, made during the intervals of showers; and it was an event of frequent occurrence for the observers to be disturbed by a copious fall of rain while actually engaged in noting the transit of a star." The climate of Newfoundland appears to have been no better for astronomical work than that of Valencia. Messrs. Dean and Goodfellow reached Heart's Content on the 20th September, but saw neither sun, moon, nor stars until the 16th October, when they made their first imperfect adjustments of the transit and clock. In such circumstances, it is certainly most creditable to the skill and energy of the observers, that so many and excellent observations were obtained, as are discussed in this report.

The most formidable difficulty encountered, however, was the defective condition of the long land line of 1,100 miles between Newfoundland and Maine. On this line there were no less than four "repeaters," or double relay magnets, and two stations at which messages were re-written, without even an automatic "repeater," until these indispensable instruments were furnished for those stations by Mr. Davidson. But even with these provided, and with the most laborious precautions taken in other respects, all efforts at direct communication proved unavailing, day after day, and week after week. Mr. Davidson's health becoming impaired, his place at Calais was taken by Mr. Boutelle, one of the most experienced officers of the survey. Singularly enough, it was only a couple of hours before his arrival, on the 11th December, that, suddenly, the long-desired communication was found to be established. "A sharp frost had thrown the otherwise defective line into a condition of admirable insulation, so that an interchange of clock signals was effected without difficulty." The signals afterward obtained over this line were, in the main, quite satisfactory, and sufficiently numerous to ensure a trustworthy result; still this third link in the chain of longitudes is undoubtedly its weakest part.

A further difficulty was presented by the unprecedented interval between the meridians; necessitating the use of simple clock-comparisons instead of star-signals, and preventing the interchange of observers for eliminating the effects of personal equation. Star-signals—i. e., signals transmitted at the instant a given star passes the several wires of the transit instrument, first at one station and then at the other, and registered at both —have these advantages over mere clock-signals—beats sent from each station and similarly registered—that they give results independent of the star's right ascension, which, cannot be said of the other; that, unlike the other, they are independent also of absolute time-determinations, though not of instrumental errors; and, finally, that they yield results affected only by the clock's rate, and not its error, in the interval of the star's passage between the two meridians. Where this interval is large, as in the case before us, the special advantage of star-signals mainly disappears; and even if it did not, they would require too protracted an occupation of the cable, and in the climate encountered, or indeed, in any climate, the chances would be great, with an interval of three hours, against the same star being observed at both stations.

With the superior catalogue of time-stars, however, which had been prepared, and with the careful experiments for personal equation which were made both before and after the expedition, there was good reason to believe that, even with the clock-method, necessarily used, no sensible, or at least, considerable error depending on the right ascensions would be introduced, nor any, also, arising from uneliminated personal equation. That this was true, in both cases, the discussion of these points in the report seems conclusively to show.

Still another serious obstacle in the way of accurate longitude determination was the absence of any means for the automatic registration of the signals received; inasmuch as the loss of time in noting the signals was, in the method employed, not only considerable, but quite uncertain. The most sensitive electro-magnet tried, could not be actuated unless by a charge of the cable too strong to allow the transmission of signals with sufficient rapidity. When the Valencia clock was breaking circuit during an eighth part of every second, a permanent deflection only was observed at Newfoundland.

The instrument actually employed for receiving signals was Sir W. Thomson's very ingenious reflecting galvanometer, the construction and performance of which are thus described. "A small mirror, to the back of which is attached a permanent magnet, the joint weight of the two being from five to six centigrams, is held, by means of a single fiber above and below, in the center of a coil of fine wire, which forms part of the galvanic circuit; and its position and sensitiveness are regulated by movable bar-magnets placed in the immediate vicinity. Upon the mirror is thrown a beam of light through a slit in front of a bright kerosene lamp, and the deflections of the needle are noted by the movements of the reflected beam, which is received upon a strip of white paper. The exquisite delicacy of this galvanometer, as well as the electrical excellence of the telegraph cables, may readily be appreciated after the beautiful experiment in which the electricians at Valencia and Newfoundland conversed with each other on a circuit not far from 700 myriameters (4,320 statute miles) in length, formed of the two cables joined at the ends, using a battery composed of a percussion gun-cap, a morsel of zinc, and a drop of acidulated water."

It is obvious that, with this instrument, there must be an appreciable loss of time in noting signals, due to inertia of needle, etc., which would be avoided, or more definitely measurable, with an automatic register. But no such register was available. Fortunately, this loss, as is shown in the chapter on "Personal error in noting signals," proved to be more constant and measurable than was anticipated, and, affects the final result only in a very slight and limited degree. A very delicate automatic receiving apparatus has since been suggested by Dr. Gould for recording the signal on the chronograph, by contact of the galvanometer needle, whenever slightly deflected, with wires placed close on each side of it, and properly connected with the battery.

The other instruments used were the regular apparatus of the telegraphic party of the Coast Survey; at each station, a 46-inch transit instrument, with reversing apparatus, and a reticule of five "tallies," of five spider lines each; a chronograph, (Bond's "spring governor," at Valencia and Heart's Content, and a "Kerrison's regulator" at Calais;) and a circuit-breaking clock. Upon all the chronographs one pen, which is constantly tracing a line upon a revolving cylinder, records the signals, both of the clock and of the observer, by offsets from this normal line.

At Valencia, observations were obtained on fifteen nights, on no one of which was the sky unclouded. "On only two of the five nights on which longitude signals were exchanged with Newfoundland, was it possible to obtain observations after the exchange, and this was possible, too, on only one of the three nights, when signals were successfully exchanged with Greenwich. Fifty-three complete transits were observed over all the threads; on and after November 5th, by Mr. Mosman alone; on the 25th and 28th October, by Dr. Gould; at other times by both. The clock corrections, however, whenever possible, were deduced from Mr. Mosman's observations; when otherwise, a constant correction of —0^{s}·08 was applied to Dr. Gould's. For both clock correction and azimuth, the conditional equations were solved by the method of least squares, a method introduced into this branch of the coast survey work by Dr. Gould, and an important means of giving precision to the telegraphic longitudes. We are indebted, indeed, to this eminent astronomer for the particular notation and methods of observation and reduction, which have been employed in the longitude work of the coast survey since it came under his charge, and which are described in the C. S. Report for 1856. The transit observations are given in full, with their reductions for the groups immediately preceding and following the longitude-signals, and with the normal equations and resultant values for each group.

A glance at these observations reveals at once, in the small residuals, both the accuracy of the star-places and the skill of the observers. For, necessarily, different sets of fundamental stars for clock and instrumental corrections were employed at the two stations, owing to their distance, and also at the same station on different nights, owing to the various hours at which the signals could be exchanged. Considering all the unfavorable circumstances, the degree of precision attainded is certainly remarkable. This must be attributed, in great part at least, to the extreme accuracy of Dr. Gould's own carefully elaborated catalogue of fundamental stars, prepared in 1862, for the use of the longitude parties of the Coast Survey, and in its second edition, furnishing still more precise places, especially for circumpolar stars.

The observations at Heart's Content, and at Calais, for time and instrumental corrections, were made, as at Valencia, in very unfavorable circumstances, yet, similarly, exhibit remarkable accordance. Fortunately, at the former place excellent series of transits were obtained, both before and after every exchange of longitude signals, whether with Valencia or with Calais. At the latter place, however, the clock gave serious trouble as well as the weather; yet the observations, though not as good as could be wished, proved to be much more accordant than might have been expected in the circumstances, and give, it appears, a small probable error of the final result.

Longitude signals were successfully transmitted between

Foilhommerum and Heart's Content on but five dates; October 25th and 28th, and November 5th, 6th, and 9th. The method of giving and receiving these signals is thus described in the Report. We quote at length, that a clear understanding may be obtained of that portion of the operations on which the precision of the result chiefly depends.

"Three series, of two sets each, were exchanged on every occasion; each set consisting of ten signals alternately positive and negative, at intervals of about five seconds, except that the fifth and eighth were preceded by pauses of ten seconds, which was also the interval between the two sets. The purpose of this arrangement was to discover whether the velocity of transmission was perceptibly affected by a longer time being allowed for the cable to recover its electrical equilibrium, and also to facilitate the identification of the individual signals. Some slight convenience in the practical details also arose from the circumstance that each set occupied one minute, and that each series consisted of ten positive and ten negative signals. Those signals were considered positive by which the platinum was put in connection with thc cable and the zincode with the ground.

In receiving the signals, the observer (Mr. Dean at Newfoundland, and myself at Valencia) watched the deflections of the light-spot, while his thumb rested on the button of a delicately adjusted break-circuit key, which was pressed at the instant in which the deflection was perceived. This instant was thus recorded upon the chronograph, after a certain amount of delay, which we will call the personal error of noting, and which depended upon a considerable number of influences to be discussed hereafter. The keys by which the signals were transmitted were made by the American Telegraph Company, under the supervision of Mr. Dean, and are constructed according to the arrangement devised by Prof. Thomson for the Atlantic Telegraph, in such a manner that pressure upon one button produces a positive, and upon the other a negative signal, while no current flows at other times. To this arrangement an additional contrivance was applied by which the local circuit to the chronograph passed through the same key, and was interrupted by pressure upon either button, so that every signal transmitted through the cable was recorded upon the chronograph at the station whence it was sent.

It is thus manifest that the times of sending the signals were accurately recorded, while the times of receiving signals were recorded after an interval of time dependent on the personal error of noting, and inseparable from the time of transmission through the cable, except by some independent means of measurement. If this interval were the same for both observers, it would be eliminated entirely from the longitude and merged with the time of transmission. Otherwise it would effect the resultant longitude by one-half the difference between the personal errors of noting for the two observers. Happily it proved to be very nearly the same for Mr. Dean and myself, and also measurable; so that it has been possible to eliminate its influence from the measure of velocity, as well as from the longitude."—*Rep.*, p. 54.

The relations of the several quantities derived from the observations, and entering into the final results, are expressed in the following equations: in which T and T' denote the clock times at Valencia and Newfoundland, respectively, Δt and $\Delta t'$ the clock errors, x the transmission time of the signals, and λ the longitude; these quantities being also distinguished by a subscript $_1$ and $_2$ for Valencia and Newfoundland signals respectively. Including in x the personal error of noting signals, the signals given and recorded at Valencia at the time T_1 will be registered upon the Newfoundland record at $T_1'=T_1+\Delta t_1-\Delta t_1'-\lambda+x_1$ and the signals given and recorded at Newfoundland at T_2 will be registered upon the Valencia record at $T_2=T_2'+\Delta t_2'-\Delta t_2+\lambda+x_2$. Thus the comparison of the records of Valencia signals, at the two stations gives $T_1-T_1'=\Delta t_1'-\Delta t_1+\lambda-x_1$, while the comparison of the records of Newfoundland signals gives

$$T_2-T_2'=\Delta t_2'-\Delta t_2+\lambda+x_2,$$

and consequently

$$2\lambda=(T_2-T_2')+(T_1-T{-}_1')+(\Delta t_2-\Delta t_2')+(\Delta t_1-\Delta t_1')+(x_1-x_2)$$

$$x_1+x_2=(T_2-T_2')-(T_1-T_1')+(\Delta t_2-\Delta t_2')-(\Delta t_1-\Delta t_1')$$

If we assume the personal error of noting to be the same for the two observers, and the signals to travel with equal velocity in the two directions, the term x_1-x_2 will disappear from the first equation, while the second will give a measure of the sum of the transmission-times and the personal errors of noting.

The several quantities above indicated are given in detail in the Report for the different series of signals, and exhibit excellent accordance in the results. There appears to have been no difference of clock rates to affect the deduced value of x, nor of velocity for eastern and western signals to require a correction of λ depending on the clocks.

The resultant values for the longitude, subject, however, to a correction for personal equation in determining time, are as follows:—

1866, October	25,	$2^h\ 51^m\ 56^s{\cdot}477$
	28,	56·487
November	5,	56·455
	6,	56·481
	9,	56·460

The mean interval between the moments of giving the signals and of their record upon the chronograph sheet is similarly found to have been

October	25,	0^{s}·62 ±	0^{s}·008
	28,	0·64	·010
November	5,	0·59	·004
	6,	0·55	·007
	9,	0·54	·005

in which the quantities appended are the probable errors of the respective determinations as deduced from the total results of the several sets, there being six sets for each determination except that of November 6th.

Between Heart's Content and Calais the clock signals were much less satisfactory; being obtained only on four nights, on only two of which the clock errors could be determined either immediately before, or soon after, the exchange, and on one of these two there was special trouble from the clock. By some misunderstanding, also, the signals were not exchanged strictly according to programme, and thus a further source of uncertainty was introduced from the partial confusion occasioned in the record, and the greater difficulty of eliminating personal equation. The resulting longitude and time of transmission exhibit, however, better accordance than was to have been expected, and are as follows (subject to a correction for personal equation):

1866.		λ.	x.
December 11,	0^{h} 56^{m}	37^{s}·89	0^{s}·24
12,		37·53	0·31
14,		37·84	0·27
16,		37·78	0·28

From the description of the mode of receiving signals, quoted on page 236, it will be seen that there is introduced, in all the observations, a special error due to the different methods employed at the two stations of recording a given signal. This is called in the report, "the personal error of noting." It is the interval between the arrival of the signal at the galvanometer and its record on the chronograph; for it will be remembered that the signal sent through the cable does not pass at once, automatically, to the chronograph, as at the transmitting station, but reaches it only through the mediation of an observer, who, after noticing the deflection of the light-spot from the galvanometer, sends a second telegraphic signal to his own chronograph. The whole interval, x, therefore, which elapses between the giving of a signal at one station and its chronographic record at the other, is made up of several parts.

These parts, as given in the Report, consist of the time requisite,—

1. For the signal to arrive at the other station.
2. For the galvanometer needle to move through a perceptible arc.
3. For the observer to notice the motion and tap his break-circuit key.
4. For this observation-signal to be recorded upon the chronograph.

Of these the second and third constitute the "error of noting," which is, therefore, partly instrumental and partly personal; the one due to the inertia of the galvanometer-magnet, the other being the "transmission-time" along the nerves from eye to brain, through brain, and thence to finger-tips; consisting, therefore, of three distinct intervals, viz.: 1, between phenomenon and perception; 2, between perception and volition; 3, between volition and giving signal. The sum of these three intervals, according to the experiments made by Dr. Gould, could hardly exceed, for good observers, 0·3; which would give for velocity of transmission through nerves about 13 feet, unless, indeed, some considerable time is occupied in the act of volition after perception, as seems probable from the experiments of de Jaager and Hirsch. Helmholtz made the velocity of nerve-sensation in the frog (dead), by different experiments, 87 and 89 feet per second. Dr. Schelske found that velocity in the nerves of common sensation and along the spinal cord in living men, to be about 97 feet. Others make the velocity not more than half as great.

With respect to the four intervals which make up the quantity x, it is remarked that, if equal at the two stations, they become wholly eliminated in the resultant longitude; if unequal, the longitude must be increased by one half the excess of their sums for westward signals. In either case, the operations for longitude give only their total sum at the two stations. The chronographs at both stations being similar, it might be presumed that the time lost by both in making the record after the key is tapped would be the same; and repeated examinations showed that this was sensibly the case. It therefore becomes eliminated in the resultant longitude; as does also the transmission-time through the cable, if sensibly the same in both directions; as Dr. Gould shows that it is.

The method employed for determining the "error of noting" was alike ingenious and successful. It was by carefully observing a series of signals similar to those exchanged for longitude, and so arranged that both the original signal and the observation of the consequent deflection were recorded on the same chronograph. The similarity requisite in respect to intensity of current, was secured by an ingenious adjustment of battery force to both galvanometer and chronograph, by means of a divided circuit, the branch of it through the galvanometer being a German silver wire so fine as to give an intensity, as compared with that in the other branch, of 1 to 100; the requisite reduction. Other equally skillful devices secured similarity and freedom from error in other respects.

These experiments showed the range of variation of this error of noting for different individuals to be very small; the mean error for Dr. Gould at Valencia being $0^s{\cdot}271$, and for Mr. Dean at Newfoundland, $0^s{\cdot}335$; the difference being due to the galvanometer, probably, rather than the observer. The sum of the two $0^s{\cdot}606$, is the quantity to be deducted from the value of $x_1 + x_2$ to obtain the true time of transmission, and half their difference, or $0^s{\cdot}032$, that to be deducted from the longitude after all other corrections are applied.

In conducting these experiments a very curious source of error was detected, in the effect of hearing the click of the key, upon observations supposed to depend only on seeing the deflection of the needle. This is a point of so much importance to observers, that we quote the suggestion of the Report, "that a very marked effect upon the observation of transits of stars is likely to be produced when the chronograph is in the same apartment, so that the regular beats of the magnet are audible. When the intervals between the transit-threads are approximately multiples of half a second, the tendency is very great so to tap upon the observing key as to produce a rhythmical beat in the armature; and when the interval differs from the multiple of a second, the occurrence of that magnet-beat which records an even second often precipitates the tap of the observer, whose nerves are in keen tension awaiting the instant of bisection. Only a strong effort of will can obviate these perturbing influences—which are akin to those exhibited in the measurements just described."

This curious effect seems to show that eye and ear do not report to the brain with the same promptness, and that, when addressed simultaneously, the mind cannot recognize which makes the report, but takes that of one or the other, or both together, indiscriminately, the range of uncertainty being equal to the difference in promptness of the two senses. Possibly part of the effect may be due to a definite lapse of time between the occurrence of the audible and the visible signal; which, however, should give a more constant effect.

Want of space compels us to pass the interesting chapter on "Personal equation in determining time," with simply the remark that the ordinary methods of measuring that equation being inapplicable, by reason of the distance and other circumstances, comparisons were made between the observers for this purpose as soon as practicable after their return; and the values deduced, though in many respects quite unsatisfactory, were adopted as the best approximation that could be obtained for the different observers. They are these:

Gould—Mosman =+0^{s}·02
Dean—Mosman =+ 0·11
Goodfellow—Dean =+ 0·14
Boutelle—Goodfellow =− 0·14
Boutelle—Chandler =− 0·04

A special arrangement, by which it was intended to entirely eliminate personal equation at Heart's Content, unfortunately failed through some misapprehension of the observers. The fact, however, that they had been in the practice of observing together for many years and had always found the equation between them to vary between very narrow limits, on the two sides of zero, rendered the failure of little practical consequence.

We hasten to give briefly the final results for longitude.

Between Foilhommerum and Heart's Content we have for the several dates, after corrections applied:—

1866, Oct.	25,	2^{h} 51^{m} 56^{s}·457
	28,	·468
Nov.	5,	·455
	6,	·481
	9,	·460

The final longitude deduced, after correction for personal equation in determining time, Dean—Mosman = +0^{s}·11, and in noting signals, Dean—Gould = +0^{s}·03, becomes

$$\lambda = 2^{h}\ 51^{m}\ 56^{s}\cdot54$$

Between Heart's Content and Calais, the results, similarly corrected, are for the several dates:—

Dec. 11,	0^{h} 55^{m} 37^{s}·93
12,	[37·53]
14,	37·84
16,	37·82

And the final result, corrected by −0^{s}·14 for personal equation between Boutelle and Goodfellow, and omitting Dec. 12, is—

$$\lambda = 0^{h}\ 55^{m}\ 37^{s}\cdot72$$

Between Greenwich and Foilhommerum, the longitude was obtained by satisfactory signals on two nights, and compared with two previous determinations by Mr. Airy for other points at Valencia, by short geodetic connections. The two nights exchanges gave

			λ.	x.
1866, Nov. 5,	0^h	41^m	33^s·305	0^s·115
13,			33·280	0·110
Mean,	0	41	33·29	

This differs by —0^s·10 from that adopted by Mr. Airy as deduced from the great chronometric expedition of 1844, and the telegraphic determination of 1862.

The combination of the three longitudes thus determined, gives—

Greenwich—Foilhommerum,	0^h	41^m	33^s·29
Foilhommerum—Heart's Content,	2	51	56·54
Heart's Content—Calais,	0	55	37·72
Greenwich—Calais,	4	29	7·55

"The Valencia observations having been made by, or referred to, Mr. Mosman throughout the whole period, his personal equation is eliminated; the equation between Messrs. Goodfellow and Dean, always small, may be regarded as trustworthy, and by a happy coincidence the personal equations of Mr. Boutelle on the west, and of Mr. Mosman on the east, seem to be almost identical, so that even a total disregard of this quantity would have resulted very nearly in its perfect elimination, the oceanic arc being diminished and the land arc increased, each by about 0^s·14."

The only probable influence of personal equation in the entire longitude-measurement, comprising, as it does, three-sixteenths of the whole circumference, lies in the difference between the observations of Messrs. Dunkin and Boutelle.

The longitude of Calais, as heretofore telegraphically determined, is as follows:—

Calais—Bangor,	0^h	6^m	0^s·31
Bangor—Cambridge,	0	9	22·99
Cambridge—New York,	0	11	26·07
New York—Washington,	0	12	15·47
Calais—Washington,	0	39	4·84

whence we have

Greenwich—Washington, 5^h 8^m 12^s·39

The Seaton Station being 12^s·44, and the dome of the Capitol 10^s·17, east of the Naval Observatory, to the center of the dome of which the preceding value refers, we have as their longitudes from Greenwich—

Seaton Station,	5^h	7^m	59^s·95
Capitol,	5	8	2·22

From the facts, as exhibited in this report, there would seem to be every reason to believe that this result is within a very small fraction of a second of the exact longitude, and that the small probable error, whatever it may be, is mostly due to the weakest link in the whole chain—that between Newfoundland and Calais.

The indiscriminate mean of all the chronometer determinations, as given early in this article, using the numbers of chronometers as weights, is $5^{h}\ 8^{m}\ 12^{s}{\cdot}14$; leaving out Bond's first discussion of the 175, it is $5^{h}\ 8^{m}\ 12^{s}{\cdot}37$. The mean of Peirce's two from occultations of Pleiades, without regard to weight, is $5^{h}\ 8^{m}\ 12^{s}{\cdot}29$.

We regret that we have not room for even a brief abstract of the valuable closing chapter on the transmission-time of signals, but are compelled to dismiss it with barely stating the general result and some conclusions with respect to a few points, as derived from the experiments.

The transmission-time by cable, after correction for personal error in noting signals ($0^{s}{\cdot}303$), was found to be for the several dates:—

1866, October	25,	$0^{s}{\cdot}314$	Cable of 1865, with earth and condenser.
	28,	·343	" " " " " "
November	5,	·280	Both cables, no earth.
	6,	·248	" " " "
	9,	0·240	" " " "

The battery-strength on these nights was as follows:—

October	25,	10	cells at Valencia,	10	cells at Newfoundland.
	28,	10	" " "	10	" " "
November	5,	3	" " "	3	" " "
	6,	3	" " "	10	" " "
	9,	4	" " "	10	" " "

From these results the inferences seem warrantable, 1st, that the velocity of transmission is greater when the circuit is direct and consists of a good metallic conductor exclusively, than when the signals are given by induction, although the earth may be at the other electrode; and 2d, that an increase of intensity in the electromotive force is attended by an increase in the velocity of propagation of the signal.

From the observations for longitude, and from other experiments made with special reference to particular points, the following general conclusions were reached.

"It appears manifest that not an electrical charge or discharge, but simply an electrical disturbance, is requisite for transmitting a signal; that an inductive impulse, sufficient to deflect the galvanometers employed, was transmitted through one cable, having at each end a condenser with 10 cells, in somewhat less than the third of a second, five seconds after the transmission of an impulse of the opposite sort; that with a circuit formed by the two cables, a smaller electromotive force sufficed to transmit the signals with yet greater rapidity; that the signals traveled more rapidly through a cable which had not recovered its electrical equilibrium after a current of the opposite character; and that the speed of the signals is modified by the earth-connections, more readily than by changes in the battery-power. And the very marked differences, found in the rates of transmission, between signals given by completing an interrupted circuit and those given by interrupting a closed circuit, may perhaps lead to investigations which will afford an explanation."

C. S. LYMAN.

ART. XXVIII.—*Meteors of November*, 1869; compiled by H. A. NEWTON.

THE cloudy weather in most parts of the United States and Europe prevented continuous observations of the November meteors, at the time of their return in 1869, by nearly all those who were watching for them. The observers, in the few stations where the skies were clear, furnish, however, ample testimony to the appearance of unusual numbers on the morning of Nov. 14th. The display, moreover, like that of 1868, continued for several hours. I give below the particulars of the observations at various places.

1. *At New Haven.*—Two members of the Junior Class, Messrs. C. B. Dudley and R. P. Maynard, saw, through openings in the clouds, six meteors, between 4^h and 5^h on the morning of the 14th. Some of them had the peculiar trains belonging to the November meteors. On the next morning, Nov. 15th, the sky was nearly overcast, but between 3^h and 3^h 45^m, we saw eight meteors, four of them conformable. During the rest of the morning the sky was overcast, and even in this interval it was at no time more than one third clear.

2. A similar failure, nearly or quite complete, is reported by Prof. Eastman, at the U. S. Naval Observatory, Washington, by Mr. Marsh and Mr. Taylor, at Philadelphia, by Mr. Fuertes at Stamford, Conn., by Mr. Boerner, at Vevay, Ind., by Prof. Rockwood at Brunswick, Me., and by various others who were in readiness to make observations.

3. *At Pensacola, Florida.*—To the courtesy of Commodore Sands, Sup't. of the U. S. Naval Observatory, we are indebted for a letter of Commander Wm. Gibson, from Pensacola. He says that the night of the 13th–14th was exceedingly bright and

clear, and that the shooting stars were observed in extraordinary numbers, from 1^h 15^m, A. M., until dawn, most numerously between 3^h and 4^h, A. M. It was difficult to give the average per minute. It varied from two or three to twenty or thirty, or more, the star-showers flashing and intermitting like the bursts and pauses of a gusty rain. In magnitude they varied from mere moving points of light, to those which were larger than Jupiter. One train remained visible at least 50 minutes, drifting slowly to the northward.

4. *Upon the Pacific Ocean.*—To the courtesy of the Secretary of the Smithsonian Institution, we are indebted for a letter of Mr. Alexander Evans of Elkton, Md.

On the morning of Nov. 14th, he was upon the Pacific Ocean, lat. 8° 30′ N., and long. 84° 30′ W. He watched from two till four o'clock, when the sky became overcast. Between these hours the sky was partly covered. The display was, he says, quite equal to that of 1868 which he observed throughout. He thought that the radiant point was not as last year, in the center of the sickle in Leo, but a little more to the eastward between the stars η and γ. There were several nonconformable meteors whose radiant seemed to be the zenith.

5. *At Santa Barbara, California.*—To the courtesy of Prof. Peirce, Superintendent of the U. S. Coast Survey, we are indebted for the observations of Mr. Geo. Davidson, Assistant, and Mrs. E. Davidson at the Coast Survey station, at Santa Barbara (lat. 34° 24′, lon. 7^h 59^m). Between 1^h 18^m, A. M., and 3^h 43^m on the morning of Nov. 14th, they counted 556 meteors. The following table represents the number of meteors seen by the two observers *per minute*. They are taken from the diagram forwarded by Prof. Peirce.

1^h	23^m	2·2	meteors.	2^h	16^m	5·0	meteors.	2^h	46^m	3·0	meteors.	3^h	15^m	5·0	meteors.
	35	3·6	"		21	5·2	"		51	4·0	"		21	2·5	"
	47	4·8	"		25	5·0	"		55	6·0	"		25	2·0	"
	53	5·0	"		31	2·2	"	3	1	6·0	"		31	3·7	"
	59	3·8	"		35	2·2	"		5	3·0	"		35	3·2	"
2	9	4·8	"		41	6·6	"		11	2·0	"		41	4·0	"

Mr. Davidson says, "the night was beautifully clear, the moon being ten days old, and 4° south. I was called at 1^h 10^m, A. M., up to which time 22 meteors had been seen. After 3^h 43^m, A. M., watch was kept for any unusual display, but the numbers gradually diminished. Some of the meteors were very brilliant and left persistent trains. About half a dozen meteors were observed moving in directions toward the radiant.

"At 2^h 33^m, A. M., I observed a brilliant meteor start from a point above, and a little to the left of the pointers; it left a persistent train and disappeared at a point about 9° or 10° above Polaris, and 6° to the right. The train was 5° in length;

gradually it took a wavy form; then curved until it formed two-thirds of an irregular circle, and was 3° in diameter and half a degree in width. I examined it with a good binocular, and found it not of uniform density, but having open spaces in it. It remained visible 8½ minutes, and in that time apparently moved in a line toward the radiant point in Leo, over a space of 8°."

The small number of meteors reported above as seen between 2^h 30^m and 2^h 40^m was due to Mr. Davidson's being engaged in watching the train of this meteor.

At 1^h 15^m Mr. S. R. Thockmorton, Jr., saw a meteor appear and disappear without apparent motion. It was about two degrees above and to the left of the bright star in the blade of the sickle.

6. *At Fredericton, N. B.*, (lat. 46° 3′, lon. 66′ 45′).—A watch was kept up by relays of students of the University of New Brunswick, throughout the night of Nov. 13th, 14th. They report the times and general directions of the individual meteors, with duration of flight, brilliancy, &c. The following table of the numbers seen in each 10 minutes of the night is compiled from their report.

Number of meteors seen at Fredericton, N. B. on the night of Nov. 13th, 14th.

Hour.	0^m–10^m	10^m–20^m	20^m–30^m	30^m 40^m	40^m 50^m	50^m 60^m	Total.
7— 8				1	1	1	3
8— 9	1	1	3	1	0	1	7
9—10	0	0	1	1	1	2	5
10—11	4	0	4	3	2	5	18
11—12	8	12	4	6	6	3	39
12— 1	10	6	4	5	8	26	59
1— 2	6	18	6	8	12	17	67
2— 3	11	10	26	32	37	20	136
3— 4	24	44	23	55	21	14	181
4— 5	22	30	28	35	34	18	167
5— 6	25	31	20	16	22	20	134
6— 7	7	30	4				14
				Total in 11 hours,			830

The observations were made by three parties, each watching for two successive hours. Messrs. Byers, Crozier, Stone, Connell and Williston watched from $7\frac{1}{2}^h$ to 9^h and from 1^h to 3^h.

A second party, consisting of Messrs. Vanwarts, Cliff, Wortman, Belyea, and Lawrence, watched from 9^h to 11^h and from 3^h to 5^h, and a third party consisting of Messrs. Willbur, Scovil, Chandler and Walker watched from 11^h to 1^h and from 5^h to $6\frac{1}{2}^h$. Only four persons were, however, observing at any one time.

7. *In England.*—The clouds prevented observations for most of the time in Great Britain. At Glasgow, Mr. A. S. Herschel

and Mr. Robert McClure had a tolerably clear sky from half past 4 till half past 5.

"Meteors of the brightest class of the November shower, having luminous streaks, were crossing the sky at the rate of about 40 in an hour, for one observer, corresponding to a rate of frequency of at least one hundred per hour, in all the sky. The apparent paths of thirty of these meteors were recorded upon a map, by their course among the stars, and the direction of their flight was from the usual radiant point in the constellation Leo. * * * *

"A clear view of the sky was again obtained towards eleven o'clock on Sunday night, when a single shooting star, passing from Gemini to Taurus, and leaving a faint streak, appeared to be the last visible representative of the November meteors. Although the sky continued clear, and an attentive watch was kept for nearly three hours afterwards, no other meteor could be seen. The purely negative character of this result adds a fresh proof to the evidence obtained in former years of the definite boundaries and narrow limits between which the stream of the November meteors is confined."

Observations at Greenwich and at most other places in England were not successful on the morning of the 14th.

At Culloden, Mr. A. Forbes counted upwards of 200 between the hours of 3^h and 7^h, A. M., the maximum of the shower appearing to be about 5 o'clock.

8. *In France*, the "Association Scientifique" organized a system of observations, at various stations near the Mediterranean, embracing even one or two in Italy. The results were to be reported and discussed at sessions to be held in the latter part of the month of November, at Marseilles and Bordeaux. We have not learned what success rewarded the zeal of the French observers.

9. *At Paris.*—Mr. Chapelas reports to the French Academy of Sciences, that notwithstanding unfavorable weather and moonlight, "observations conscientiously made, give us for the mean hourly number reduced to midnight and to clear sky, and corrected for the influence of the moon, for the night of Nov. 12th, 6·8 meteors; for the night of Nov. 13th, 24·8 meteors."

He says that the observations for the preceding nights gave steadily, a smaller hourly number than the mean for that period of the year, which is, according to him, 13·6.

Mr. Chapelas, as usual, does not say how many meteors he saw, nor how long he observed, nor how large a correction he applies for cloudiness, nor what for moonlight, nor what to reduce to midnight. We are not aware, even that he has published rules or constants for such reduction. His *observations* might perhaps have some value, if we knew what they were.

They could then be criticized, and compared with those of other observers. If found to have been carefully made they would be valuable to science. But as now given to the world, the observations of Mr. Coulvier-Gravier and Mr. Chapelas are inextricably mixed up with their deductions. The manifest errors of the latter taint the whole, and make it necessary to throw them all away. The French Academy of Sciences publishes his reports, and we respectfully call its attention to the matter.

10. *At Vienna.*—Prof. Weiss of the Observatory of Vienna, with three aids, determined 36 paths with the meteoroscope between 2^h and 6^h, A. M., of the 13th (Prof. Schmidt in *Heis Wochenschrift*, Dec. 8th). Some of them were from the Leo radiant. The total number visible was verv moderate.

The next night was stormy, and for an interval of 10 minutes only at 4^h, A. M., could anything be seen. A few stars of Leo, Gemini and Auriga were then visible through openings in the clouds, and along with them, though principally seen through haze, were some meteor tracks, having trains and radiating from Leo. Prof. Schmidt estimated that the hourly number for one observer was about 50, reckoning only the brighter meteors, and considered the display not greatly different from that seen by him at Athens in 1863.

At Munster and elsewhere in northern Germany the sky appears to have been overcast.

11. *At Rome.*—The cloudy skies which covered northern Europe on the night of the 13th, 14th, also impaired the Italian observations. Padre Secchi reports a few, though incomplete, from Rome.

At 2^h 30^m A. M. the clouds broke away in the west and in five minutes 18 meteors were seen. At 2^h 35^m the sky was nearly clear except low in the northeast, and a regular count was begun.

From 2^h 35^m to 2^h 40^m, 29 meteors were seen; from 2^h 40^m 2^h 48^m, 41; from 2^h 48^m to 3^h 0^m, 73; from 3^h 0^m to 3^h 15^m 40. At 3^h 18^m it became again overcast. The whole number in 40 minutes was 183, of which 5 were unconformable. He omits to state the number of observers. Padre Secchi concludes:

1st. That there was a real recurrence of the display.

2d. That it was but little different from the display last year, there having been then seen in the same interval about 200 meterors. The hour moreover was not that of the maximum.

3d. That the radiant point was within the bend of the sickle in Leo, and made with the stars ε and μ an equilateral triangle. But this determination was not very precise owing to the want of tracks near the radiant.

4th. That the greater part of the meteors passed to the north of the radiant.

It was further remarked that the trains were visible only a few seconds, and that there were none of the beautiful scrolls of smoke which were seen last year.

Madame Scarpellini gives in her *Corrispondenza Scientifica* accounts of observations at Rome by herself, (with negative results) and the following from Perugia and Civita Vecchia.

12. *At Civita Vecchia.*—Prof. Pinelli reports to Madame Scarpellini the following observations from Civita Vecchia.

Nov. 12th–13th. Between $10^h\ 30^m$, P. M., and 1^h, A. M., 4 meteors seen of 1st magn., 6 of 2d magn., and 8 of the third. Only a few clouds.

Nov. 13th–14th. Between $11^h\ 30^m$ and $2^h\ 35^m$, 54 meteors seen. Very cloudy and finally overcast.

Nov. 14th–15th. Up to $1^h\ 20^m$, A. M., only 16 meteors seen.

13. *At Perugia.*—On the morning of the 13th from 12^h to 6^h, A. M., Prof. Bellucci counted the following numbers in the several hours; viz., 26, 22, 26, 28, 24, and 17. In all 138 in six hours.

On the next night in successive hours from 6^h, P. M., till 4^h, A. M., he saw 3, 1, 0, 2, 1, 2, 0, 39, 190, 246. During the next half hour 71 were seen, making 555 in all. From $4^h\ 30^m$ it was overcast.

14. *At Velletri.*—Prof. D. Ignazio Galli, of the Municipal Observatory at Velletri, watched during the two nights of the 12th and 14th of November. (*Bull. Met. Rom.*)

On the first night, two observers, with a clear sky, in five hours, from $12^h\ 45^m$ till $5^h\ 45^m$, saw 71 meteors. The distribution through the five hours was as follows:

From $12^h\ 45^m$	to $1^h\ 45^m$,	2 conf.,	9 unconf.
" $1^h\ 45^m$,	" $2^h\ 45^m$,	5 "	7 "
" $2^h\ 45^m$,	" $3^h\ 45^m$,	8 "	6 "
" $3^h\ 45^m$,	" $4^h\ 45^m$,	7 "	7 "
" $4^h\ 45^m$,	" $5^h\ 45^m$,	11 "	9 "

On the second night they were able to see only through openings in the clouds. There were three or four observers, enough to fully command the whole of the visible portions of the sky. The results were as follows:

Time.				Meteors seen. Conf.	Unconf.	Portion of sky visible.	Total computed for clear sky.
1^h	0^m,	1^h	15^m	50	1	0·2	255
1	15	1	30	48	1	0·2	245
2	0	2	15	41	6	0.4	117
2	15	2	30	31	7	0·3	126
2	30	2	45	43	4	0·4	117
2	45	3	0	62	8	0·5	140
3	0	3	15	17	4	0·2	105
Total in $1^h\ 45^m$,				292	31		

The meteors of this second morning were much finer than those of the preceding morning. The sporadic meteors of each night appeared to radiate from Auriga.

15. *From Moncalieri.*—Director F. Denza reports in *Les Mondes*, for four observers:

Night of the 12th of Nov.	from	14^h	to	17^h	145 meteors.
" 13th	"	9		17	720 "
" 14th	"	$14^h\ 10^m$		$17^h\ 10^m$	99 "

These numbers need to be increased, as their principal object was to determine the position of the tracks and the instant of appearance of the meteors.

On the night of the 13th, 14th, there were seen:

At Alexandria	by 4	observers from	$9^h\ 45^m$	to	$16^h\ 45^m$,	168 meteors,
At Aosta,	1	"	$14^h\ 30^m$		18^h	189 "
At Varallo,	1	"	$13^h\ 25^m$		$16^h\ 25^m$,	121 "

Prof. Denza adds that "the meteoric shower of Nov. 1869, differs little from that of August, and in some places was even inferior to it."

16. *At Port Said, Egypt.*—In the *Monthly Notices* for Dec. are some observations of G. L. Tupman, Esq., at that place. On the mornings previous to that of the 13th, he detected some tendency to radiation from Leo. On that morning, of thirteen meteors, four were conformable.

On the morning of the 14th from $12^h\ 30^m$ to $13^h\ 15^m$ only two meteors were seen, neither conformable. There was a pretty large patch of clear sky overhead.

"The watch was resumed at $14^h\ 30^m$, the sky being then partly clear in patches, and continued until a quarter past 5, long before which the shower had entirely ceased. At $2^h\ 30^m$ it was at its height, most of the meteors being remarkably brilliant, and many of them tinted green. The greater part left bright streaks, which often remained visible a considerable time. The duration of the meteors or their 'time of flight' was considered to be less than half a second—too short a time to estimate even roughly.

"The following are the observations. Being unassisted, I stopped at every sixteenth to make the necessary entries.

"If the numbers in the table be reduced to an uniform interval of time and then multiplied by the cosecant of the altitude of the radiant, it will be seen that between $14^h\ 30^m$ and $16^h\ 24^m$ the numbers were nearly uniform, and slightly decreasing. The maximum, then, was either before or about $14^h\ 30^m$; but the *center* of the dense part must have been passed about 15 hours, as there was no sign of the shower at $13^h\ 15^m$.

Meteors observed at Port Said, Nov. 13th, Alexandria mean time.

From		To		No. of Meteors.	Elevation of the Radiant.
h	m	h	m		
10	40·0	13	15·0	0	15
14	30·0	14	40·0	16	35
14	52·0	15	2·5	16	40
15	8·0	15	19·7	16	43
15	24·0	15	33·6	16	46
15	38·5	15	52·5	16	50
15	59·0	16	7·4	16	54
16	12·0	16	24·0	16	57
16	26·0	16	38·0	6[*]	60
16	40·0	16	52·0	7	63
16	54·0	17	14·0	4	67

· Seven other meteors were observed, but they did not radiate from Leo.

From 11 orbits he determined $\alpha=151°·0$, $\delta=21°·5$, measured from the equinox of 1869. No single point satisfied all the paths that were observed.

The conclusions of Mr. Tupman respecting the closing of the shower are of course set aside by the observations in Italy and England by those of Fredericton and Santa Barbara.

17. The duration of the whole shower was at least twelve hours,and it appears to have been somewhat fitful in its intensity. Perhaps the apparent fitfulness may be due to the clouds, though we think that that is not the only reason.

Thus after the time named by Mr. Tupman we have an increasing display shown by Prof. Bellucci's numbers. At Culloden the report seems to carry this display on two hours longer. The numbers seen at Fredericton and Santa Barbara show a tolerably uniform continuance for several hours longer. There was little trace of the display on the next morning.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On Ammonia-chromium bases.*—Cleve has communicated to the Royal Swedish Academy of Sciences a memoir on the ammonia-chromium bases, which, though of not very recent date, has not, we believe, been noticed either in German or in English journals. The author sets out with the chlorid discovered by Fremy and investigated to some extent by himself in a previous memoir. To this he gives the formula, $Cr_2Cl_3, 4NH_3, 2HO$, and the name tetramin chrom-chlorid. It will be sufficient for our purpose to give

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[*] During this observation it was more cloudy than before, but during the two following ones it was much clearer.

the formulas of the various compounds with a general account of their properties. The formulas of the salts described by Cléve belonging to the tetramin series are as follows:

Chlorid,	Cr_2Cl_3, $4NH_3$, $2HO$.
Chlorplatinate,	Cr_2Cl_3, $4NH_3$, $2HO+2PtCl_2$.
Chlorhydrargyrate,	Cr_2Cl_3, $4NH_3$, $2HO+6HgCl$.
Chlorobromid,	Cr_2ClBr_2, $4NH_3$, $2HO$.
Bromid,	Cr_2Br_3, $4NH_3$, $2HO$.
Bromochlorid,	Cr_2BrCl_2, $4NH_3$, $2HO$.
Chloro-iodid,	Cr_2ClI_2, $4NH_3$, $2HO$.
Iodid,	Cr_2I_3, $4NH_3$, $2HO$.
Chlorosulphate,	Cr_2ClO_2, $4NH_3 2SO_3$, $2HO$.
Bromosulphate,	Cr_2BrO_2, $4NH_3 2SO_3$, $2HO$.
Chlorochromate,	Cr_2ClO_2, $4NH_3 2CrO_3$, xHO.
Chloronitrate,	Cr_2ClO_2, $4NH_3$, $2NO5$, $2HO$.

All these salts are crystalline and easily soluble in water. They have a carmine red or garnet red color and their solutions are easily decomposed on heating, with separation of chromic oxyd and evolution of ammonia. The author calls attention to the very noteworthy fact that they all contain 2 atoms of water (in the old notation).

The second series of compounds described are the salts of heptamin-dichromium.

The formulas of the only observed members of this series are as follows:

Double nitrate,	$2(Cr_2O_3, 3NO_5), 7NH_3+NH_4O, NO_5+9HO$.
Oxalo-nitrate,	$2(Cr_2C_3, NO_5, (C_2O_3)_2), 7NH_3+6HO$.

The salts of the triamin series are as follows:

Oxalate,	Cr_2O_3, $3C_2O_3$, $3NH_3$, $3HO$.
Double oxalate,	$2(Cr_2O_3, 3C_2O_3, 3NH_3)+NH_4O, 2C_2O_3, HO+3HO$.

The salts of the heptamin and triamin series resemble those of the tetramin series so closely as not to require special description. The nitrate of the heptamin series is obtained by the action of nitrate of silver upon the chlorid of tetramin-chromium. The oxalate of the triamin series is obtained by the action of oxalic acid upon the same chlorid. Besides these three series the author describes the following salts, which may obviously be regarded as members of other and analogous groups.

$$Cr_2O_3,\ NO_5,\ 2NH_3+3HO.$$
$$Cr_2O_3,\ C_2O_3,\ NH_3+8HO.$$
$$2(C_2O_3),\ SO_3,\ 2NH_3+24HO.$$

It is, however, very doubtful whether these three substances were obtained in a state of purity, as they were all amorphous and could not be obtained crystallized.—*Kongliga Svenska Vetenskaps-Akademiens Handlingar*, *Ny Följd*, 1866, 7 vol., 2d half.

W. G.

[*Note.*—The compounds described by Cleve are of especial interest when considered from an atomistic point of view. The formulas of the triamin and tetramin series are of course to be doubled and we should then have for the three chlorids the empirical formulas

$6NH_3, \text{Ꞓr}_2Cl_6$	Hexamin-chromium chlorid.
$7NH_3, \text{Ꞓr}_2Cl_6$	Heptamin-chromium chlorid.
$8NH_3, \text{Ꞓr}_2Cl_6$	Octamin-chromium chlorid.

Adopting Blomstrand's view of the constitution of the analogous platinum and cobalt compounds, these three bodies may be written as follows:

$$\overset{vi}{\text{Ꞓr}_2}\left\{\begin{array}{l}-NH_3-Cl\\-NH_3-Cl\\-NH_3-Cl\\-NH_3-Cl\\-NH_3-Cl\\-NH_3-Cl\end{array}\right. , \quad \overset{vi}{\text{Ꞓr}_2}\left\{\begin{array}{l}-NH_3-Cl\\-NH_3-Cl\\-NH_3-Cl\\-NH_3-NH_3-\text{Ꞓl},\\-NH_3-Cl\\-NH_3-Cl\end{array}\right. \quad \overset{vi}{\text{Ꞓr}_2}\left\{\begin{array}{l}-NH_3-Cl\\-NH_3-Cl\\-NH_2-NH_3-Cl\\-NH_3-NH_3-Cl,\\-NH_3-Cl\\-NH_3-Cl\end{array}\right.$$

Cleve suggests that the heptamin series may be only double salts of his tetramin and triamin compounds, and in view of the unsymmetrical structure of the chlorid this seems at least probable. It is also to be borne in mind that the want of symmetry is only seen if we adopt Blomstrand's theoretical views, and does not appear when the ammonia bases are formulated according to the principles of variable atomicity supported by the writer.* The invariable occurrence of two atoms of water in all the octamin compounds is difficult to explain upon any theory.—W. G.]

2. *On some new sulphur salts.*—SCHNEIDER has described some new sulphur salts belonging to the same class with those which we have already noticed. Potassio-platinous sulpho-platinate is easily obtained by fusing 1 or 2 parts of platinum sponge with 6 parts of pure potassic carbonate and six parts of sulphur, and treating the cooled mass with water. The author gives to this salt the formula:

$$K_2S, Pt''S, Pt''S, \} Pt^{iv}S_2$$

and points out its analogy to a copper salt described in a previous paper. The new compound forms small, hard, sharp and distinctly formed, six-sided tables of a blue gray color and strong metallic luster. The larger tables have a reddish tint and in thin layers are translucent with a red brown color. The density of the salt is 6·44. Dilute chlorhydric acid slowly dissolves out the potassium without the slightest evolution of sulphuretted hydrogen, the final product of the reaction being sesqui-sulphid of platinum Pt_2S_3 or platinous sulpho platinate PtS, PtS_2. This is a steel gray crystalline powder of density 5·52, which when heated in the air burns like tinder and leaves pure spongy platinum. Sodio-platinous sulpho-platinate has a precisely analogous formula, sodium replacing potassium. It is a crystalline powder of density 6·27 and with a color between blue-gray and reddish lead-gray. A re-examination of the compound containing tin, platinum, potassium and sulphur, described in his previous paper, has led Schneider to the conclusion that this body contains no oxygen, and that its true formula is

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* This Journal, vol. xlix, p. 108.

$$K_2S, Pt''S, Pt''S, Pt''S \} Sn^{iv}S_2,$$

in which the quadrivalent platinum is replaced by quadrivalent tin. In addition to the sodium salt corresponding to the compound last formulated, Schneider describes a disodium salt which has the formula $Na_2S, Na_2S, Pt''S, Pt''s \} Pt^{iv}S_2$.

When freshly prepared, this salt forms thin brilliant light copper red needles. It rapidly changes color in the air and becomes brown and finally almost black. Water dissolves it with partial decomposition, leaving a dark red crystalline powder of bisulphid of platinum. Schneider obtained by double decomposition silver and thallium salts corresponding to the disodium compound.—*Pogg. Ann.*. cxxxviii, 604. W. G.

3. *Synthesis of Hydroxylamin.*—By the action of tin and chlorhydric acid upon ethylic nitrate LOSSEN obtained the chlorhydrate of a new base, $NH_3\Theta$, which he termed hydroxylamin. Ludwig and Hein have succeeded in preparing this body by the direct addition of nascent hydrogen to nitric oxyd, expressing the reaction by the equation: $N\Theta + H_3 = NH_3\Theta$,
which is more correctly written $2N\Theta + 3H_2 = 2NH_3\Theta$. The nitric oxyd was prepared by the action of nitric acid upon ferrous sulphate and collected in a glass gasholder from which it was made to pass through a series of flasks containing tin and boiling chlorhydric acid. After separating the tin and ammonic chlorid the chlorhydrate of hydroxylamin was obtained with all the properties described by Lossen.—*Berichte der Deutschen Chem. Gesellschaft*, *2ter Jahrgang*, p. 671. W. G.

4. *On a new group of double chlorids belonging to the platinum bases.*—By adding solutions of various metallic chlorids strongly acidulated with chlorhydric acid to a solution of the chlorid of Reiset's first base, $PtCl, 2NH_3$ or $4NH_3, PtCl_2, PtCl_2$, BUCKTON obtained a series of double chlorids embraced by the general formula $4NH_3, PtCl_2, PtCl_2$. Thomson has found that a second series of double chlorids of the same quantitative constitution but with wholly different properties is obtained by adding an ammoniacal solution of a metallic salt as copper, silver, nickel, &c., to a solution of double chlorid of platinum and ammonium, $PtCl_2, 2NH_4Cl$. The new salts are crystalline and vary in color according to the metal used; they are insoluble or with difficulty soluble in water and ammonia, but dissolve rather easily in dilute chlorhydric acid and are precipitated from this solution by ammonia. Buckton's salts are soluble in water and insoluble in chlorhydric acid.

W. G.

5. *A system of instruction in Quantitative Chemical Analysis;* by Dr. C. R. Fresenius, from the last English and German editions, edited by Prof. S. W. Johnson of Yale College. 8vo, pp. 631. New York: (John Wiley & Son). 1870.—In explanation of the alterations made in this American edition we quote the following from Prof. Johnson's preface.

"In preparing this edition of Fresenius' Quantitative Chemical Analysis, the editor has sought by various changes to adapt it to the wants of the American student.

The foreign editions have attained such encyclopedic dimensions as to occasion the beginner no little confusion and embarrassment. For this reason the bulk of the work has been considerably reduced. A few processes which the editor's experience has convinced him are untrustworthy, and many more that can well be spared because they are tedious or unnecessary, have been omitted.

The section on Organic Analysis has been reduced from sixty to thirty pages, mainly by the omission of processes which from their antiquity or inferiority are more curious than useful. The chapters on Acidimetry and Alkalimetry have been likewise greatly condensed, and all that especially relates to Soils and Ashes of Plants has been left out. The recent appearance of an excellent special treatise on "Agricultural Chemical Analysis" by Professor Caldwell, of Cornell University, justifies the last named omission.

On the other hand, some important matter has been added. Bunsen's invaluable new methods of treating precipitates are described in his own (translated) words. Various new methods of estimation and separation are incorporated in their proper places.

The additions which have been made to the methods of examining ores, it is believed, adapt the work to meet all the ordinary requirements of the metallurgical and mining student."

The editor acknowledges his indebtedness to Dr. Wolcott Gibbs, Dr. J. Lawrence Smith, Mr. O. D. Allen and others for important contributions and descriptions of new methods. We are confident that this excellent work will be of great service in advancing the study of analytical chemistry in this country, and that its publication will be welcomed by all teachers and students of quantitative chemical analysis.

6. *Effects of the Sun's Heat on a Sand Hill.*—Extract from a letter of Geo. Davidson, Esq., of the Coast Survey, dated U. S. Coast Survey Station, San Buenaventura, Cal., January 23d, 1870, (communicated for this Journal, by Mr. D. B. Smith, of Germantown, Pa.)—I have had a very curious experience at this station. It is on the edge of a sandy, steep bluff, seventy feet above the low flat margin that extends 300 yards to the sea beach. At the station, I had an 18 inch theodolite, with three reading microscopes, and was engaged in determining the azimuth of the principal lines of the triangulation from the station San Buenaventura. This involved observations from sunrise to 10 a. m., and from 3½ p. m. to 11 p. m. Imagine my surprise when I found that the heat of the

sun pouring all the P. M. upon the southwest face of this bluff so expanded it that the level showed changes as great as 45″! Then in the evening contraction began, and continued until the level at sunrise exhibited changes of 45″ the other way. Here *was a change of* 1′ 30″ *certainly due to changes of temperature;* our lowest temperature was about 40°; our greatest about 79° in the shade, say 100° in the sun.

But this is not all: I was dismayed to find that in cooling during the evening, the tongue of the bluff upon which the station is situated, *twisted irregularly* in azimuth as much as 18″ in three hours. This, of course, vitiated all my results, and I continued a full series simply as an experiment, for I could not change my position for an eccentric one without many drawbacks. I did change my latitude instrument and transit from their positions near the station, and where the same phenomena were exhibited by them. At 102 yards from the edge of the bluff they are as steady as a rock, and I have nothing but the excessive undulations of the heated air to contend with.

7. *On the existence of Ammonium in the Ammoniacal Amalgam, and on a new Test for the presence of Nascent Hydrogen;* by ALBERT H. GALLATIN, M. D., of New York.—Berzelius and De Pontin in 1808, using the voltaic current as Davy had done, endeavored to do as much for the ammoniacal compounds as he had done for those of the fixed alkalies. They made what is known as the ammoniacal amalgam. That ammonium exists in this body has never been demonstrated, notwithstanding that its constituents in their proper proportions were always found escaping from the amalgam: that does not prove thea they were united; on the contrary, 2 vols. of NH_3 and 1 vol. of H are the products. Moreover, if it were ammonium, it had never been made to unite with any other metal than mercury. I have endeavored to overcome both of these objections.

If the hydrogen escaping from the mercury together with the ammonia can be shown to be in the nascent state, it would be evidence that it had just been in chemical combination with the ammonia, in other words, that metallic ammonium (NH_4) existed in the amalgam. Some pellets of sodium were placed in contact with some particles of the transparent variety of phosphorus, wrapped in bibulous paper and plunged beneath the surface of water. A red glow was seen; and the nascent hydrogen from the decomposing water came into contact with the phosphorus, bubbles of phosphid of hydrogen were formed. Occasionally one would flame as it came into contact with the atmosphere, placing the nature of the reaction beyond doubt. As phosphid of hydrogen cannot be formed by direct synthesis if ordinary free hydrogen be employed, this becomes a test for the presence of that gas in its nascent state. The hydrogen escaping from the ammoniacal amalgam was now tested by this process. A sodium-amalgam dipped beneath a solution of chlorid of ammonium was employed; and it became necessary to wait until the sodium was exhausted, that results might not be vitiated by the nascent hydrogen escaping from the water. At the proper time the decomposing amalgam was covered with fragments of transparent phosphorus, when many bubbles of inflammable phosphid were obtained. The hydrogen must then have been in the nascent state and just escaping from the ammonium.—*Phil. Mag.*, xxxviii, 57.

8. *On the Existence of an Alloy of Ammonium and Bismuth, and on another new Test for the presence of Nascent Hydrogen;* by Albert H. Gallatin, M. D., of New York.—Ammonium had never yet been seen united with any other metal than mercury. Mercury being the only metal fluid at ordinary temperatures, should another alloy be formed it would be solid. Some bismuth was melted in a porcelain dish and alloyed with sodium by dropping a piece of that metal on the clear surface of the fluid bismuth. Chlorid of ammonium was then dusted on the fluid alloy, and then water added in a fine quick stream. The bismuth swells, appears pasty and porous, and then congeals. Abundance of hydrogen escapes from the water, and the ammoniacal odor is set free. This body must now be dried. If it be placed near the ear a distinct crackling noise will be heard, a phenomenon which endures for some days. To ascertain if this be ammonium escaping from the bismuth, the body was placed beneath the surface of water, when bubbles of hydrogen escaped, easily to be collected and recognized; the ammonia, if any, must have been absorbed by the water. To test for this red litmus-paper was placed in the liquid. Wherever the currents from the bismuth struck it a blue spot became visible. On dissolving sulphate of copper in distilled water and placing the well-dried bismuth therein, the characteristic flocculi of ammonio-sulphate of copper appeared at once.

It remains to show that the hydrogen escaping is in the nascent state. There was not enough of it to test with phosphorus. The bismuth compound, when placed in a solution of sulphate of copper, becomes rapidly coated with metallic copper. Now bismuth unalloyed will not precipitate copper from its sulphate. To test if the precipitation of the metallic copper was due to the presence of nascent hydrogen, an alloy of bismuth and sodium was made and dipped in a solution of sulphate of copper. It instantly became coated with that metal, owing to the nascent hydrogen escaping from the water. The hydrogen was therefore escaping in the nascent state from the bismuth and ammonia, and therefore it was a true alloy of bismuth and ammonium. If the temperature of this alloy be raised, it will rapidly decompose with a crackling noise. On one occasion it exploded, sharply scattering the metal. The loud crackling noise produced by this substance may be heard for many days after it is made. That there is no mere surface-action in the case of the mercurial and bismuth alloys of ammonium, is shown by the pores which are formed by the escaping gases in both cases. In the amalgam these pores may be seen produced by the escaping ammonium long after the water has exhausted the sodium. In the mercurial body the pores are evanescent; in the case of bismuth they remain, and may be examined at leisure.

These are different phenomena from those displayed hy spongy platinum when it forces hydrogen and oxygen to combine.

Appendix.—Continuation of the investigation at the laboratory of the Royal Mint, London, by the kind permission of Mr. Roberts:—

The alloy was dried *in vacuo* over sulphuric acid. It was then heated *in vacuo* by means of a Sprengel pump, when it decomposed, and the resulting gas was collected over mercury. It was found to have twenty-seven times the volume of the original solid. Analysis of the gas proved it to contain nitrogen and hydrogen. The results of a further examination will shortly be given.—*Phil. Mag.*, IV, xxxviii, 58. June 23, 1869.

II. MINERALOGY AND GEOLOGY.

1. *Preliminary Field Report of the U. S. Geological Survey of Colorado and New Mexico*, conducted, under the authority of Hon. J. D. Cox, Secretary of the Interior, by F. V. Hayden, U. S. Geologist. 156 pp. 8vo. Washington, 1869.—Prof. Hayden, in this expedition of the year just past, was over his old field again, in which he has done so much by his observations and collections to promote the progress of American geology and paleontology. His explorations, however, were continued farther west, to and among the heights of the summit. His route was from Cheyenne, where he commenced his labors, to Denver; from Denver to the silver mines at Georgetown, the gold mines of Central City, and thence to the Middle Park; then back to Denver, and southward to Colorado City, Soda Springs, Cañon City, Spanish Peaks, Raton Hills, Fort Union, Mora Valley, and Santa Fé; thence up the Rio Grande, through the San Luis valley, Poncho Pass, Arkansas Valley, through the South Park to Denver again. Prof. Hayden remarks that "the coal formation along the base of the mountains was studied with great interest. With these coal beds are associated valuable deposits of brown iron ore. The coal and iron deposits of the Raton Hills extend from the Spanish Peaks to Maxwell's, and the supply of both is quite inexhaustible and of excellent quality. The future influence of these two important minerals at this locality, on the success of a Pacific Railroad, cannot be over-estimated. It is believed that the coal and iron mines of the Raton Hills will be of far more value to the country than all the mines of precious metals in that district.

The next locality for coal was at the Placiere Mountains. In one locality here, the coal has been changed into anthracite by the eruption of a basaltic dike, the igneous material of which had poured over the coal strata. Vast quantities of brown iron ore are associated with this coal, and magnetic iron ore is found in the gneissoid rocks of the mountain. The gold mines here are very rich and are now wrought upon a true scientific plan."

As this Report consists of field notes, the geological facts are present as they were observed along the principal routes; and we shall wait for a systematic review of the results from the author before presenting them, and cite in this place only some of the paragraphs from his account of the trip to the famous "Middle Park."

Our route to the Middle Park was through the Berthoud Pass, from the valley of Clear Creek. The range of mountains in which the pass is located is composed of gneissic rocks—as are all the ranges in the mining districts. The ascent was very steep on the south side, up to the region of perpetual snow; but the descent on the north side is quite gradual.

Great quantities of loose materials from the basis rocks are scattered thickly over the summits, of every variety of the metamorphic class. Most of the peaks are well rounded, and covered with soil and vegetation. Grass and flowers grow far up above the limits of arborescent vegetation. As we ascend, the pines, spruces and cedars dwindle down in size, until they become recumbent and trail on the ground. Some of the highest peaks are very sharp, and covered with loose rocks, as if only the usual atmospheric influences had ever affected them. Their sides are often massive escarpments of rocks down which an infinite quantity of fragments have fallen, making a vast amount of debris at the base. Of course their rocky sides are entirely free from vegetation, and the oxyd of iron gives them a rusty reddish appearance. One mountain at the head of Clear Creek is called Red Mountain, from the fact that the rocks have a bright red color in the distance. The evidences of the outpouring of igneous rocks in this mountain are very marked; indeed, it may be called an eruptive range.

From the summit of Berthoud's Pass, at a height of eleven thousand eight hundred and sixteen feet, we can look northward along the line of the main range, which gradually flexes around to the northwest, while the little streams seem to flow through the rifts. The general appearance of the western slope of this great range would indicate that it is a huge anticlinal, composed of a series of ranges on each side of a common axis, and then smaller ranges ascend like steps to the central axis. The western side of this ridge slopes gently, while the eastern side projects over abruptly. This main range also forms a narrow dividing line, or "water-divide," between the waters of the Atlantic and Pacific. I stood where the waters of each side were only a few feet apart, and felt a real joy in passing down the western slope of the mountain by the side of a pure crystal stream whose waters were hastening on to the great Pacific.

The Middle Park is really made up of a number of smaller parks, which are somewhat independent of each other. Each one may present different geological formations. The little park on the south side, which we first enter, is a very beautiful one. The grass is luxuriant, and the timber excellent. None of the older sedimentary rocks were seen along the flanks of the mountains, but a recent tertiary deposit seemed to cover the country. On the east side of Fraser Creek there is a long high ridge, which is cut by the stream in several places, formed of the white and yellow sands and marls which mark the pliocene tertiary on the east side of the mountain. I have no doubt that it is a formation of the same kind as that of the Arkansas marls, and cotemporaneous with it.

Along this creek there are some massive walls of this formation, mostly yellow marls, but some layers of sandstone. This ridge extends from the mountains far northward, and is about two miles wide; and between it and the immediate base of the mountains is situated a beautiful valley of considerable width.

The Middle Park is apparently a quaquaversal, surrounded by the lofty snowy ranges; and the lower ranges descending like steps to the valley which constitutes the true park. The park does not appear to be more than from ten to twenty miles wide from east to west, and from fifty to sixty long from north to south. In this park, also, the ranges of mountains so surround it that the slopes seem to form a sort of quaquaversal inclining toward a common center.

Viewed from Middle Park, Long's Peak, and the range immediately connected with, has a rugged, saw-like edge, as if composed of eruptive rocks, and ridge after ridge inclines from it in regular order.

About ten miles north of our camp, in the first park, we come to low ridges of massive red feldspathic granite, and parallel with these granite ridges are a series of sedimentary beds, commencing with the brick-red beds. The strike is nearly north and south, and the dip west. These ridges are all so grassed over that the true nature of the underlying rocks is not easily determined. Then comes ridge after ridge, untill all the beds—jurassic and cretaceous—are shown.

On this stream we have a fine system of terraces. On the north side are three distinct terraces above the bottom, and the lowest one has a bed of cretaceous sandstone, nearly horizontal, cropping out at its base. This is a low one, not more than fifteen feet high; the next one is fifty feet high, and the third, which descends from the high hills, is two hundred feet. A little west of south, at the junction of Grand River with Frazer Creek, five high peaks are visible, which form in that direction a part of the main range. All around us, in every direction, we could see the snowy peaks, and the beds which form the ridges of upheaval inclining in every direction.

To the south of the park, the older sedimentary rocks dip north in lofty ridges, at least two thousand feet high, presenting high escarpments when split by streams, and reaching almost the highest margin of the mountains.

About ten miles above the hot springs, Grand River flows through an enormous gorge cut through a high ridge of basalt, which seems to be an intrusive bed, for above and below the sedimentary rocks are well shown, but partially changed. Underneath are the cretaceous shales of Numbers 4 and 5, and above are the lignite tertiary beds. These beds all dip west 23°.

These eruptive rocks are very rough, as if they had been poured out without much pressure. Much of it is a very coarse conglomerate, the inclosed masses appearing to be the same kind as the paste; that is, originally, of igneous origin. Some of the inclosed rocks are very compact, close, and all were more or less worn, before being inclosed. This rock is a true dolorite. I did not see any inclosed masses that I could call unchanged. This basalt extends a great distance, continuing a nearly uniform thickness, and inclining in the same direction with the cretaceous beds below and the tertiary beds above.

On both sides of Grand River, but especially on the east and northeast sides, extending up nearly to the foot of Long's Peak, are quite large exposures of the recent tertiary beds. They are nearly horizontal, and have much the appearance in color of the Fort Bridger beds, of which Church Buttes is an example. These beds are composed, for the most part, of fine sand and marl, but there are a few small rounded boulders scattered through it. Below the gorge, on the north side of Grand River, these outflows of basalt have formed some well-defined mesas; at least three beds, ascending like steps from the river. The basalt lies in horizontal beds, but on the south side is the sloping side of a basaltic ridge. The dip is nearly northwest, though the trend of this basaltic ridge is by no means regular. One portion of it has a strike northwest and southeast, and another north and south. The tertiary rocks reach a great thickness, and are elevated high up on the top of the basaltic ridge, eight hundred to one thousand feet above the river. They are mostly formed of fine sandstone and puddingstone. These fine sandstones contain some well-marked impressions of deciduous leaves, among which are good specimens of *Platanus haydeni.* On the north side of Grand River, in some localities, the tertiary beds are elevated so high, on many of the eruptive mountains, that they are covered with perpetual snow. These eruptive beds are certainly among the most remarkable examples of the overflow of igneous matter that I have ever seen in the West. * * * *

Immediately below the hot springs the Grand Cañon commences, and the river cuts its way through an upheaved ridge of massive feldspathic granite, for three miles, between walls from one thousand to one thousand five hundred feet high. The south side is somewhat sloping, and covered thickly with pines, while the north side is extremely rugged, the immense projecting masses of granite forbidding any vegetation to gain a foothold. It would seem that the river had worn its way through a sort of rift in the granite, but at the upper end it has cut through the uplifted sedimentary ridges nearly at right angles. In some places the north side is gashed out in a wonderfully picturesque manner, so that isolated columns and peaks are left standing, while all the intermediate portions have been worn away. This granite ridge will average perhaps five miles in width, and extends an unknown distance across the park, northeast and southwest, and it is from the southeast side that the ridges of upheaval, above described, incline.

The granite ridge seems to form a sort of abrupt anticlinal. On the southeast side the rocks all are bare, or covered with a superficial deposit of recent tertiary marls. None of the older unchanged rocks are seen on this side, but the modern sands and sandstones are exposed in a horizontal position in the channel of the river.

The hot springs are located on the right bank of Grand River, at the juncture of the sedimentary rocks with the granites. Just east of the springs is a high hill, Mount Bross, one thousand to one thousand two hundred feet above Grand River, which seems to be composed mostly of the older tertiary strata, alternate yellow and gray sandstones, and laminated arenaceous shaly clays. The whole is so grassed over, that it is difficult to take a section. The beds incline east of north at a small angle. I regard the beds as of the age of the coal formations of the West, older tertiary. I found excellent impressions of deciduous leaves, among which are those of the genus *Magnolia.* Just opposite the springs, the left bank of the river shows a perfect section of all the layers, from the cretaceous to the jurassic. The bank is not more than ten feet thick above the water, and yet it shows that the river itself rolls over the upturned edges of all these beds.

The section in descending order is as follows:

1. *Tertiary* strata, forming the greater part of the hill known as Mount Bross. The *Cretaceous*, consisting of—

2. Gray laminated sandstones, passing down into arenaceous clays, with *Baculites ovatus*, &c.

3. Black clays of No. 4. These are of great thickness, and every variety of texture. As shown in a cut bank of the river, it is yellow arenaceous clay, with layers of sandstone, in which the impressions of deciduous leaves were observed. These layers project up, a distance along the bank, of seventy paces.

4. Dark plastic clay, with cone in cone, seams of impure clay, iron ore. Then comes an interval in which no layers could be seen, sufficient to include No. 3—two hundred and fifty paces.

5. Dark steel-black laminated slate, with numerous fish scales; dip twenty-seven degrees. This slate passes down into alternate layers of rusty sandstone and shaly clay. * * * *

I have given this detailed description of the cretaceous rocks to show the exceeding variableness of their texture, and also to call the attention of scientific men, who may hereafter visit this interesting locality, which will soon become celebrated, to a section of the rock through which the waters of the spring must pass in reaching the surface. Now, in whatever rocks these springs may originate, the water must pass a long distance through the almost vertical strata of the Cretaceous period, in the sediments of which are found in other localities nearly all and perhaps all the mineral constituents found in these springs. The deposits around these springs are very extensive. No analysis has as yet been made, but large masses of gypsum and native sulphur can be taken out at any time from the sides of the large basin-like depression into which the water flows. They are properly "Hot Sulphur Springs," varying in temperature from 80 to 112°.

Troublesome Cañon, at the head of the creek bearing this name, is entirely basaltic, and the rugged walls, not only of the main stream, but also of the little branches, form a most picturesque view.

Below the Cañon, the valley of Troublesome Creek, and also that of Grand River near the junction, is occupied by belts of modern tertiary sands and marls, like those observed at the entrance to the park, by Berthoud's Pass. Where the little stream cuts the terraces, horizontal strata of whitish and flesh-colored sands and marls are exposed. I looked in vain for fossils, and found only specimens of silicified wood. There are cold sulphur springs in this valley. All through the park, the benches or terraces are conspicuous in the vicinity of streams, as at the base of mountain ranges. In the park through which Frazer's Creek flows, these benches or terraces are most beautifully carved out from the modern marls.

In summing up the geology of Middle Park, we find that all the sedimentary rocks known in this country are found there. I did not see any beds that I could define as carboniferous, but the triassic, jurassic, cretaceous, and tertiary are well developed. I have no doubt as to the existence of true carboniferous limestones in the Middle Park.

The tertiary deposits of this region may be divided into two groups, viz., the lignite or older tertiary, and the modern pliocene marls and sands, which seem common to the parks and mountain valleys. The former conform perfectly to the older beds, while the latter seldom incline more than three to five degrees, and do not conform to the older rocks. The marl group is undoubtedly contemporaneous with the Arkansas and Sante Fé marls.

This volume contains also a valuable Report of P. Frazer, Jr., on the mines and minerals of Colorado, and another by Cyrus Thomas, on the agriculture of Colorado.

2. *The Gold Fields and Mineral Districts of Victoria, with Notes on the Modes of Occurrence of Gold and other Metals and Minerals;* by R. Brough Smyth, F.G.S., &c., Secretary for Mines for the colony of Victoria, Melbourne. 644 pp. Royal 8vo. 1869. (Trübner & Co.)—We have examined this elaborate volume with equal pleasure and instruction. It is, in fact, an extensive treatise, full of minute details and exact information rendered in an unpretending matter of fact style. The work is illustrated by plans, sections, sketches, diagrams and maps, and is carefully printed and accompanied by ample tabular statements, statistical and otherwise.

Perhaps the most striking thing brought to light by the Victoria gold fields, to one who is also familiar with those of California, is the fact that, to all appearance, the area of the Australian fields yet unexplored, or very imperfectly so, is vastly greater than any reserves known to exist upon the Pacific slopes of North America. Of a minimum of 20,000,000 acres of known gold fields in Victoria, not more than 600,000 acres, or one thirty-third part, have been opened and explored, while many of the older districts are still yielding, with improved methods and economy, better results than at any former period. The fact of the general similarity of the modes of occurrence of gold in gulches, deep lying placers, and in old river courses, under lava, and basalt, in Australia and California, has long been known, but the subject receives new and valuable illustration by the labors of Mr. Smyth. Hitherto comparatively little has been known in this country respecting the modes of occurrence and productiveness of the gold-quartz mines—*reefs*, as they are called—in Victoria. This want Mr. Smyth fully supplies. He tabulates the course and direction of nearly 500 quartz veins in the several mining districts, the directions being from N. 89° W. to N. 47° E.; but by far the greater number of veins run about N. 30° to 40° W.; and mention is made of results obtained from the workings of over 2400 mining companies, mostly engaged upon quartz. The number of quartz reefs (veins), actually proved to be auriferous, is over 2,600, and the total extent in square miles of auriferous alluvial and quartz ground worked upon is upwards of 880 square miles.

The average yield of gold from certain parcels of quartz, the details of which are given from 1859 to 1868, from returns made to the Mining Surveyor and Registrars, is shown to be from 5,816,669 tons 9 cwts. of quartz crushed, 3,346,201 oz. 8 dwts. of gold or an average, per ton of 11 dwts. 12·37 grains. This value is very unequally distributed between the different districts, as appears from an inspection of the details tabulated in the appendix. The average fineness of the Australian gold is considerably above that of California.

Full and very interesting details are given of several of the most important quartz mining operations, of which the Port Philip Co., at Clunes, is the most important. The following is a return of quartz crushed at Clunes for the 12 months ending September, 1865, viz.: 54,413 tons of quartz crushed yielded 20,596 oz. 10 dwts. 12 grains of gold, averaging 7 dwts. 13 grains per ton, and valued at $20 per oz.=$417,920. To crush this amount of quartz, 80 stamps were required, viz.: 56 heads of about 6 cwts., each striking 75 blows per minute, requiring about one horse power each, and crushing an average of about 2 tons, 4 cwts. each; while 24 stamps weighing about 8 cwts. each, crush 4 tons per diem each of fine material; about 8 gallons of water per head per minute is required by these stamps, or 921,600 gallons per diem. Two steam engines drive the stamps. Of the total gold saved, 66·08 per cent are found in the stamp beds, 22·95 in the mercury-boxes, and 10·97 comes from the blankets.

Compared with the best California mines, the yield per ton of the St. Philip, is small. Thus in 1868–9, 20,638 tons of gold quartz crushed at the Eureka Mill, Grass Valley, California, yielded $537,687.89, or an average of $27.80 per ton of ore; and this is considerably less than the average of some former years. The average of the Amador Company, Sutter Creek, on a somewhat greater crushing is over $25 per ton, and has steadily increased from $5 or $6 to the present rate in sinking to a depth of 1200 feet, which is double the depth of the St. Phillips, at Clunes.

Mr. Smyth discusses at length Sir Roderick Murchison's notion that quartz gold veins become poorer in depth, and effectually proves that there is no evidence of this; but after a careful comparison of over 200 examples, he announces the following general conclusions: 'Taking the whole of the information and results obtained into consideration, there is much reasonable evidence produced in support of the theory *that quartz reefs are richer as they increase in depth*, and in addition to this, *that they are wider*.'

A remarkable feature of the Australian gold fields, in which they are distinguished above all others, is the magnitude and frequency of the nuggets found in them. Mr. Smyth gives two interesting tabular statements of remarkable nuggets, the history of which is known, as compiled by Mr. Birkmyre, an experienced assayer, and the second by Mr. F. Knox Orme, the warden of Dunolly. Of Mr. Birkmyre's list (over 150 specimens), only 27 are American or European. We enumerate a few of the most remarkable Australian nuggets, compiled from both tables:

	lbs.	oz.
Dunolly, date, &c., unknown,	248½	
The "Welcome Stranger Nugget," found Feb. 2d, 1869, in the auriferous alluvium, near Bull Dog Reef, Dunolly, Victoria; 21 inches long, 10 inches in thickness; mixed with the quartz, but the great body of it solid gold; 986 fine; value £9534. The neighborhood of Dunolly is almost an unprospected country. For many miles there are out-cropping reefs which have yielded very large pieces of gold.	190	10
The "Welcome Nugget," found June 15, 1858, at Bakery Hill, Ballaarat, Victoria, at 180 feet depth; water-worn and irregular shaped; 20 inches long by 12 broad and 7 deep; by assay, 992 fine; value, £9.325.	184	9
The "Blanche Barkly Nugget," found 27th August, 1857, at Kingower, Victoria, at a depth of 13 feet, and within five or six feet of holes dug three years before. It measured 28 inches by 10, and contained about two lbs. of quartz, clay, &c.; assay, 955; value, £6.905.12.	145	3
Found at Canadian Gully, Ballaarat, Victoria, 31st of January, 1853, at a depth of 60 feet; 989 fine; value, £5,532 7s. 4d.	134	11
Found in July, 1851, by a native boy, in a heap of quartz at Meroo Creek, River Turon, 53 miles from Bathurst, N. S. W. It was in three pieces when found, though considered as one mass.	106	
Found in 1857 at Dunolly, Victoria, two specimens with gold distributed through a rust-colored matrix.	237	
Found Nov. 1st, 1858, at Burrandong, near Orange, N. S. W., at a depth of 35 feet, mingled with quartz and pyrites; of 874 fineness.	107	2
The "Lady Hamilton Nugget," found 1854, Sept. 8th, near Canadian Gully, Ballaarat, Victoria, at a depth of 135 feet.	98	1

Did space permit, this list might be continued, by far the larger number of nuggets found being under 100 lbs. Troy, and the history of some of them is sufficiently curious. Many of the large nuggets of Australia are distinctly referred to quartz veins in the immediate vicinity, and masses are specified of several pounds weight, which have been found attached to the veins. How rare this circumstance is in California, is well known; the only example of much significance being in the Morgan claim on Carson Hill. The famous Cabarras Co. (N. C.), (1821,) nugget of 34 lbs. weight was alluvial gold. The largest nugget found in California was at Valiceto, (24th April, 1852), near Carson Hill, and weighing 25 lbs. 5 ounces.

The following tables exhibit interesting information.

Returns showing approximately the gold obtained from quartz veins and alluvial workings during the years 1864 *to* 1868 *inclusive, Victoria:*

Year.	From Quartz Veins.		From Alluvial Workings.	
	oz.	dwts.	oz.	dwts.
1864	503,618	5	1,041,076	10
1865	450,000	0	1,093,801	0
1866	521,017	0	958,177	15
1867	560,527	0	873,160	6
1868	587,694	0	1,069,804	0

The ratio of quartz gold to alluvial gold is thus shown to be much higher in Australia than it is in California, where not over one quarter of the annual production is considered to arise from quartz, while in Victoria it is seen to be about one half.

Yield of Gold per annum: Victoria.

Year.	Quantity Exported. ounces.	Value at 80s. per oz.
1851 for 3 mos.	145,146	580,584
1852	2,218,782	8,875,128
1853	2,676,345	10,705,380
1854	2,150,730	8,602,920
1855	2,751,535	11,006,140
1856	2,985,991	11,943,964
1857	2,762,460	11,049,840
1858	2,528,478	10,113,912
1859	2,280,950	9,123,800
1860	2,156,660	8,626,640
1861	1,967,420	7,869,680
1862	1,658,207	6,632,828
1863	1,626,872	6,507,488
1864	1,544,694	6,178,776
1865	1,543,801	6,175,204
1866	1,479,194	5,916,776
1867	1,433,687	5,734,748
1868	1,657,498	6,629,992
Totals,	35,568,450	132,273,800

To arrive at the total quantity exported, it is necessary to add 1,267,241 ozs. to the above.

Mr. Smith's volume contains ample material for study, and offers many tempting extracts, but our space compels us to forbear.

3. *On Old Water Courses;* by Dr. J. S. **Newberry.**—(To the citations from the memoir of Dr. Newberry on Surface Geology at page 111, we here add the following.—Eds.)

"Some of the valleys and channels which bear the marks of glacial action—evidently formed or modified by ice, and dating from the ice period or an earlier epoch—are excavated far below the present lakes and water-courses which occupy them.

These valleys form a connected system of drainage, at a lower level than the present river system, and lower than could be produced without a continental elevation of several hundred feet. A few examples will suffice to show on what evidence this assertion is based.

Lake Michigan, Lake Huron, Lake Erie, and Lake Ontario are basins excavated in undisturbed sedimentary rocks. Of these, Lake Michigan is 600 feet deep, with a surface level of 578 feet above tides; Lake Huron is 500 feet deep, with a surface level of 574 feet; Lake Erie is 204 feet deep, with a surface level of 565 feet; Lake Ontario is 450 feet deep, with a surface level of 234 feet above the sea.

An old, excavated, now-filled channel connects Lake Erie and Lake Huron. At Detroit the rock surface is 130 feet below the city. In the oil region of Bothwell, &c., from 50 to 200 feet of clay overlie the rock. What the greatest depth of this channel is, is not known.

An excavated trough runs south from Lake Michigan—filled with clay, sand, tree trunks, &c.—penetrated at Bloomington, Ill., to the depth of 230 feet.

The rock bottoms of the troughs of the Mississippi and Missouri, near their junction or below, have never been reached; but they are many feet, perhaps some hundreds, beneath the present stream-beds.

The borings for oil in the valleys of the Western rivers have enabled me not only to demonstrate the existence of deeply buried channels of excavation, but in many cases to map them out. Oil Creek flows from 75 to 100 feet above its old channel, and that channel had sometimes vertical and even over-hanging cliffs. The Beaver, at the junction of the Mahoning and Shenango, runs 150 feet above the bottom of its old trough.

The Ohio throughout its entire course runs in a valley which has been cut nowhere less than 150 feet below the present river.

The Cuyahoga enters Lake Erie at Cleveland, more than 100 feet above the rock bottom of its excavated trough. The Chagrin, Vermilion, and other streams running into Lake Erie exhibit the same phenomena, and prove that the surface level of the lake must have once been at least 100 feet lower than now.

The bottom of the excavated channel in which Onondaga Lake is situated, and the Salina salt-wells bored, is at least 414 feet below the surface level of the lake and 50 feet below the sea level. (Geddes. Trans. New York State Agricultural Society, 1859.)

The old channel of the Genesee River at Portage, described by

Prof. Hall in the Geology of the 4th District of New York; the trough of the Hudson, traceable on the sea bottom nearly 100 miles from the present river mouth; the deeply buried bed of the Lower Mississippi, are additional examples of the same kind; while the depth to which the Golden Gate, the Straits of Carquinez, the channel of the lower Columbia, the Canal de Haro, Hood's Canal, Puget Sound, &c., have been excavated, indicates a similar (perhaps simultaneous) elevation and erosion of the Western coast of America.

The falls of the Ohio—formed by a rocky barrier across the stream—though at first sight seeming to disprove the theory of a deep continuous channel in our Western rivers, really afford no argument against it, for here, as in many other instances, the present river does not follow accurately the line of the old channel below, but runs along one or the other side of it. In the case of the Louisville falls the Ohio runs across a rocky point which projects into the old valley from the north side, while the deep channel passes under the lowland on the south side, on part of which the city of Louisville is built.

The importance of a knowledge of these old channels in the improvement of the navigation of our larger rivers is obvious, and it is possible it would have led to the adoption of other means than a rock canal for passing the Louisville falls, had it been possessed by those concerned in this enterprise.

I ventured to predict to Gen. Warren that an old filled-up channel would be found passing around the Mississippi rapids, and his examinations have confirmed the prophecy. I will venture still further, and predict the discovery of buried channels of communication between Lake Superior and Lake Michigan—probably somewhere near and east of the Grand Sable—at least, between the Pictured Rocks and the St. Mary's River—between Lake Erie and Lake Ontario through Canada,—between Lake Ontario* and the Hudson by the valley of the Mohawk,—between Lake Michigan and the Mississippi, somewhere along the line I have before indicated. I also regard it probable that a channel may be found connecting the upper and lower portions of the Tennessee River, passing around the Mussel Shoals. This locality lies outside of the area where the Northern Drift deposits were laid down to fill and conceal ancient channels, but the excavation and the filling up of the channel of the Tennessee—like that of the

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* When the water in the lake basin had subsided to near its present level, its old avenues of escape being all silted up by the Drift clays and sands the surplus made its exit by the line of lowest levels wherever that chanced to run. As that happened to lie over the rocky point that projected from the northern extremity of the Alleghanies into the lake basin, there the line of drainage was established in what is now known as Niagara river.

Though among the most recent of the events recorded in our surface geology, this choice of the Niagara outlet by the lake waters was made so long ago that all the erosion of the gorge below the falls has been accomplished since. The excavation of the basin into which the Niagara flows—the basin of Lake Ontario, of which Queenstown Heights form part of the margin—belongs to an epoch long anterior.

Ohio—were determined by the relative altitude of the waters of the Gulf. The channel of the Lower Tennessee must have been excavated when the southern portion of the Mississippi valley was higher above the Gulf level than now, and Prof. Hilgard has shown that at a subsequent period, probably during the Champlain epoch, the Gulf coast was depressed 500 feet below its present relative level. This depression must have made the Lower Mississippi an arm of the sea, by which the flow of the Ohio and Tennessee was arrested, their channels filled, terraces formed, &c. If the Upper Tennessee has, as appears, a channel lower than the Mussel Shoals, it must be somewhere connected with the deep channel of the lower river.

4. *The Volcano of Kilauea, and great Earthquake Waves;* by Rev. Titus Coan.—I have lately returned from a tour of exploration to the active crater of Kilauea and the volcanic district of Puna. At Kilauea the action was dull. The central area of this immense crater remains a deep concavity, depressed about 400 feet below its margin; but this margin is a new rim or black ledge of lava, itself depressed a thousand feet below the banks of the crater, and marking the former level of its bottom. In this profound basin I noticed a scanty growth of ferns rooted in the superficial strata of lava. They had gone down uninjured to a depth of 400 or 500 feet, as the crust subsided upon the disgorgement of the molten lava beneath. The old South Lake retains its locality; but its contours are entirely changed. It is now a nearly circular pit, about 400 feet deep in the bottom of the crater. Its diameter, as I measured, is four-fifths of a mile, and its walls, of black hardened lava, are jagged and frowning; in some places beetling; in others, perpendicular or retreating. To this abyss access is anything but easy; it is possible to enter it at but a single place, and by making a *trajet* over a steep, rugged, and difficult incline of *debris*, yet a brave American lady of my party ventured to descend under my guidance. Here, 2,000 feet below the surface of the ground, we tramped together over the floor of the awful pit. It seems now the half-cooled forge of Vulcan. The fiery billows no longer roll and break as they were wont to do over this vast area of indurated lavas; yet, in many places we could look into red-hot ovens and chimneys, and down through orifices in the crust to the molten sea below, and hear, and see, and feel the incandescent minerals boiling, and surging, lashing the sides of the deep cavern, and sending up volumes of white sulphur vapors. But now this principal focus of volcanic action is comparatively quiet. Our last great eruption has lowered its fiery tides far below their usual level; and we are content to enjoy this lull in the strife of the telluric forces which are so ably described in Dr. C. F. Winslow's recent work on Force and Nature—a work which gives the completest explanation that has yet appeared of volcanic phenomena in Hawaii. I spent three days at Kilauea, making careful observations of the crater; and, when these were completed, went to the seashore at Ke-a-la-ko-mo, a village in the volcanic district of Puna, situated about twenty

miles from Kilauea. In this district the subsidence of the land was distinctly marked. Throughout a coast line of many miles in extent the shore has settled from six to eight feet.

On the 25th of July last a remarkable flood tide visited the southern shore of Puna, tearing away the faces of the sea-cliffs, sweeping over barriers 25 feet high, rolling in foaming surfs hundreds of feet inland, and carrying with it huge boulders and angular masses of rock of from a ton to three tons in weight. The sea rose nearly *thirty feet* in perpendicular height, or ten feet higher than the great earthquake wave, already described in your Journal, of the 2nd April, 1868. Several houses which were not reached by that wave were swept away on the 25th. By the latter calamity nearly everything has been destroyed upon the high shores of Ka-ha-na-le-a, and the delicious bath in the fissure at Pu-na-luu is doubled in depth. Nor are the changes that the plastic volcanic agency is impressing upon this country yet completed. At Ka-la-pa-na the sea is more and more invading the land. Two channels are opened through the beach, and the tide ebbs and flows over large fields of the Ka-la-pa-na plain. The old stone church of the village is deeply buried in sand and boulders, and the tides sweep entirely over it. From Ka-mai-li to Ka-po-ho the shores are terribly torn by the action of the waves. Between the villages of O-pi-hi-kaa and Po-ho-i-ki the waves dashed a thousand feet inland and destroyed a mile of road running parallel with the shore, upon a line never before reached by the sea. But for the declivity of our shores, these gigantic waves would have penetrated much further inland. The point of disturbance, from which these remarkable waves radiated, is still unknown to us. The focus of the great Peruvian earthquakes of 1868 was near the city of Arica, where the first shocks were felt at 4¾ P. M., Aug. 13. The resulting earthquake wave reached our islands, a distance of 6,000 miles to the northwest, late at night *upon the same day*, and, traveling toward the southwest, reached the harbor of Lyttleton, New Zealand, at 4¾ o'clock in the morning of the 15th. Making the allowances in time required by a difference in longitude of 243 deg. 18 min. (counting eastward), it appears that the wave occupied but 19 hours and 17 minutes in making the latter distance of 7,200 miles. This makes 368 miles per hour, or 540 feet per second, about half the nominal velocity of a cannon-ball. On the 23d of December, 1854, a similar wave was transmitted across the entire breadth of the Pacific Ocean, from Japan to California, in 12 hours and 38 minutes. The great earthquake wave of April 20, 1868, passed from our Hawaiian shores to the coasts of Mexico, California, and Oregon, in *five hours'* time, as indicated by self-registering tide-gauges at San Francisco and Astoria, which announced its arrival upon the evening of the same day in which it had desolated the coasts of Puna and of Ka-u. These waves are entirely distinct from the tidal swing of the ocean, and are proper *waves of translation.—N. Y. Tribune.*

I returned to Hilo by the coast-route, experiencing occasional earthquakes by the way, some of them a little startling. These still continue at intervals; but we are inclined to expect a season of comparative volcanic quiet at present, and have no sensation, in the way of natural phenomena, in definite prospect earlier than the transit of Venus in 1874. We are already promised visits by scientific observers upon that occasion.

5. *A Treatise on Ore Deposits;* by BERNHARD VON COTTA, Professor of Geology in the Royal Saxon School of Mines; translated from the second German edition, by FREDERICK PRIME, Jr., Mining Engineer. 594 pp. 8vo. New York, 1870. (D. Van Nostrand).—Mr. Prime has done a good thing for all interested in the study of ore deposits, in giving us a translation of Van Cotta's "Erzlagerstättenlehre." This book has long been the standard authority in Germany, and we are fortunate in now having an edition of it in English, which is even more complete than the last German edition, as Mr. Prime not only had the approval and permission of the author, to translate the work, but Prof. v. Cotta also has made alterations and furnished additions to it, so that it is in reality a new edition. The work commences with a general discussion of the different kinds of metalliferous deposits, covering some 90 pages, and then takes up, by way of illustration, the description of the most important mining districts of Europe, closing with a chapter of theoretical considerations, suggested by the observed facts. We trust that the publication of this excellent work will inspire our mining engineers and geologists with a sense of the importance of carefully observing and recording the facts connected with the occurrence of ore deposits in this country. The work already done by some of our best observers, especially such admirable researches as those of Prof. J. D. Whitney on the Upper Mississippi lead region, shows what may be done. The forthcoming reports of the California survey, and of the survey of the 40th parallel, will undoubtedly furnish a large amount of new material, which, with the facts already published in various geological and mining reports, might form the basis of a work on American "ore deposits," that would very appropriately supplement Mr. Prime's translation of von Cotta's book, and give a still further impulse to this important study.

6. *Notice of Asbestus and Corundum with other Minerals at Pelham, Mass.;* by J. H. ADAMS, Senior in Amherst College.—In the towns of Pelham and Shutesbury, Mass., there are several localities of asbestus, known to the mineralogists in this part of the state, although no special mention is made of them in any scientific work, they being scarcely noticed in the geological survey. Surface specimens of indifferent quality have been common in collections for years. Dark colored pieces, hard and compact, have sometimes passed for petrified wood, to which there is a strong resemblance. The most noted of those localities, is on the land of Mr. Samuel Newell in Pelham. Recent excavations to obtain asbestus for economical purposes reveal its presence here in great

quantity. Much of it is soft, of a grayish-white color, composed of delicate parallel fibers, often one foot in length, with a marked cross cleavage oblique to the fibers, as in hornblende.

Intimately associated with the asbestus, is an altered mica, allied to vermiculite or jefferisite. This appears in irregular veins, separated by a decomposed asbestiform mineral, and granular actinolite, which crumbles upon exposure. It occurs in loose foliated scales and small angular aggregations, occasionally several inches in size. Hardness, about 1·5. Luster pearly, sometimes metallic. Color light brass-yellow to yellowish brown, scarcely transparent, even in very thin folia. Flexible, inelastic. Before the blowpipe exfoliates remarkably, and at last fuses slightly on the edges. This property however is variable. Of five specimens taken, only two exfoliated in a marked degree. The laminæ after heating appear as if melted or pressed together, and not readily separating. It differs in this respect from the jefferisite of Westchester, Pa., but seems to resemble it in optical characters. Both exhibit a six-rayed astersim, although from its extreme opacity a sufficient thin lamina of the Pelham mineral is obtained with difficulty.

These veins of altered mica are free from admixture with soil or other minerals; and the substance is often so loose as to be readily displaced by the hand without the aid of a pick. Farmers use it as an absorbent for bedding cattle. Might it not be of service in other ways?*

Not the least interesting circumstance, however, is the presence at this spot of corundum. Flattened masses of brownish-black mica, superior in hardness to the surrounding mineral, contain nodules of corundophilite which inclose a white corundum. It is often streaked of a deep sapphire blue. This is, I think, the second observation of the kind in northern New England, the other locality being at Chester. It rarely occurs in situ, and in small quantity; but doubtless farther excavations will develop more valuable specimens. The locality promises considerable interest to the mineralogist; and it is believed that the asbestus may prove of sufficient value to warrant mining.

Amherst College, Nov. 13, 1869.

7. *Note on the Remains of Fossil Birds;* by O. C. MARSH.—Just after the article on pages 205 to 217 had gone to press, I received from Professor F. V. Hayden a unique specimen, which forms a most interesting addition to the fossil bird remains there described. It is the distal portion of a large feather, with the shaft and vane in such excellent preservation, that it may perhaps indicate approximately the nature of the bird to which it belonged. The specimen was discovered in a fresh-water Tertiary deposit, of the Green River group, in Wyoming Territory, and will be figured and described in full with the osseous remains noticed in the above article.

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* A similar mineral is used in Europe for sanding paper, in place of blotters.

8. *The Dinornis and Saurian remains in Australia;* by Rev. W. B. Clarke.—A femur of a species of Moa or Dinornis has been announced by Rev. W. B. Clarke as found in the Pleistocene drift of Peak Downs, Australia. The femur was probably buried in a fresh water fluviatile drift, over which was formed a lacustrine deposit. Mr. Clarke regards the discovery as evidence of the former connection of Australia and New Zealand, and makes the following additional observations on the subject. "Returning, in conclusion, to the supposed connections, in former geological times, of New Zealand and Australia, a fresh testimony has been offered to me within the last few days, in the statement of a discovery during the last few months of abundant remains of Saurian reptiles along the Waipara River in New Zealand, of the same kind as those which have been found to occur along the Flinders River in Queensland, and which, in addition to the Plesiosaurus discovered by the same friend, Mr. Hood, show that there was a similar geological fauna in times as far back at least, as the Cretaceous period. The Plesiosaurus was mentioned in my paper 'On recent geological discoveries in Australasia' (1861). Although now the flora is dissimilar, yet there is evidence that in even earlier geological times, such as our Carboniferous epoch, for instance, there was a similar flora.

"The views I have adopted lead me to maintain, that we in Australia inhabit only a fragment of a vast continent, of which in New Zealand there was probably, as there still is, the culminating ridge or the highest portion of Pacific dry land. That this opinion is not without support, I find, by a passage in Dr. Mantell's account of his son's discoveries of Dinornis. Without remembering that passage (which, indeed, I am not sure I ever read till this evening), I have expressed myself in language nearly identical with that of Dr. Mantell, who says, in 1850, 'It seems probable that these stupendous animals were not anciently confined within the narrow limits of modern New Zealand, but ranged over a vast continent, that is now *submerged*, and of which the isles of the Pacific are the culminating points.'"

9. *Synopsis of the Extinct Mammalia of the Cave Formations in the United States, with observations on some Myriapoda found in and near the same, and on some Extinct Mammals of the Caves of Anguilla, W. I., and of other localities;* by Edward D. Cope. (From the Proceedings of the American Philosophical Society, Philadelphia, vol. xi, p. 171, 1869.)—This includes an account of remains of mammals discovered in a cave breccia in Wythe county, Virginia, by the author, among which several extinct forms occurred associated with existing species. The new species are *Stereodectes tortus* (gen. et sp. nov.) allied to *Arctomys; Tamias lævidens; Sciurus panolius; Mixophagus spelæus; Galera perdicida.* Associated extinct species are *Megalonyx Jeffersonii; Tapirus Haysii; Dicotyles nasutus; Ursus amplidens,* and others. Among the recent species are *Castor fiber; Neotoma? Floridanaum; Lepus sylvaticus; Cariacus (Cervus) Virginianus; Procyon*

lotor, etc. The species previously described by Le Conte and Leidy, from a similar deposit in the lead-bearing limestone of Galena, Ill., are also included in the list. The whole number of species is 27, of which 14 are extinct and represent 5 extinct genera, while 6 genera are of "neotropical type." Seven species of land shells, and remains of reptiles and birds were also found. The species of *Myriapoda* obtained are enumerated and several new species and a new genus, *Andrognathus*, are described. In the second part of the paper two genera of gigantic rodents from Anguilla are described: *Amblyrhiza inundata* and *Loxomylus longidens*. In connection with these a human instrument of shell was discovered. The third contains descriptions of *Anoplanassa forcipata*, a new genus and species of Cetacea from near Savannah, Georgia, and *Hemicaulodon effodiens*, a large Sirenian allied to the Dugong, from the Eocene Marl of Monmouth county, N. J. v.

10. *The Extinct Mammalian Fauna of Dakota and Nebraska*, including an account of some allied forms from other localities, together with a *Synopsis of the Mammalian remains of North America*, illustrated with 30 plates; by JOSEPH LEIDY, M.D., LL.D., Professor of Anatomy in the University of Pennsylvania; preceded with an introduction on the *Geology of the Tertiary Formations of Dakota and Nebraska*, accompanied with a map; by F. V. HAYDEN, M.D., Professor of Mineralogy and Geology in the University of Pennsylvania, etc. Vol. VII, of 2nd series, of the Journal of the Academy of Natural Sciences of Philadelphia. Philadelphia, 1869.—In this large and beautiful volume, issued by the Academy of Sciences of Philadelphia, Dr. Leidy has given a thorough revision of all his own labors and those of others, with regard to the ancient Mammalian Fauna of the Upper Missouri region. As he observes in his preface, the materials for this work "have been gradually and continuously accumulating for the last twenty-three years." Through the new facts which have come to light, the author has been enabled to revise his former determinations of species, and to write out the characteristics of each with far more precision and fullness. The synonymy is well worked up and contains many points of historical interest. The value of the work is much enhanced by the addition of the "Synopsis of the Mammalian remains of North America," which occupies 84 pages of the volume, and must have cost the author a vast amount of labor. The thirty lithographic plates show exactness in delineation, and illustrate not only the species of the Nebraska region, but also several of those of other parts of the country.

A large part of the personal observations on the geology of the region, and of the collections of its fossils have been made by Prof. F. V. Hayden, and it is therefore especially appropriate that the introductory chapter should have come from his pen. A large colored map of the geology of the region accómpanies the work which was also prepared by Prof. Hayden. A further analysis of the work we have to defer to another occasion.

11. *On the Geology of the New Haven Region, with special reference to the orign of its Topographical features;* by JAMES D. DANA. 70 pp. 8vo, with a map. From vol. II, of the Transactions of the Connecticut Academy.—The author concludes, from the facts brought out in this paper, "that, by special facts, and by the course of events, this region in the Glacial era, like that of New England to the North, was moulded at surface largely by the action of the Connecticut valley glacier and its underflowing streams, and covered through the subsequent melting of the ice, with stratified and unstratified drift-formations simultaneously; that icebergs had no part in the matter, and the supposed iceberg sea over New England no existence."

12. *On the Mixture of Cretaceous and Eocene Fossils*; by T. A. CONRAD, (communicated for this Journal).—We frequently hear of passage beds between the Chalk and Eocene, but the evidence is never produced which would show a gradual transition from one to the other. It has been said that in California such beds have been found; but Mr. Gabb in his extensive explorations did not observe them. The collection of fossil shells which he considered a newer Cretaceous formation, and designated as No. B is now in the collection of the Academy of Natural Sciences, and a careful revision of the species fails to enable us to detect one recognized form of the Cretaceous Period of California, or anywhere else. It is a peculiar group, however, unlike any other known. A glance at the large *Venericardia Hornii*, will strike any one accustomed to compare the Chalk and Eocene fossil shells as peculiarly indicating the Eocene. This statement of the distinct character of different formations, takes no account of those local mixtures of fossils which are easily traced to disturbances in the bed of the ocean.

13. *Diamonds in Australia.*—A letter from the Austrian Consul in Australia, to Mr. Hochstetter of Vienna, announces that the diamonds of Australia are remarkable for size as well as beauty. They vary in weight from half a carat to 150. One found on the property of the Consul, was of the first water, and weighed 30½ carats; and another, of 46 carats, was sold in London at 128,000 francs. The region especially abundant in diamonds is the frontier of the Orange River country, at Sikatlory.—*Les Mondes, Jan.* 13.

14. *The Æpyornis of Madagascar.*—MM. ALP. MILNE-EDWARDS and ALF. GRANDIDIER describe in Comptes Rendus, 11th Oct. 1869, certain curious anatomical peculiarities of the bones of the Æpyornis, and they observe "that it belongs to a group of shortwinged birds, but constitutes amongst them a type characterized by its massive forms, and by feet of a size difficult to conceive. It must be placed beside the dinornis and apteryx, although it is removed from them by important features of its organization, and by the pneumaticity of its thigh bones. The height of this bird was much less than J. Geoffroy St. Hilaire thought. Taking the length of its foot as a basis of calculation, the Madagascar bird scarcely exceeded two metres, which is the height of a large ostrich, while the *Dinornis giganteus* varied from two and a half to three metres. But if the Æpyornis is not, as was supposed, the

biggest of all these birds, it is the stoutest, the most massive, the most elephantine, if we may so express it."—*Student, Nov.* 1869.

15. *Preliminary Notice of the Lamellibranchiate Shells of the Upper Helderberg, Hamilton and Chemung Groups;* [*Preparatory Studies for the Palaeontology of New York*]; part 2; by JAMES HALL. 80 pp. 8vo. State Col. Nat. Hist. 1869.—This paper by Prof. Hall contains descriptions of several new genera and species, and revised references of other old species. The new genera are PALÆANEILO for *Nuculites constricta* Conrad, etc.; LIMOPTERA for *Lima macroptera* Conrad; MYTILARCA for *Megambonia ovata* Hall, etc.; PHOLADELLA for *Nuculites radiata* Conrad; CIMITARIA for *Cypricardites corrugata* Conrad, etc.; PHTHONIA for *Cypricardites sectifrons* Conrad; MODIOMOPHA for *Cypr. oblonga* Con. and *Modiola concentrica* Hall, etc.

III. ZOOLOGY.

1. *Molluscan Fauna of New Haven. A critical review of all the Marine, Fresh Water, and Land Mollusca of the region, with descriptions of many of the living animals and of two new species;* by GEO. H. PERKINS, Ph.D. From Proceedings of the Boston Society of Natural History, November and December, 1869.—The total number of species given in this catalogue is 162; of these 97 are Gasteropods (51 marine); 54 Lamellibranchs (40 marine). Of the 91 marine species 50 are said to occur north of Cape Cod; 13 in Labrador; 8 in Greenland; 8 in Europe; 51 extend to South Carolina and some of them farther; 37 occur in the Post Pliocene; 26 in the Pliocene; 19 in the Miocene. A list of 65 species recorded from Long I. Sound, but not yet found at New Haven, is given at the end. The two new species described and figured are *Nassa fretensis* (like *N. vibex*) and *Astarte lutea* (allied to *A. sulcata*). A new generic name, in errata *Tottenia* (by error *Tottenniana*) is proposed for *Venus gemma* Totten, and *Crassivenus* instead of *Mercenaria* for *Venus mercenaria* Linn., the name, *mercenaria*, being objectionable because properly a *specific* name and an adjective. *Mytilus hamatus* Say is referred to *Brachydontes*, and *Pleurotoma brunnea* is proposed for *P. plicata* Adams.

The synonomy is far from complete, and although completeness could hardly be expected in a catalogue of this kind, yet it seems desirable to give, if any, such references as are necessary to explain the nomenclature adopted and the principal synonyms in all cases. But besides want of completeness there are many positive errors that are scarcely excusable even in a local list. In looking it over casually the following errors were noticed, besides others of less importance.

Thus "*Melantho decisa* Binney," should have H. & A. Adams as authority, and "*Vivipara decisa* Gill" in the synonymy, should be *M. decisa* Gill, instead of omitting the reference entirely (as in errata), for Prof. Gill was the first to correctly limit the two genera, as found in this country.

The "*Cytherea Sayii* Conrad," p. 147, should be *Cytherea Sayana* Conrad, Jour. Phil. Academy, vol. vii, p. 124, 1834; the reference to Gould, "p. 34," should be p. 84; "*Callista convexa* Say," should be *Cytherea convexa* Say; and finally the correct reference for "*Callista convexa*" is Adams' Gen., ii, p. 425. This species is really a *Callista*, unless we adopt Römer's subgenus, *Caryatis*, to which it also belongs. But Conrad's grounds for rejecting Say's name, *convexa*, seem to be insufficient,—at least I am unable to find another species of *Callista* with the same name. "*Mercenaria violacea* Stimpson," should be *M. violacea* Schumacher, "*Modiola modiolus* Linn," should read *M. modiolus* Turton, (*Mytilus modiolus* Linn.), and *M. barbatus* is no doubt a distinct Mediterranean species. "*Scapharca transversa* Say," should be *S. transversa* H. & A. Adams, (*Arca transversa* Say).

The following names, quoted as having Stimpson (Check List), Tryon, Conrad, etc., as authorities, are found in H. & A. Adams' Genera of Recent Mollusca, and some of them, perhaps, in earlier works:—*Amycla Gouldiana*, *A. dissimilis*, *Tritia trivittata*, *Cerithiopsis Emersonii*, *Lunatia heros*, *L. triseriata*, *Turbonilla interrupta*, *T. nivea*, *Melantho decisa*, *Bittium Greenii*, *Tectura testudinalis*, *Martesia cuneiformis*, *Xylotrya palmulata*, *Siliqua costata*, *Angulus tenera*, *A. polita*, *Peronea tenta*, *Macoma fusca*, *Brachydontes plicatula*, *Scapharca transversa*. V.

IV. ASTRONOMY.

1. *Elements of Asteroid* (109); by E. H. F. PETERS, of the Litchfield Observatory of Hamilton College. Communication dated Clinton, Oneida Co., N. Y., November 25, 1869.*—The following elements of asteroid (109) are computed from my observations of Oct. 9, 20 and 31.

Epoch 1869,	Oct. 0·0 Berlin mean time.	
Mean anomaly,	337° 1′ 3″·35.	
Longitude of perihelion,	55 53 48·04.	Mean Equ. 1870·0
Longitude of node,	4 51 45·43.	
Inclination of ecliptic,	7 56 56·55.	
Angle of eccentricity,	17 25 14·13.	
Mean daily motion,	809″·580.	
Logarithm of major semi-axis,	0·4278314.	

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On Force and Will;* by B. A. GOULD.—(We copy the following from an able address of Dr. Gould, at a meeting of the American Association last August, at Salem, Mass., as retiring President of the Association.)—

Scientists are now of accord that "force can neither be created nor destroyed," and that "the quantity of force in nature is just as eternal and unalterable as the quantity of matter." Its various forms are eminently convertible, yet utterly indestructible. And to avoid that fruitful source of disagreement among the ablest men, which has arisen from the ambiguous signification of the word, we must adopt the meaning which is finding general acceptance, and define force as "that which is expended in producing or resisting motion;" thus clearly discriminating between force and its cause.

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* This announcement was issued on a loose slip in connection with the January number of this Journal.

In his retiring address before this Association, last year, our honored ex-president Dr. Barnard presented an argument, so vigorous and clear that I see no room for an adequate rejoiner, in opposition to the doctrine which would extend the principle of the conservation of force to the phenomena of consciousness,—"a philosophy which at the present day is boldly taught in public schools of science, and which numbers among its disciples many very able men." He says, for instance:—

> "Organic changes are physical effects, and may be received without hesitation as the representative equivalents of physical forces expended. But sensation, will, emotion, passion, thought, are in no conceivable sense physical."—[*Proc. Amer. Assoc.*, *Chicago*, p. 89.]
>
> "The philosophy, which makes thought a form of force, makes thought a mode of motion; converts the thinking being into a mechanical automaton, whose sensations, emotions, intellections. are mere vibrations produced in its material substance by the play of physical forces, and whose conscious existence must forever cease when the exhausted organism shall at length fail to respond to these external impulses."—[*Ibid*, p. 91.]
>
> "Thought cannot be physical force, because it admits of no measure. * * A thing unsusceptible of measure cannot be a quantity, and a thing that is not even a quantity cannot be a force."—[*Ibid.* pp. 93, 4.]

Before the cogent reasoning carried out by President Barnard, of which the general tenor is indicated by these quotations, the view that force affords a middle term between the moral and the material worlds can be sustained as little as the pure materialism against which the argument was directed. But if we ascend a grade higher, and consider that which guides and compels force, as force guides matter, I am disposed to believe that the problem may be nearer to a solution. Yet I offer my views with hesitation, not unmindful of the great thinkers who have considered these exalted topics, and shrinking from the rebuke of presumption.

There is an elegant experiment, in which the tension of a spring is made to produce heat by percussion, thus developing the current from a thermo-electric battery, which by successive modifications of its force exhibits heat, chemical action, magnetic attraction, and finally bends another spring; the same original force successively appearing in all these various manifestations until it is reëstablished in its primitive form. In such an experiment the imperfections of the apparatus would of course entail some loss at each successive step, and thus preclude the practical recovery of an available force equal to that expended in the original flexure of the spring. Yet the fact is beyond question that such loss is due solely to the inadequacy of our implements for collecting and transmitting the force at each stage of the experiment; for the law of conservation teaches that it is in every instance converted into other form or forms without diminution. Could such an apparatus be constructed with theoretical perfection, it would represent an eternal circuit of force; and, like the frictionless pendulum in a vacuum, it would exhibit a perpetual motion, after the needful impulse had once been applied. The spring would oscillate forever, did no extraneous force oppose, whether the force producing its rebound were or were not transmitted through a chain of modifications.

In this inert apparatus no force whatever would have been embodied, yet qualities would have been implanted by design, which would compel an indestructible force, applied to it, to play the part of an unwilling Proteus. The influence seems unavoidable that force may be guided and controlled, compelled to exert itself in this or that shape, without the outlay of any other force for the purpose. If it be objected that it is an intrinsic law of force that it shall change its form in exerting itself, the case is in nowise altered by the expression of this truism. Our design has prescribed, and (extraneous force being absent) might indefinitely prescribe, the modes and directions in which that constant force should manifest itself.

Muscular force is directed, and in its vital action is usually controlled, by will. If we assume it to be coequal with the expenditure of tissue,* measurable alike by its transferred results and by the decomposition of this tissue, where and what is that power which lets loose or withholds this force, and whose action is attended by a conscious effort? It is the will,—a something which directs and controls force without expending it. Not only are thought and forms of consciousness not forces, if the reasoning already adduced be correct, but, although often moral incentives to the will, they are not even motive energies, in the sense in which I think we must concede the will to be such. It is true that the exercise of thought is followed by fatigue, yet it is not attended by a sense of effort, except in so far as it is directed by an exertion of the will. And although the former doubtless consumes tissue, have we any reason for believing that the exercise of will does the same, apart from that consumption which corresponds to the forces whose mode of action it prescribes?

Thus it would appear that the metamorphosis of force, though not "work done" in the mechanical sense, is the result of some definite mode of causation. What this causation is, and whether it is susceptible of measurement, are the next questions. In the same category with this agency, or energy, or influence, the vital principle would seem to belong,—directing forces while it neither expends nor consumes them. In the growth of organic beings, unstable combinations are formed, and organized structures are thence reared, in which, as Kant has so beautifully said, "all parts are mutually ends and means." If in such organic development force is consumed, disorganization without decomposition ought to evolve it. Of the deposit of force in the unstable material of the tissues, I am not speaking, but of the vitality itself, which represents an energy requisite for the development and growth of organisms,—their dissolution being in turn attended by development of inferior forms of life, which suggest that this energy may have again been made available,—an energy too which is not "force," as this term has just now been defined.

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* Even if it be also, to some extent, supplied by the disorganization of food not fully converted, the argument is not thereby affected.

No comparison can be drawn between vitality and those molecular forces which build the crystal. Crystaline forms arise when the molecular attractions enjoy the freest scope, and their construction must be attended by an evolution of force, which ought to be recognizable by physical tests, and which should also be measurable by an excess of their resistance to solution, over that of comparatively amorphous masses of the same material, in which equal weights present equal surfaces.

So, too, not only in that individuality which life confers and in the impossibility of insulating or transferring vitality, but also in its hereditary character and its apparent susceptibility of indefinite increase or diminution, the vital energy violates our fundamental conceptions of force, and demands a separate category, seeming to belong in the same with will. If will and life be forms of force, their total amount must be limited by the law of conservation. If, on the other hand, they are outside the realm of forces, we may more readily indulge the conviction to which experience would lead, that their freedom is unfettered by any restrictions within our knowledge,—each injoying an indefinite, though possibly a correlated scope in its own domain. The indestructibility of both matter and force implies a fixed coefficient of force for matter in equilibrium; but how great is the contrast offered in this respect by such energies as life and will!

Now if this reasoning be correct, we may have in this class of energies that middle term, so earnestly desired and so intensely needful, which unites the phenomena of matter with those of spirit, and forms the connecting link between science and religion; their harmonious conjunction affording the highest system of philosophy. It is this class of energies which, controlling the forces of matter, guides and governs their modifications and transformations. It is this, moreover, which, inseparable from mind, is exerted by all conscious organism. The mystic play of coequal, but to our senses, so dissimilar forces, and the equally recondite mutual action of the eye, the brain and the nerve, alike demand agencies transcending all our science, yet implicitly obeying physical laws. The highest manifestations of these agencies is in will; the highest agent is the Almighty. Thus the dictum of faith, that the universe exists only by virtue of the continued will of its Creator, represents a palpable scientific fact; and we may see that the pantheist, the materialist and the spiritualist (I will not be debarred from this noble word hy the associations of its misuse to-day) have been contemplating the same exalted truth from different aspects, with limited ranges of vision.

2. *On Auroral appearances and their connection with the phenomena of Terrestial Magnetism*; by Balfour Stewart, F.R.S. F.R.A.S.—Some years since, I ventured to suggest that auroral displays might be secondary currents due to small but rapid changes, caused by some unknown influence, in the magnetism of the earth. In developing this idea, the earth was compared to the core of a Ruhmkorff machine, and the moist upper strata of the earth, as well as the upper strata of the atmosphere, to secondary conductors, in which currents will take place whenever the magnetism of the earth changes from any cause. These views would appear to be confirmed by the very interesting records of earth-currents obtained by Mr. Airy at the Greenwich Observatory, in which it is found that during times of very great magnetic disturbance there are strong earth-currents alternating from positive to negative, the curves lying nearly equally on both sides of the zero.

A further development of this idea has lately occurred to me, in consequence of a remark of my friend Mr. Lockyer, that the zodiacal light may possibly be a terrestial phenomenon, and may therefore be somehow connected with the phenomena of terrestial magnetism. For not only will secondary currents be caused in a stationary conductor in presence of a magnetic core of variable power, but also in a conductor moving across the lines of force of a constant magnet. The question arises, have we on the earth such moving conductors? In answer to this, let us reflect what takes place at the equator. When once the anti-trades have reached the upper regions of the atmosphere, they will become conductors from their tenuity; and as they pass rapidly over the lines of the earth's magnetic force we may expect them to be the vehicles of an electric current, and possibly to be lit up as attenuated gases are when they conduct electricity. May not these form the zodiacal light?

Such moving currents will, of course, react on the magnetism of the earth. We may therefore suppose that somewhat sudden and violent changes are likely to take place in the earth's magnetism at those seasons at which the earth's great wind-currents change most rapidly. May not this account for the excess of disturbances at the equinoxes?

Besides the anti-trades there are also, no doubt, conviction-currents, caused by the daily progress of the sun, taking place in the upper regions of the earth's atmosphere. May not these also be vehicles of currents as they cross the lines of the earth's force, and account, to some extent at least, for the daily variations of terrestrial magnetism? and may not this be the reason of the likeness observed by Mr. Baxendell between the curves denoting the daily progress of the wind and those denoting the variations of the declination of the magnet? Such currents (in as far as they are electric conductors), taking place in the upper regions of the atmosphere, would not be felt by the earth-current wires at Greenwich, and I think Mr. Airy has noticed that this is the case. But the tidal wave represents a motion of a conductor on the earth's surface, with two periods in one lunar day. This motion cannot produce a very great secondary current; but may it not be sufficient to account for the lunar-diurnal magnetic variation, which is also very small?

Such a current taking place in a conductor electrically connected with the earth's upper surface ought to be felt by the Greenwich wires; and, if I am not mistaken, Dr. Airy has detected a current of this nature.

May we not also imagine that there are two varieties of aurora —one corresponding to stationary conductors under a very rapidly changing core, and the other to rapidly moving conductors under a constant core? And might not an aurora of the latter kind indicate the approach of a change of weather?

These remarks are thrown out in order to invite comment and criticism, and they will have served their purpose if they direct attention to the part that may be played by moving conductors in the phenomena of terestrial magnetism. It will be noticed that these remarks do not touch upon the mysterious connection believed to exist between magnetic disturbances and the frequency of solar spots.

P. S.—Since writing the above, Sir W. Thomson has called my attention to a paper by him in the Philosophical Magazine for December, 1851, in which it is suggested that moving conductors may play a part in the phenomena of terrestrial magnetism.—*Monthly Notice of the Royal Astronomical Society, Dec. 10th*, 1869.—*Phil. Mag.*, IV, xxxix, 159.

3. *The approach of violent storms announced by telegraph.*—In December, 1869, a memorial was presented to the House of Representatives, from Prof. J. A. Lapham, of Milwaukee, Wisc., calling the attention of Congress to the fearful loss of life and property occurring annually on our Great Lakes, and suggesting the possibility of doing something to prevent at least a portion of this loss in future. A bill was at once introduced by Hon. Halbert E. Paine, of Wisconsin, providing that the Secretary of War be authorized and required to provide for taking the necessary meteorological observations, at the military stations in the interior of the continent, and for giving notice on the northern lakes and Atlantic coast, by means of the electric telegraph, of the approach and force of storms. Letters were subsequently presented to the House of Representatives, from the Surgeon General of the U. S. Army, from Prof. Joseph Henry of the Smithsonian Institution, from Prof. Elias Loomis of Yale College, and from the chief Signal Officer of the U. S. Army, approving of the proposal of storm warnings, and suggesting some of the advantages which might be expected to result from them. The bill was passed in the House of Representatives, Feb. 2, and in the Senate Feb. 4, 1870. The following is a copy of the bill as adopted:—

"Be it resolved, etc., That the Secretary of War be and he hereby is authorized and required to provide for taking meteorological observations at the military stations in the interior of the continent, and at other points in the States and Territories of the United States, and for giving notice on the northern lakes and on the sea coast, by magnetic telegraph and marine signals, of the approach and force of storms."

If the system authorized by this act should be prosecuted earnestly and faithfully by the War Department, we anticipate that it will result not only in an important addition to our knowledge of the laws of storms, but will materially diminish the number and severity of marine disasters. Systems of this kind are already in operation in England, France, Holland, Italy, and other countries, and are producing important results. Our Atlantic seaboard is more favorably situated for receiving intelligence of approaching storms, than the western coast of Europe, since a majority of our violent storms have their origin on the land, and moving eastward may be telegraphed in advance to the principal commercial cities of the Atlantic coast.

4. *The Sars Fund.**—We are glad to find that the appeal made in our pages by Mr. Gwyn Jeffreys, on behalf of the family of the late Professor Sars of Christiania, is being warmly seconded in Paris by M. Alglave, the editor of the *Revue des Cours Scientifiques.* In the last number of the *Revue* Mr. Gwyn Jeffreys' article is reprinted *in extenso*, and an announcement made that subscriptions to the Sars Fund will be received at the office of that journal. But M. Alglave has not waited for the publication of his notice before beginning his good work; he has already collected the sum of 2,026 francs (81*l.*), and publishes with the notice a first subscription-list containing the names of many of the most eminent naturalists in France. We have now the pleasure of giving in our advertising columns a list of the contributions already promised to Mr. Gwyn Jeffreys. Sars belonged to the best type of scientific men, the genuine lover of science, contented to work in obscurity without thought of honors or reward. His family have a special claim to help, inasmuch as the distress in which they are left is not due to neglect or extravagance on the part of the lamented Professor, but is solely attributable to his having devoted himself to studies, which, notwithstanding the most self-denying labors, did not enable him to make any provision for the future. Those of our readers who have visited Norway, who know the genuine unworldly ways of the Norwegians, and who have enjoyed the enthusiastic welcome so readily given to the English, have now a graceful opportunity of reciprocating the kindly feeling shown them by the countrymen of Sars.—*Nature, Feb.* 3.

5. *The Family of the late Prof. Michael Sars.*—In a notice of Prof. Sars, the editors state in the American Naturalist, Salem, Mass., of March, 1869, that in view of the fact that American Zoölogists are deeply indebted to Prof. Sars for the light he has thrown upon many of the lower animals, in the unrivalled investigation embodied in his publications, we feel it a duty to solicit aid for his family. Any sum, however small, which may be sent them will be welcomed, acknowledged and forwarded to his family through the Norwegian Minister.

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* The death of this eminent zoologist of Norway, Mr. Sars, is mentioned in our January number, on page 144.

6. *Lighting Power for Buoys. Premium for the year* 1871 *offered by the Netherland Society for the Promotion of Industry.*—One of the greatest impediments to navigation is darkness in buoyed waters. If it were possible to develop a lighting power in the buoys, this difficulty would be greatly diminished, to the advantage of navigation.

The Society offers, therefore, her gold medal (representing a value of hundred fifty florins, Neth. Cy.) and an award of three hundred florins for the most practical means of investing buoys with a lighting power, for service at night.

Conditions.—1. The answers must bear a distinctive mark, epigram or motto, and be accompanied by a sealed envelope, containing the competitor's name, and bearing outside the same mark, epigram or motto as above.

2. The competitors are requested to communicate an intermediate address, in case of eventual correspondence.

3. The answers and any other accompanying writing must not be in the competitor's own hand.

4. The successful answer becomes the property of the Society, which reserves to itself the right of publication.

5. The Society takes no responsibility for eventual damage to models or instruments, illustrative of the answers, and reserves to itself the right of not returning them to the competitors.

6. Answers are requested post paid before the 30th of September, 1871, to the address of the General Secretary and Treasurer of the Society, F. W. VAN EEDEN, Haarlem, the Netherlands.

7. *Academy of Sciences, Paris.*—Mr. A. DESCLOIZEAUX, the distinguished Crystallographer and Mineralogist, whose optical researches in connection with crystals have done so much to advance the science of mineralogy, has been elected a member of the Academy in place of the late Mr. d'Archiac; and Professor Mayer, the eminent physicist, in place of the late Professor Matteucci.

OBITUARY.

REV. GEORGE JONES, U. S. N.—On the 22d of January, 1870, at the Naval Asylum, in Philadelphia, died Rev. George Jones, long connected with the United States Navy as a Professor and a Chaplain; but better known in science for his important contributions to our knowledge of the zodiacal light. His labors in this department fill the third volume of the U. S. Japan Expedition, embracing observations from April 2d, 1853, to April 23d, 1855, made chiefly on board the U. S. steam frigate Mississippi, and are included on 340 charts, followed by Cassini's observations (10 in number) in 1685 and 1687. In his Introduction to this volume, Mr. Jones expresses his intention to spend a year at Quito, Ecuador, with a view to compare and extend his zodiacal light observations from that equatorial and lofty position. This purpose he executed in the year 1856, adding greatly to the fullness of his researches upon this wonderful phenomenon. A summary, by himself, of his observations in Ecuador was published, with three plates, in vol. xxiv of this Journal, 1857, p. 374; but the great mass of data then collected remain still unpublished. Mr Jones was a patient and most conscientious observer, and his contributions in this department of astronomy must ever form an important feature in any discussion of the phenomenon, notwithstanding Prof. Piazzi Smyth's most extraordinary and flippant assertion that Mr. Jones had never seen the zodiacal light at all!

Mr. Jones was the author of Sketches of Naval Life, 2 vols., New Haven, 1829, and Excursions to Cairo, Jerusalem, &c., one volume, New York, 1836, besides other works of a religious character. It was largely owing to the assiduity and patient tact of Mr. Jones that the U. S. Naval Academy was created against the instructive prejudices of most of the older naval commanders, a labor for which he never sought credit or fame, but not the less worthy of being remembered and recorded to his lasting honor. Mr. Jones graduated at Yale College in 1823, and was a tutor there from 1828 to 1830.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Transactions of the Chicago Academy of Science*, Vol. I, Part II, 1869. Large 8vo, with steel engraving and fourteen plates.—The first volume of Transactions of the Chicago Academy, now completed, does great honor to the society and to the enlightened liberality of those gentlemen who have furnished the means of illustrating it with so many beautiful plates. The first article is a biographical sketch of Dr. Robert Kennicott, the late Director of the Academy, with portrait and interesting extracts from the diaries, written during his well-known arctic explorations. The article on the Antiquity of Man in North America, by Dr. J. W. Foster, is of special interest at this time, and contains a summary of all the evidences hitherto obtained upon the subject. It is illustrated by six plates. The "List of the birds of Alaska, with biographical notes," by Wm. H. Dall and H. M. Bannister, and Prof. Spencer F. Baird's descriptive list of the "Additions to the Bird-Fauna of North America made by the Scientific Corps of the Russo-American Telegraph Expedition," illustrated by excellent colored plates of all the species, are very valuable contributions to American Ornithology. In the former 212 species of Alaska birds are enumerated, with valuable notes on their habits and distribution. Mr. S. H. Scudder enumerates 46 species of butterflies collected by J. A. Allen in Iowa, several of which are described as new. V.

2. *Guide to the Study of Insects;* by A. S. Packard, Jr., M. D. 8vo, 702 pages, with eleven plates and 650 wood-cuts. Salem, Mass., 1869. Naturalist's Book Agency.—This excellent work, published in ten numbers, is now completed. It is really an admirable manual of entomology by an author thoroughly versed in the science, and who is at the same time an original investigator. On this account it has a much higher character than a mere compilation, which is too seldom the case with scientific manuals and text books. The work is well illustrated, and followed by a

glossary and full index. The structure and classification of insects are fully discussed, as far as the families and the prominent genera and species in each family are described. It includes also full directions for collecting and preserving insects, and various other useful information on transformations, habits, distribution, geological history, etc., as well as descriptions of new species and genera in some of the orders. It embraces the *Arachnida* and *Myriapoda* as well as the Hexapod insects, which is quite unusual in treatises on entomology. V.

3. *Theory of Existence:* Part I. Devoted to the enunciation of the laws which determine the motions that result from the collision of ponderable bodies; by ELIAS DEXTER. 156 pp. 8vo, with 5 plates. 1869. New York. (Edward Dexter, 564 Broadway).—The laws enunciated in this singular book, beginning with the first paragraph defining velocity, are, with few exceptions, entirely at variance with the received principles of mechanics, so that in the concluding sections, which are devoted to the refutation of Newton's Laws of Motion, the author finds it easy to show by reference to the earlier part of his own book, that the views of Newton are quite erroneous. The age is prolific in such waste efforts by the "advance men" of the times.

4. *A Practical Treatise on Metallurgy adapted from the last London edition of Professor Kerl's Metallurgy.* Vol. iii; *Steel, Fuel, Supplement;* illustrated with 145 wood engravings; by WILLIAM CROOKES, F.R.S., and ERNST RÖHRIG, PH.D., M.E. 820 pp. 8vo. New York, 1870. (John Wiley & Son.)—This volume is a most valuable addition to our previous literature upon the important subjects of which it treats, and it is not too much to say that the metallurgical student will seek in vain, elsewhere, for such a comprehensive summary of recent progress in this department of knowledge. The arrangement of the materials is excellent and the subjects are discussed with all desirable fulness; thus the Bessemer steel process covers 80 pages; while proper notice is taken of all other methods, even to the Ellershausen process, Bessemer's new system of high pressure, hot blast furnaces, and Siemen's process of producing steel direct from the ores. The table of contents covers 21 pages, and exhibits clearly the great range of interesting topics which fills the volume. Full references to original memoirs in all languages bearing on steel and fuel add value to the work. This is altogether the most important contribution of the three volumes, of which it is the last as well as best.

5. *Lithology of the Seas of the Old World;* by M. DELESSE.—Of this work by M. Delesse we have seen only the chart. In the work the author, as is stated in a resumé received from him, takes up the lakes and seas of the old world in succession, describes their several features, or peculiarities, the character of the shores and bottom, and the relative amount of life distributed over the several parts. The subject is one of great interest and if thoroughly carried out, would make a very important contribution to the sciences of geology and physical geography. The map is an exceedingly beautiful example of engraving and coloring.

6. "*Nature.*"—This scientific weekly, published by MacMillan & Co., London,* the first number of which was noticed in our last volume (p. 451) sustains well the promises made in its prospectus. It is popular in the character of many of its articles, and well furnished with the scientific news of the day, besides reviews of new works. It is not in any proper sense a special organ of Darwinism, which the first number seemed to suggest. Illustrations are often given when the subjects require it. The number for December contains a map of a part of Africa, presenting a view of the recent discoveries and Livingstone's route, which would be convenient to readers of Livingstone's letter in the last number of this Journal, and that for Jan. 20th has a map of the polar regions, and the routes of the recent exploring parties in that direction.

7. *Paris Universal Exposition*, 1867; *Reports of the United States Commissioners.*—We have already made mention of some of the more important of these Reports, and have offered the titles of all which have appeared up to this time, and copies of which have reached us by the courtesy of the editor, Professor WM. P. BLAKE, who has carried these documents through the press in a manner creditable to the occasion.

Machinery and Processes of the Industrial Arts, and Apparatus of the Exact Sciences; by FREDERICK A. P. BARNARD, LL.D. pp. 669. Plates.

Examination of the Telegraphic Apparatus, and the Processes in Telegraphy; by SAMUEL F. B. MORSE, LL.D. pp. 166. Plates.

The Production of Iron and Steel in its Economic and Social Relations; by ABRAM S. HEWITT. pp. 183. Plates.

Report upon the Precious Metals: being statistical notices of the principal gold and silver-producing regions of the world, represented at the Paris Universal Exposition; by WILLIAM P. BLAKE. pp. 669.

The Progress and Condition of Industrial Chemistry; by J. LAWRENCE SMITH. pp. 146. Plates.

General Survey of the Exhibition, with a Report on the Character and Condition of the United States Section. pp. 324.

The Manufacture of Beet Sugar and Alcohol, and the Cultivation of Sugar Beet; by HENRY F. Q. D'ALIGNY. pp. 90. Plates.

Report on Corals; by SAMUEL B. RUGGLES and G. S. HAZARD. pp. 26.

Report upon Cotton; by E. R. MUDGE, with a Supplemental Report by B. F. NOURSE. pp. 115.

Report upon Buildings, Building Materials and Methods of Building; by JAMES H. BOWEN. pp. 96.

Report upon Wool and Manufacturers of Wool; by E. R. MUDGE, assisted by JOHN L. HAYES. pp. 143.

Preparation of Food. Pressed or Agglomerated food. Culture and Products of the Vine. Photographs and Photographic Apparatus. Outline of the History of the Atlantic Cable. School Houses and the Means of Promoting Popular Education.

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* The subscription price for "Nature" is fourpence; or for America, as announced by the house of Macmillan & Co., 63 Bleeker st., New York, 12 cents a number, and $5.00 a year.

Report on Silk and Silk Manufacturers; by Eliot C. Cowdin. pp. 51.

Report on Instruments and Apparatus of Medical Surgery and Hygiene; by Thomas W. Evans, M.D. pp. 70.

Report on the Fine Arts, and the Fine Arts applied to the Useful Arts; by Frank Leslie, S. F. B. Morse and Thomas W. Evans. pp. 43. Plates.

Report on Béton-Coignet: its fabrication and uses. Report on asphalt and bitumen, and their application to streets, roads, buildings, &c.; by Arthur Beckwith. pp. 21, and 31 plates.

Report upon Steam Engineering, as illustrated by the Paris Universal Exposition, 1867; by William S. Auchincloss. pp. 72.

Report on the Munitions of War; by Chas. B. Norton and W. J. Valentine. pp. 213.

In addition to the foregoing reports, there are yet to come others on the following subjects: *Mining; Clothing; Engineering Works; Education; The history of the Organization and Progress of the Exhibition;* including *titles, list of reports, authors, tables of weights and measures, etc.* The whole series will be bound in six volumes of some 600–700 pages each.

Proceedings American Philosophical Soc., Vol. XI.—p. 119, Prodromus of a study of the Fresh Water Algæ of Eastern North America; *H. C. Wood, Jr.*—p. 147, Seventh Contribution to the Herpetology of Tropical America; *E. D. Cope.*—p. 171, Synopsis of the Extinct Mammalia of the Cave Formations in the United States, with observations on some Myriapoda found in and near the same; *E. D. Cope.*—p. 195, Some Suggestions on the maintaining forces of Cosmical motion; *W. H. Lowrie.*—p. 202, Tidal Rain-fall; *P. E. Chase.*—p. 204, Abstract of Results of Measurements and Examinations of the Photographs of the Total Eclipse of Aug. 7, 1869; *A. Mayer.*—p. 209, Profile of Recent Flood in the Schuylkill; *J. C. Cresson.*—p. 213, Indian Relics from New Jersey; *G. B. Wood.*—p. 215, Comets and Meteors; *D. Kirkwood.*—p. 220, Search for a Normal Cause of the Recession of Cosmical Nodes; *W. H. Lowrie.*—226, Medical Activity of the Hemp Plant as grown in North America; *H. C. Wood, Jr.*—p. 233, Silver Coins; *W. E. DuBois.*—p. 235, Coal Borings in the Wilkesbarre Basin; *P. W. Sheafer.*—p. 237, Supposed Laurentian Fossil; *R. J. Roscoe.*—p. 237, Experiment on the Revival of Peach Trees; *G. B. Wood.*—p. 240, Second Addition to the History of the Fishes of the Cretaceous of the United States; *E. D. Cope.*

Proceedings Boston Soc. Nat. Hist., Vol. XIII.—p. 139, Molluscan Fauna of New Haven, part II, Acephala and Bryozoa; *G. H. Perkins.*—p. 167, Occurrence of the Remains of *Tarandus rangifer* Gray, at Big Bone Lick, Kentucky; *N. S. Shaler.*—p. 169, American Lepidoptera, II, Phalænidæ; *C. S. Minot.*—p. 172. Native Carbonate of Magnesia from California; *C. T. Jackson.*—p. 172, Remarks on the Relations of the Rocks in the vicinity of Boston; *N. S. Shaler.*—p. 178, Notes on the Mammals of Iowa; *J. A. Allen.*

Annals Lyc. Nat. Hist. of New York. Vol. IX, No. 8.—p. 237, Catalogue of Birds from Puna Island, Gulf of Guayaquil, in the Museum of the Smithsonian Institution, collected by J. F. Reeve; *G. N. Lawrence.* (Continued.)—p. 238, Additional Notes on the Geographical Distribution of Land Shells in the West Indies; *T. Bland.*—p. 242, Note on Lovén's Article on "Leskia mirabilis, Gray;" *A. Agassiz.*—p. 246, Observations on a Collection of Chalchihuitls from Central America; *E. G. Squier.*—p. 265, Characters of some New South American Birds, with Notes on other rare or little known Species; *G. N. Lawrence.*—p. 276, On the names applied to Pisidium, a genus of Corbiculadæ; *T. Prime.*—p. 280, List of the Species of Mollusca found in the vicinity of North Conway, New Hampshire; *T. Prime.*

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXIX.—*On a method of producing, by the Electric spark, figures similar to those of Lichtenberg;* by ELI W. BLAKE, Jr.

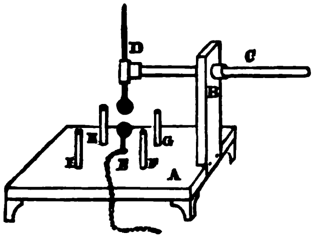
LICHTENBERG'S figures, discovered in 1777, are a result of the attraction of an electrified surface for light particles of electrified or unelectrified dust. Prof. Rood has shown that figures entirely similar in form are produced, when the spark is allowed to fall on the sensitized collodion film of an ordinary photographic plate, and the latent image is developed in the usual way.

The method I have to describe consists in throwing the discharge upon the surface of a fusible non-conducting body. If the body be near its fusing point the figure appears at once,—if cold, a latent image exists which may be "developed" by heat.

The non-conducting surface is prepared by coating a plate of metal with an even film of pitch. Pieces of sheet-tin, 3 inches square, coated with films of pitch of a thickness varying between 0·01 and 0·02 in., were used in most of my experiments. The pitch was the ordinary commercial article, freed from sand, fragments of bark, &c., by being melted and strained through a muslin bag. Shellac, rosin, Burgundy-pitch, bees-wax and Canada balsam were in turn tried as substitutes for pitch, but with unsatisfactory results.

A simple apparatus for holding the plate during the discharge, is represented in fig. 1. The upright B supports the insulating arm C,—a rod of glass, which may be turned in its bearing, but is prevented from moving longitudinally. The arm C holds the wire D. This wire slides up and down with considerable friction so as to retain any position given to it. It is graduated to tenths of an inch, and terminates, at one end, in a sharp point—at the other, in a metal ball ½ inch in diameter. Directly beneath D is a similar wire, E, passing through the board A, and held by friction. Around E, at the corners of a three-inch square, are disposed four insulating posts, upon which the prepared plate is laid.

1.



In experimenting, merely to obtain the figures, the arm C is turned so as to bring the wire D into a horizontal position,—the prepared plate is laid upon the insulating posts (for the + figure with the pitch side uppermost). The wire E is made to touch the plate below, and is put in connection with the earth. D is then made vertical, and is adjusted to the desired striking distance. An insulated connection being made between D and a charged prime conductor, the + spark passes over to the pitch. The − figure is obtained when the pitch side is beneath, D in contact with the plate, and E depressed to the proper striking distance. The discharge having taken place, D is again made horizontal, and the plate may be removed for development. This process consists in gradually warming the plate over a lamp. The metal side must be presented to the lamp, as the slightest touch of a flame on the excited pitch instantly dissipates the electricity. At a certain temperature, (in my experiments about 60° C.,) the figure will begin to appear, and in a few seconds the development is completed. The plate being now allowed to cool, the figure becomes permanent. If the plate be overheated the figure is destroyed. It may be instantly obliterated by exposure for a second to the naked flame, and the plate may then be used again. The proper temperature for development is some degrees below the real fusing point of the pitch used.

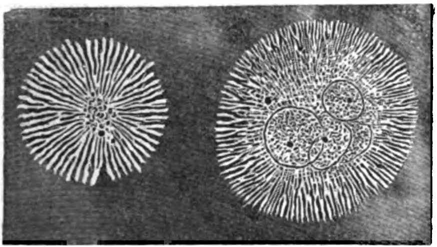
The figures, obtained as described, are formed by depressions and elevations of the excited surface. The depressions would appear to be the true figures, as they correspond exactly in form to those obtained by Lichtenberg. The plate may be dusted before development; the form thus revealed will be reproduced in depressions upon warming. The depth and sharpness of these depressions vary with the quantity of electricity, and the thickness of the film of pitch. The thinner the film, the sharper the lines. The exact depth of the depressions below the general level is very difficult to measure, owing to the elevations produced. In films of 0·015 in. thickness, the deepest lines are about 0·005.

When cold, the pitch is sufficiently hard to allow of several impressions being taken from it in printing ink. The wood-cuts given below were made from such impressions, transferred to a block, and then engraved. The white portions therefore represent depressions. The "ground," representing the level surface of the pitch, was lightened by ruling, so as to bring out the exterior ring of the negative figure.

Frictional Electricity.

Discharge from balls.—The positive spark produces the figure of a star (2 and 3). Sometimes the rays diverge almost from the center, but generally the central portion is broken up into a confusion of minute elevations, which in the cuts show as dots. The rings sometimes seen in this central portion resemble the negative figures, and suggest the idea of oscillations in the discharge.

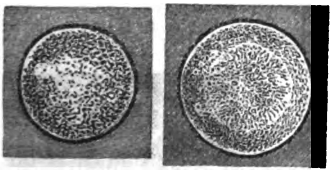
2. 3.



Positive spark from ⅓ inch balls. Striking distance, ⅓ inch.

The negative spark gives the figure shown in (4). An elevated ring forms the outer boundary; inside of this comes a deeply depressed circle surrounding a circular disc, whose surface is so irregular that I have not been able to determine whether it is, on the whole, above or below the general level of the pitch film. Perfectly similar figures were obtained by means of the electricity developed by the Holz machine, the electophorus and that accumulated in a Leyden jar.*

4. 5.



Negative spark from ¼ inch balls. Striking distance, ⅓ inch.

Discharge from Points.—When a single, instantaneous discharge, from a fine point, falls upon the film, figures similar to the foregoing, but not so regular, are obtained. If, however, the discharge be of negative electricity, and be continued for a short time, (e. g. during a quarter-revolution of a 20-inch plate of the ordinary frictional machine,) the first effect of developing is to bring out a *star*, which might readily be mistaken for the positive figure. Inspection shows however that the rays are not *depressed*, but *elevated*. The rays are generally more or less curved, and resemble the projection on a plane of the meridians of a hemisphere. The plane of projection is different in almost every figure. Precisely such a star occurs in the figure, given below, of the negative spark from the induction coil.

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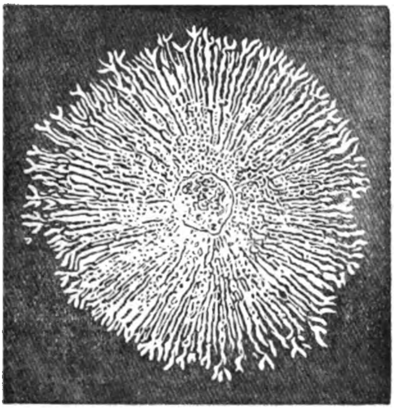
* In experimenting with a well charged Leyden jar, if the electrodes D and E (fig. 1) are connected with the coatings of the jar, the discharge is so violent as to perforate the film of pitch. Surrounding the minute perforation is a circular crack. Generally the circular fragment thus set free is thrown out, and in the center of the bright spot of tin exposed, a minute dot is seen. On examination by a magnifier, it will be seen that the tin is fused at this point. That this is not a thermo-electric action of the tin, and iron, is proved by the fact that the fusion takes place whether the dischage is positive or negative. In using such a jar for the production of figures the electrode E should be removed and the spark thrown on the insulated plate.

If the discharge from the point be continued for some seconds, the plate, on developing, shows an infinity of minute circular depressions with no characteristic distinction between +E and −E. In developing these plates, especially those charged with −E, vivid sparks may be seen to rise from the pitch, visible even in broad day-light.

Figures produced by the Induction Coil.

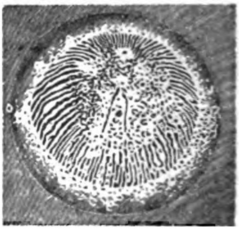
The coil used in these experiments was made at Ruhmkorff's establishment in Paris. It is capable of giving an 8-inch spark, but, by reducing the primary current, the striking distance was brought down to ½ inch. A single Bunsen's cell was used,—the carbon being withdrawn so as barely to touch the nitric acid.

6.



Positive spark from terminal wires of the induction coil.

7.



Negative spark from terminal wires of the induction coil.

The positive figure obtained is represented in the accompanying cut (6). Except its larger size, as compared with frictional electricity of the same striking distance, there is nothing noticeable in it.

The corresponding negative figure is seen in (7). The central star with curved rays, referred to above, is surrounded by a deeply depressed circle, which is bounded by a slightly elevated ring. The terminal wires evidently acted here as points, for the *star* was not obtained when the discharge took place from balls.

Simultaneous production of the Positive and Negative Figures.—For this, a plate coated with pitch on both sides is required. The electrodes D and E (fig. 1) are adjusted at equal distances from the upper and lower surface of the plate. Upon connecting D with the prime-conductor the positive spark falls upon the upper, while the negative spark leaps up to the lower surface of the plate. Development by the lamp, without obliteration of one of the figures, being impossible, the plate may be heated in an air-bath to about 60°–65° C. It is more convenient, however, to throw the discharge upon the plate when warm, the figures then appear at once. By using plates of glass, or mica, instead of metal, the relative size of the figures is at once seen. The negative figure is considerably less in diameter than the positive.* This fact explains why, when +E and −E are thrown on the same spot, they do not neutralize each other and the resulting figure is a combination of the two.

An interesting fact, in connection with this subject, is the length of time that may elapse between the reception of the spark by the plates, and the development of the figure. Several plates were charged in immediate succession, and developed, one by one, at intervals of two hours. The last, developed after twelve hours, showed hardly any loss in depth or sharpness, although the weather was damp and unfavorable. Of another series, the last, developed after *seven days*, still came out distinctly.

A charged plate may be breathed upon, and the condensed moisture be allowed to evaporate, several times, without apparently injuring the latent image. To test the discharging power of a point, the following experiment was made. One end of a copper wire was filed to a sharp point; the other end was then soldered to a metal plate. This plate had two intersecting lines scratched on its surface, and was coated with pitch. The spark having been made to fall on the spot marked out by the intersection of the lines, (visible through the pitch), the copper wire was bent over so that its point was directly over the center of the latent image and very near it. After 12 hours, this plate, on developing, gave a good figure.

From the manner in which the figures are produced, it would appear that they are due to the attractions and repulsions of the excited surface.[*] This seems proved beyond doubt by the identity of the Lichtenberg figures, with the depression figures produced on developing. No chemical change of the pitch could enable it to attract dust.

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* Riess has shown (Pogg. Ann., B. lxix), that the areas of the surfaces occupied by +E and −E of equal quantity, and developed under the same conditions, are as 7 : 1.

As the point of temperature at which developement begins is considerably below the true fusing point of the pitch, the *work* performed by the electricity is no inconsiderable quantity. Does the electricity disappear in performing this work? The fact that the depression of the surface stops at a certain point, while the attraction for the opposite E on the metal plate should be constantly growing stronger, seems to point to an affirmative answer. As pitch, however, is said to become a conductor when fused, it may be that the two electricities are gradually transmitted and neutralize each other. Experiments have been undertaken in the hope of obtaining a decisive answer to this question, but as yet with no result worthy of publication.

Cornell Univ., Ithaca, N. Y., Feb. 6th, 1870.

ART. XXX.—*On the Magnesium and Electric Lights, as applied to Photo-Micrography;* by Brevet Lt.-Col. J. J. WOODWARD, Assistant Surgeon, U. S. Army. Report to Brevet Major General J. K. Barnes, Surgeon General of the United States Army, dated Army Medical Museum, Microscopical Section, January 4, 1870.[†]

I HAVE the honor to inform you that, on the 25th of October last, I began to conduct in person a series of experiments, intended to devise means for escaping certain difficulties which had hitherto prevented the successful preparation of Photo-micrographs of specimens, selected from the valuable and daily increasing series of permanently mounted microscopic sections of normal and pathological tissues, which form so interesting a portion of the treasures of the Museum. In these experiments I used the sun as a source of illumination, and, following the process which I have described in full elsewhere,[‡] I had no difficulty in arranging a method, by the aid of which this class of objects could be photographed quite as successfully and readily as the diatoms and other test objects which had previously been so satisfactorily reproduced in this section of the Museum. I shall take occasion in the course of a few days to lay before you prints of some of the tissue-preparations thus reproduced. At present it is my desire to call your attention to certain important observations which I had the good fortune to make, while my experiments were in progress, and which it appears to me cannot fail to be of interest and service to all microscopists.

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[*] The laws of attraction and repulsion, for both positive and negative electricity being the same, it is not clear to what cause the difference of the figures is to be attributed.

[†] Communicated for this Journal by Lieut. Col. J. J. Woodward.

[‡] Circular No. 6, War Department, Surgeon General's Office, Nov. 1, 1865, page 148, *et. seq.*; this Journal, II, vol. xlii, Sept., 1866.

During the last week of October and the first two weeks of November, I relied wholly on the sun as the source of illumination for producing negatives. In this period, during which I had but two perfectly cloudless working days, and several fractional days on which my work was continually interrupted by passing clouds, I had ample opportunity to convince myself that the uncertainty of the weather was a most serious hindrance to the preparation of successful photographs of microscopic objects, and I ceased to wonder that European microscopists, who are exposed to a climate even more variable than our own, have not yet succeeded in placing the art of Photomicrography upon such a basis, as to make a convenient and habitual auxiliary in all microscopical investigations. This desirable end I believe I have attained; but it has been by resorting to artificial lights and thus making the success of the process wholly independent of the weather.

On the 12th of November I commenced a series of experiments with artificial lights which were most fortunately crowned with success, both the magnesium and the electric lights proving adequate sources of illumination for the production of Photo-micrographs even with the highest powers.

For the production of the electric light I used a Duboscq's lamp, set in motion by a battery of fifty small Groves' elements. I found that, with this source of light, photographs could be successfully taken with any power with which pictures can be taken by sunlight; and I was delighted to find, as I had anticipated, that the very exaggeration of light and shadow which has prevented the electric light from being generally adopted as a source of illumination in the preparation of photographs of the size of the object, or smaller, proved of immense advantage in the reproduction of the feeble microscopical images of highly magnified objects, and that the pictures were hence clearer and better defined than any photographs of similar objects I had hitherto seen produced by sunlight. I found also that the electric light was so much more manageable than sunlight as a source of microscopic illumination, that I could readily arrange it to produce negatives with much shorter exposures than are indispensable with the sun.

The magnesium light shared these qualities to a high degree, but I found that its best work was done when the object was not to be magnified more than a thousand diameters, and that there were certain limitations to its use on test objects which will be referred to in the sequel.

With one or the other of these artificial lights as a source of illumination, I have prepared a considerable number of negatives of interesting microscopical objects, of which a few are appended to this report by way of illustration, while the others will be laid before you in future reports on special subjects.

The magnesium and electric lights are mentioned as possible sources of illumination for the production of Photo-micrographs by Dr. Lionel Beale, in the 4th edition of his "How to Work with the Microscope," page 275. I am not aware, however, that any one has made successful negatives with high powers with either of these lights prior to the experiments here recorded. There are in the Museum a few photographs with low powers taken with the magnesium light by Dr. C. F. Crehore, of Boston, Mass., who kindly presented them August 3, 1866. Negative No. 90, old Microscopical Series, Army Medical Museum, represents a few villi from the small intestine of a mouse, photographed by the electric light with a $\frac{4}{10}$th objective of Wales arranged to magnify 84 diameters. The electric light was produced by forty Bunsen's cells, and as I had no electric lamp at the time, I held the carbon points in two retort holders and managed as best I could, during the exposure, the uncertain light thus produced. I know of no other Photo-micrographs than the above to have been actually made by the electric or the magnesium lights; certainly if any have been, they have not been sufficiently successful for their authors to be willing to give them any degree of publicity. I have no hesitation, therefore, in claiming for the Museum and for myself the credit of having demonstrated the serviceable character of these lights as sources of illumination for the preparation of negatives with high powers, and of having devised a simple method which brings their use within the reach of every microscopist.

I propose now to sketch briefly the process by which negatives of microscopic objects can be conveniently produced with these artificial lights.

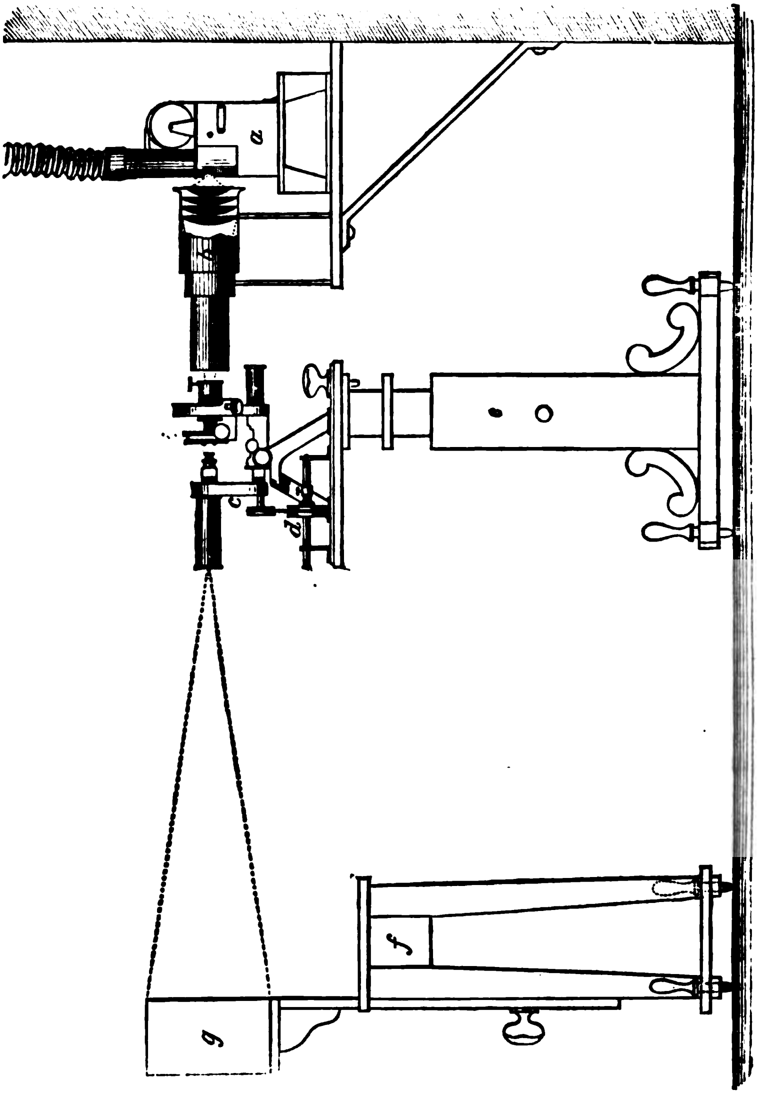
1. The electric light is by far the best of all artificial lights for the production of photo-micrographs, and, when used as I am now about to describe, it is both convenient and economical. I use a Grove's battery of fifty elements. The battery is placed just outside of the operating room in a closet from which the fumes escape through an earthen pipe into the main chimney of the building. This battery was furnished by Mr. William Ladd, Nos. 11 and 12, Beck street, Regent street, London, W. The rubber cups are 4¾ inches high, 3¼ wide and 2 thick. The platinums are 5½ inches by 2⅛, and weigh about 60 grains each. The zincs are bent on themselves so as to present a part of their surface on each side of the platinums, and weigh, when new, about a pound apiece. Mr. Ladd furnishes these batteries in

trays of ten elements, at five pounds sterling per tray, and I find that a battery of five trays is sufficient for most purposes. Seven pounds and a half of strong commercial nitric acid, and three of sulphuric, diluted with ten times the quantity of water, is sufficient to charge this battery, which will then produce the light continuously for from three to four hours. The cost of running the battery for this time, including in the estimate the amount of zinc consumed, and the cost of amalgamating every third or fourth time of using, is very moderate. I make it a practice to have the battery washed out, the acids thrown away and the porous cups put to soak immediately after I have done the day's work, and all this is so simple that I have no difficulty in instructing an orderly to do it, so that the management of the battery does not occupy any part of my time.

The Duboscq's lamp, the microscope and the plate holder are arranged in a dark room which enables me to dispense with the use of a camera. The general arrangement of the apparatus is shown in the cut.

The electric lamp of Duboscq (*a*) is placed on a stool against the wall at one end of the room, and its light concentrated by a pair of condensing lenses (*b*) on the lower lens of the achromatic condenser of the microscope. The microscope (*c*) (a large Powell and Lealand's stand) is placed on a small table (*e*) which is so arranged that it can be lowered or elevated at pleasure and can be levelled by means of three levelling screws at its base. The plate holder (*g*), also arranged so that it can be raised or lowered at pleasure, is supported by a small table (*f*) which stands on three levelling screws. The floor of the apartment is quite level. The lenses employed for the microscope are those of Mr. William Wales of Fort Lee, New Jersey, specially constructed for bringing the actinic rays to a focus. For powers above the $\frac{1}{8}$th, however, I have found that the achromatic objectives of Messrs. Powell and Lealand, of London, answer an excellent purpose, and indeed that their immersion $\frac{1}{16}$ exceeds in defining powers any objective which has as yet come under my notice.

In taking photographs with this apparatus, I proceed as follows: The electric lamp being set in motion, the table holding the microscope (which has previously been levelled), is raised or lowered and moved from side to side till the center of the achromatic condenser is brought to the center of the illuminating pencil proceeding from the lamp; the object is then placed on the stage and carefully adjusted. A cell of plate glass containing a saturated solution of the ammonio-sulphate of copper is fixed just below the achromatic condenser, and not only prevents the admission of non-actinic rays, but excludes the very great heat which accompanies the electric light, and also mode-rates its effect upon the eye of the observer. The light thus produced is very agreeable to the eye, and I find myself able to work with it from four to five hours without fatigue. It has also the advantage that all the colors of the object examined disappear, and the preparation appears black on an azure field which resembles the sky on a clear day, so that the observer



sees at a glance how the object will appear in the photograph (in which the same black lines or tints will be faithfully reproduced on a white field) and is thus enabled to arrange his achromatic condenser and other adjustments so as to produce the most satisfactory effect.

Every thing having been arranged at the microscope to the satisfaction of the observer, the eyepiece is taken out, and the image allowed to fall on the ground glass of the plate holder, which has previously been placed at the distance necessary to give the magnifying power desired with the objective employed. The operator adjusts the plate holder to the right height and sees that it is perpendicular to the optical axis of the microscope, which he readily does by observing that all parts of the field are equally in focus. He then takes out the ground glass and finishes the fine adjustment with a sheet of plate glass and a focussing glass, after which the sensitive plate is inserted, the exposure made and the operation is finished.

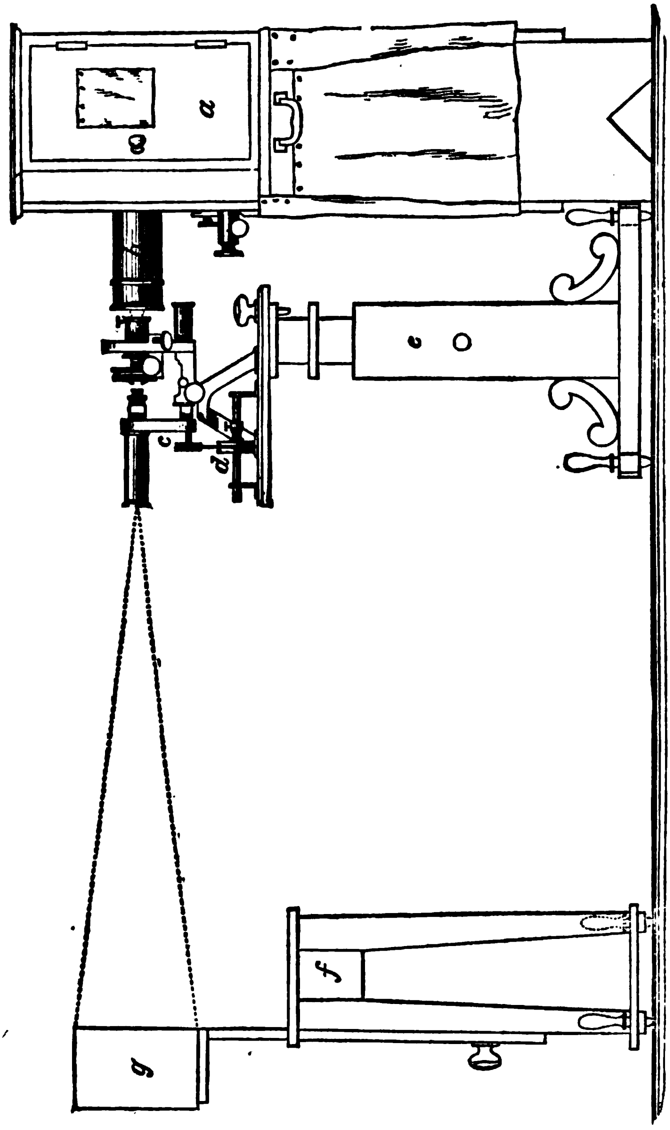
To enable the observer to focus the microscope while sitting at a distance from it at the sensitive plate, the following contrivance is employed. On the table which supports the microscope (*e*) two brass shoulders, each two inches high, are screwed. Through these runs an iron rod nine inches long, on which slips a brass pulley (*d*) which can be clamped at any point. A cord connects this pulley with the wheel of the fine adjustment of the microscope which is grooved for the purpose. It is evident that whenever this iron rod is turned, the pulley turning with it will move the fine adjustment of the microscope. To effect this the iron rod terminates in a square extremity, so that a joint of an ordinary fishing rod, to which a brass ferrule shaped like a watch key, has been rivetted, enables the operator to focus the microscope at anyordinary distance. When greater distances are required two joints of the rod may be used. The rod, being graduated into feet and inches, enables the operator to record the distance employed for each picture. When the focussing is completed, the rod is removed. I have found this simple and cheap arrangement superior in delicacy and convenience to any of the more costly arrangements, I have heretofore tried.

The chemical processes, employed in taking the negatives, do not differ in any respect from those used in ordinary photographic work, and I have found that by employing a practical photographer, allowing him to manage the dark room and confining my whole attention to the optical arrangements, I not only get many times more pictures in a day, but they are much better than can be produced by any one who attempts to do the photographic work, as well as manage the microscope himself.

I find myself thus enabled to sit down quietly of an evening, and during four hours work to produce from twelve to thirty negatives or more, in accordance with the difficulty of the subjects and my previous knowledge of them. Any microscopist who is willing to go to the moderate expense of battery and lamp, and to add two or three specially constructed objectives to his microscopical apparatus, can, by employing a photographer one or two evenings in the month, reproduce all the more interesting of his month's observations with a degree of economy and beauty not to be obtained by any other means, and if he follows the method I have above described, the character of his results will be conditioned by his skill as a microscopist rather than by any other circumstance. As to the time of exposure required for taking negatives with the electric light, I find that for one thousand diameters about thirty seconds is necessary for that class of objects, (such as Angulatum, the Nobert's plate, &c.,) for which it is not necessary to employ a ground glass plate to prevent interference phenomena. In photographing the soft tissues and many other objects, it is necessary to insert a piece of ground glass below the achromatic condenser to escape the interference phenomena which else occur, precisely as must be done in photographing the same objects by sunlight. This increases the time of exposure to about three minutes for one thousand diameters. Other powers require poportional times.

2. The magnesium light affords a beautiful source of illumination comparable to white cloud illumination of the best character, or to the light of the sun after it has passed through a sheet of ground glass. Without the use of ground glass, this light serves admirably for the production of photographs of the soft tissues with any power under a thousand diameters. The light being composed of a mixed pencil, with rays passing in all directions, there are no interference phenomena, but for the same reason, on the Nobert's plate and many test objects, the results are inferior to those produced by the sun or by the electric light; with powers much higher than a thousand diameters, however, the time of exposure becomes inconveniently long.

The process employed by me in the production of negatives with the magnesium light, is essentially the same as I have above described for the electric light, simply the magnesium lamp is substituted for the electric, and the condenser of an ordinary oxy-calcium magic lantern is made to concentrate the light on the achromatic condenser of the microscope. The cut represents the arrangement. The magnesium lamp (*a*) stands on a shelf fastened against the wall. The condenser (*b*) concentrates the light on the lower lens of the achromatic condenser of the microscope, (*c*) which stands on a table (*e*) supported on three levelling screws. The image received on the plate holder, (*g*) which is supported on a table, (*f*) is photographed precisely as in the case of the electric light as above described. The same focussing apparatus (*d*) is employed and the ammonio-



sulphate cell should invariably be inserted, but the ground glass is never necessary. I find that it requires exposures of about three minutes to produce negatives of tissue-preparations with five hundred diameters. Other powers require proportionate exposures.

The magnesium lamp used by me for this purpose was the two-ribbed lamp of the American Magnesium Company, (No. 2 Liberty Square, Boston, Mass.,) sold by that company for magic lantern purposes, price $50. The ribbon weighs about 52 centigrammes per metre, and is sold at $2.50 per ounce. Two ounces will, with care, answer for three or four hours constant work, and ought to produce from twelve to thirty negatives in accordance with the difficulties of the subjects to be represented. The fumes of magnesia resulting from the combustion are carried into a chimney five feet long, made of a spiral wire covered with muslin, which terminates in a muslin bag in which the oxyd condenses, while the draft goes on through the interstices of the muslin. The chimney and bag are furnished by the company for $2.50.

In commenting on the above processes it may be remarked that, for the anatomist and physiological investigator, the Magnesium lamp affords a satisfactory and sufficient source of light for the photography of normal and pathological tissue-preparations. The same end can be equally well or even better attained with the electric lamp, with which also the most difficult test objects can be satisfactorily reproduced. Where economy of apparatus is the object, the magnesium lamp will be preferred by ordinary workers; but where much work is to be done, the high price of the magnesium ribbon more than counterbalances the cheapness of the apparatus, and the electric light becomes the most economical. For the information of any practical photographers who may be employed for work of this character, I may add the following remarks on the chemical process employed in the production of the negatives from which the appended prints were made. An ammonium and potassium portrait collodion, rich in alcohol, was employed, developed with the ordinary solution of iron, and fixed with cyanid of potassium. Where it was necessary to intensify, the hydrosulphuret of ammonium was resorted to.

In illustration of the character of these sources of illumination as compared with each other and with sunlight, I herewith append three prints from negatives, taken with a Wales' inch and a half, from the 6th square of a Möller's diatom type-plate, specially prepared for the Army Medical Museum bv that skillful microscopist. The first from Negative 79 (new series), was taken by sunlight, with 40 diameters; in the second, from Negative 123 (new series), the magnesium light was used, and every thing else remaining the same, the distance was increased so as to give 48 diameters; in the third, Negative 158 (new series), the electric lamp was employed, and every thing else still remaining unaltered, the distance was increased so as to give 66 diameters. It will be understood at once, that on account of the increase of distance, the second picture would have been slightly less sharp than the first, and the third than the second, had precisely the same source of light been employed; nevertheless, in spite of this disadvantage, to which they were purposely exposed, the magnesium and electric pictures are far superior to that taken by sunlight, and of the two the electric is much the best. It is especially to be observed, that in the electric picture the contrast obtained is so great that the objects appear clearly defined on an almost perfectly white ground, which is never the case with photo-micrographs taken with the sun as a source of illumination.

As a further illustration of the capabilities of the magnesium and electric lights, I add a few photographs taken by each.

By the Magnesium Light.

Arachnoidiscus Ehrenbergii. Magnified 400 diameters, by Wales' $\frac{1}{8}$th. Negative 114 (new series.)

Small vein and capillaries, from the muscular coat of the urinary bladder of the frog. Magnified 400 diameters, by Wales' $\frac{1}{8}$th. Negative 103 (new series). This negative is taken from preparation No. 3378, Microscopical Series, in which the bladder was injected with a half per cent solution of nitrate of silver, and subsequently stained with carmine dissolved in borax. The epithelium was then brushed off with a camel's hair pencil, and the preparation transferred through absolute alcohol to Canada balsam: the photograph reproduces every thing but the color.

By the Electric Light.

Pleurostaurum acutum. Magnified 340 diameters, by Wales' $\frac{1}{8}$th. Negative 109 (new series.)

Triceratium favus. Magnified 340 diameters, by Wales' $\frac{1}{8}$th. Negative 110 (new series).

Navicula spima. Magnified 840 diameters, by Powell and Lealand's immersion $\frac{1}{16}$th. Negative 112 (new series).

Human red blood corpuscles. Magnified 1,000 diameters, by Powell and Lealand's immersion $\frac{1}{16}$th. Negative 145 (new series).

Section of an epithelial cancer of the larynx. Magnified 400 diameters, by Wales' $\frac{1}{8}$th. Negative 162 (new series). This negative is taken from preparation No. 2277, Microscopical Section. The print shows the nuclei and cells of the growth with great distinctness.

Grammatophora marina. Magnified 2,500 diameters, by Powell and Lealand's immersion $\frac{1}{16}$th. Negative 151 (new series).

[The copies of the photographs here referred to, sent us by Dr. Woodward, surpass in perfection and beauty any specimens of photo-micrography we have seen.—Eds.]

ART. XXXI.—*On a Mechanical Finger for the Microscope;* by J. H. B. L.

IN the Journal of May, 1866, there is a description and woodcut of a mechanical finger, by Mr. H. L. Smith.

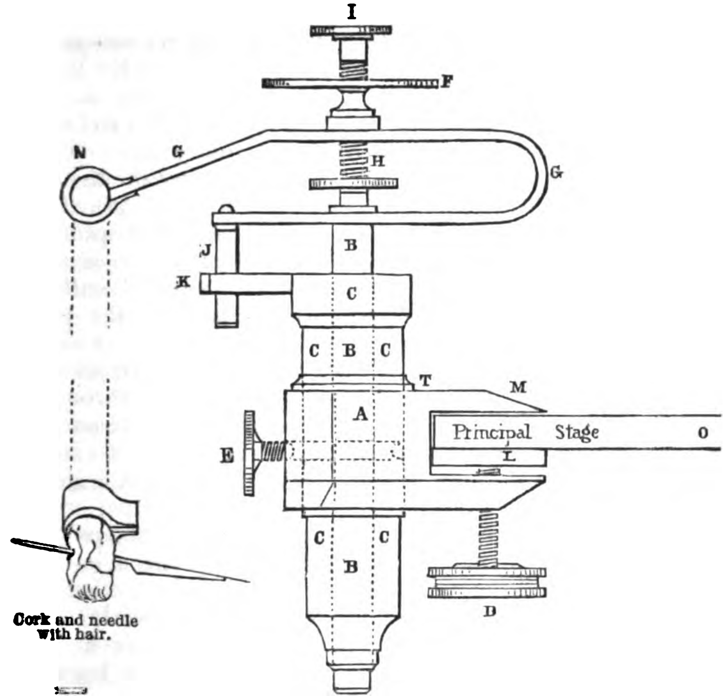
There are few naked eyes, or ordinary hands, that can select, from a mass, one of the smaller diatoms; and the engraving in the Journal was seized upon, at once, by the writer, as affording a promise of relief in the patient labor that had so often tested both his eye and hand. It was his good fortune to be within reach of one of the instruments. It was a great help, no doubt; and, after acquiring "the knack," it was possible to use it. But it wanted solidity, and the writer ventured to think that it might be made firmer, if constructed with fewer parts and joints. It was a capital idea, however, usefully illustrated; but not beyond improvement: so an improvement, as it was thought, was put on paper, and sent to Mr. Joseph Zentmayer, the well known optican of Philadelphia.

Putting aside both the improvement and the original, Mr. Zentmayer went to work upon an entirely new system, and produced what seems to be very near perfection.

The microscope, in the writer's possession, is one of Mr. Zentmayer's large first class ones, though the finger can be adapted to any other. There are three pieces: one, an independent stage, that we will call the diatom stage, (fig. 1) supported, above the principal stage, upon a tube that fits into a sleeve attached to a cylinder, (fig. 2), that fits into the sub-stage. The tube of the diatom stage is passed through the opening of the principal stage into the sleeve, as shown in the drawing, when its only movement, up and down, is regulated by the rack and pinion of the sub-stage. Light from the reflector is thrown upon the object through the tube of the diatom stage. A spring, S, with an ivory button, B, is attached to the diatom stage, as shown in fig. 4, which steadies the slide as it is moved by hand to bring different parts of it into the field.

Fig. 3 shows the third piece of the apparatus, or, really, the only piece, so far as the finger is concerned; the other three pieces being necessary to hold the slide containing the diatoms, but having, otherwise, nothing to do with the finger proper. The drawing is of full size. A is a clamp secured to the principal stage by the jaws M and the movable plate L, which is tightened by the set screw D. The cylinder C passes through the clamp resting on the shoulder T. It turns horizontally when not fixed by the set screw F, whose point presses in the groove shown in the drawing. The steel rod B, surrounded by a spiral spring, which is not shown, but which can be readily

Fig. 3.



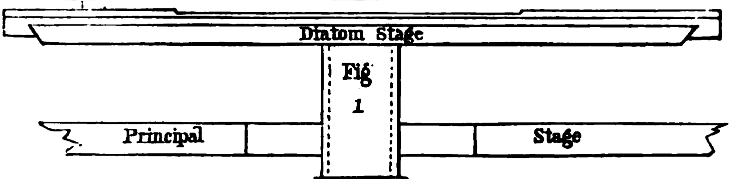


Fig 1

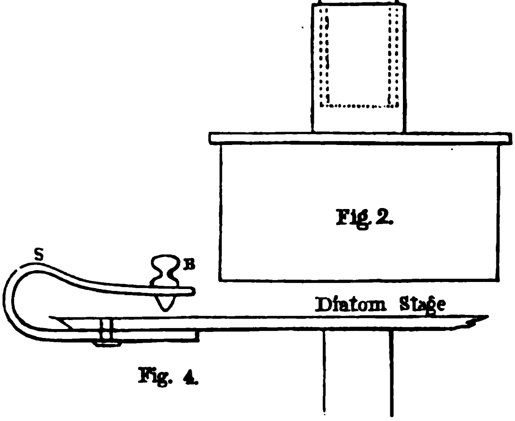


Fig. 2.

Fig. 4.

understood, passes through the cylinder C, the spiral holding it up when not pressed down. Upon this rod is the steel spring, G, bent as shown, carrying at its upper and longer extremity, at N, a cork-holder, through which is thrust the needle that carries the hair; and, at its lower extremity, the guide J, passing through a slot on the arm K, projecting from the cylinder C. The object of this is to counteract the rotary movement that would otherwise be caused by the spiral spring on the rod B when pressed downward in operating the apparatus. F is a large milled head lowering or raising the end of the spring N, in focussing the point of the hair. The down pressure is applied at I compressing the spiral spring around the bar B within the cylinder C, and bringing the point of the hair on the particular diatom.

A most excellent, if not an original, mode of attaching the hair to the needle, which is thrust, eye foremost, through the cork projecting from N, is the suggestion of a friend of the writer. Cut a piece of thin paper into the shape of the letter V about half an inch high and crease it lengthwise so as to bring the sides together. Having gummed the paper well, lay the needle on the crease, keeping its point within the paper, and place the hair along side of it. Then closing the sides of the V, the needle and the hair will be compressed together at the bottom of the crease; and when the paper is perfectly dry the hair can be cut with scissors to the proper length, if necessary, and so much of the paper trimmed off as may not be wanted to retain the needle and hair in their places. The needle may then be forced, with a pair of fine pliers, eye foremost, through the projecting end of the cork at an angle inclining somewhat downward; and, when this is done, the finger is ready for use. The angle of the needle to the stage may be changed by turning the cork, which is screwed into the opening at N, which has a thread for the purpose.

The slide containing the diatoms is now placed on the diatom stage; the part to be examined being over the opening of the tube, through which the light is reflected, focussed as usual, and the particular diatom selected. The hair is then brought, by hand, within the field of view. This is done by turning the cylinder C, and adjusting with the pliers, if need be, the length of needle and hair projecting through the cork. The set screw E is then tightened, and the observer has, in the field, the diatom in focus, and the end of the hair seen dimly, and not in focus. The screws of the principal or mechanical stage now enable him to bring the point of the hair with mathematical precision just above the diatom; when, pressing at I gently downward, the hair touches the diatom; the latter, if all goes well, adheres to it; the diatom stage is lowered; the slide removed; the clean slide, to which the diatom is to be transferred is put in its place; the diatom stage raised until the hair, with the diatom on it, is in contact, when the diatom is taken off by the moisture that has been previously breathed upon the slide.

The superiority of the contrivance here described is its simplicity and absolute steadiness.

In the drawing, the position of the spring G with the cork-holder N is reversed. They turn, as already described, in any direction, horizontally.

The facility with which a diatom may "be handled," to use the term in this connection, is one of the great advantages of Mr. Zentmayer's contrivance. The point of the hair may be brought into focus along side of the diatom, which may then, by using the screws of the mechanical stage, be pushed in any direction by the finger, and separated from the mass, or, when transferred to a clean slide, may be placed wherever required thereon. With such an instrument may be understood the *modus operandi* by which the 392 diatoms of "Möller's Diatomaceen platte" were arranged.

Nothing more has been attempted here, than to describe the particular instrument. But no one can understand the whole subject, without reading the most admirable article of Mr. H. L. Smith already referred to, and which set the writer to work to improve, if possible, the mechanism there described. L.

ART. XXXII.—*The Combinations of Silicon with Alcoholic Radicals*; by C. FRIEDEL and J. M. CRAFTS.*

IN a previous research† we studied the ethers of silicic acid, and discovered a number of new bodies, whose structure leads to the conclusion that the atomic weight of silicon is 28, and that the formula of silicic acid is SiO_2. Gaudin, and afterwards Odling, first gave silicon its true atomic weight, and silicic acid its rational formula, in order to bring them into accordance with the well known law establishing the most simple relation between the vapor density of bodies and their atomic weight, and their views have been adopted by many chemists; but hitherto conclusive proofs, based upon chemical grounds, have been wanting to establish completely the correctness of their theory, and a large number of the best authorities in chemistry have adhered to the old formula, $SiO_3(O=8)$ for silicic acid.

Our research was at first undertaken with a view to proving that the chemical properties of silicates can only be explained by adopting the new formula, and we have succeeded in obtaining bodies, whose existence and mode of formation it is impossible to account for by any other theory. Such are the chlorhydrids and acetins derived from normal silicic ether, $Si(C_2H_5O)_4$ and at first we only studied the compounds which belong to the same type as the normal silicic ether, but the research led us further than we anticipated, and resulted in the discovery of the more complicated disilicic ethers, already described, whose structure throws some light on the rational formulas of mineral silicates, and also of a remarkable class of bodies, in which the alcoholic radicals, ethyl, C_2H_5, and methyl, CH_3, are combined directly with the silicon, and not, as in the ethers, through the medium of oxygen. The present paper is devoted to the description of these latter bodies.

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* The chemical symbols used have the values which belong to them in the new system.

† This Journal, II, xliii, pp. 153 and 331; Ann. de Chim. et Phys., IV, ix, p. 5.

The study of the compounds of silicon with alcoholic radicals fortifies the conclusions already arrived at; it demonstrates the tetratomicity of silicon, and places it in the same group with tin, titanium and carbon; and it leads, besides, to the discovery of a property of silicon, which allies that element with carbon far more closely than the equality of their atomicity and the similarities hitherto observed in the structure of their compounds. In fact, silicon has been found to possess the property of combining directly with carbon, or rather with hydrocarbons; and the resulting compounds are in every respect similar to simple hydrocarbons, susceptible like them of substitution of chlorine and bromine for hydrogen, and of acting as radicals in alcohols and ethers; consequently silicon may take the place of carbon in a hydrocarbon, and in the series of bodies which can be derived from a hydrocarbon, without modifying essentially its properties.

It is easy to appreciate the importance of this result. Carbon is characterized by the property of combining with itself to build up groups of atoms, which have been compared to chains, or nuclei, about which the atoms of hydrogen, oxygen, nitrogen, &c., found in organic bodies, group themselves, and it is especially this property of carbon which fits it to play the part of the element essential to the structure of organic compounds.

It has been supposed that carbon alone had the property of combining with itself to form the nuclei of organic compounds, but it now appears that silicon shares with it this quality, and we are led to the opinion, that no element is unique in its properties, but that each has its near relatives among the others, as was indicated by Dumas in dividing the elements into natural families, and as every new discovery daily tends to prove. It is remarkable, also, that analogies of this kind, which are independent in their nature of the atomicities of the elements, should occur especially between elements having the same atomicity, and the fact enhances the value of a classification of the elements, which is founded upon the consideration of their atomicities.

SILICIC ETHYD.

We have obtained silicic ethyd by the reaction which is often employed to effect the union of ethyl with metals or with nonmetalic elements. Chlorid of silicon and zinc-ethyd do not act upon each other at the ordinary temperature, but when they are mixed in the proportion requisite to afford an equivalent of zinc for each atom of chlorine, and heated in sealed tubes to 140° centigrade, a reaction commences, and at 160° it is completed in 3 hours. On opening the tubes, a considerable quantity of a gaseous hydrocarbon escapes, which, when lighted, burns with a luminous flame. The tubes contain a liquid, together with a solid deposit of chlorid of zinc, mixed with gray particles of metallic zinc. The presence of metallic zinc accounts for the production of gaseous hydrocarbons.

The liquid can be separated by distillation into several products. It commences to boil at 40°, and at that temperature chlorid of silicon distils, mixed with a very volatile hydrocarbon, burning with a luminous flame, which cannot be condensed alone at 0°, but which is held in solution, to a considerable extent, by the chlorid of silicon. When the temperature reaches 60°, nearly pure chlorid of silicon distils, and it then rises rapidly to 150°, and the greater part of the liquid distils at 150° to 155°.

The portion which distils at 60° to 150°, treated with water, gives the products of decomposition of the chlorid of silicon, together with a certain quantity of a liquid, identical in its properties with that which distils at 150° to 155° in the first operation. The portion thus obtained in both operations, distilling at 150° to 155°, washed with water and with a solution of caustic potash, to free it from the small quantities of chlorid of silicon which can not be easily separated by distillation, and then dried over solid caustic potash, distils at 152° to 154°. The body thus obtained is not spontaneously inflammable, like many of the compounds of organic radicals with metals, but when ignited it burns in the air with a luminous flame, and gives off a white smoke of silicic acid. It is lighter than water, and is not attacked by caustic potash nor by ordinary nitric acid. It is also not attacked by sulphuric acid, and is insoluble in it; concentrated sulphuric acid, however, separates from it a small quantity of a body which will be described later. It can be completely purified by shaking it several times with concentrated sulphuric acid, and decanting by means of a pipette; finally by washing with water and by drying over melted chlorid of calcium. The product, which was finally obtained by operating in this way on considerable quantities of material, boiled at 152°.

The method of purification with sulphuric acid was not at first adopted, and the analyses made of the substance boiling at about 152°, after it had been simply treated with water and with caustic potash, in order to remove traces of chlorid of silicon, gave a slight excess of carbon for the first portions which distilled; see analysis I. This was undoubtedly due to a minute quantity of a hydrocarbon, probably ethylene, dissolved in the liquid.

The analyses of the portions which distilled last showed on the other hand a deficiency of carbon, arising from the presence of a body containing oxygen, which can be separated by means of concentrated sulphuric acid; see analyses II and III. We noticed, also, that the liquid was at first capable of absorbing a very small quantity of bromine, without being colored by it. After the treatment with sulphuric acid, the boiling point of the liquid was almost constant, and the smallest trace of bromine imparted its color to it, showing that no combinations took place.

Analysis of the liquids, which had not been treated with sulphuric acid.

I. *Boiling point*=$151°\text{–}151\tfrac{1}{2}°$.
Substance taken=0·1863 *grammes;* CO_2=0·4520 *grammes;* H_2O=0·2280 *grms.*

II. *Boiling point*=$151\tfrac{1}{2}°\text{–}153°$.
Substance taken=0·1743 *grms.;* CO_2=0·4127 *grms.;* H_2O= 0·2170 *grms.*

III. *Same product redistilled.* Boiling point=$151\tfrac{1}{2}°\text{–}152\tfrac{1}{2}°$.
Substance taken=0·1943 *grms.;* CO_2=0·4685 *grms.;* H_2O= 0·2390 *grms.*

	I.	II.	III.	Calculated for $Si(C_2H_5)_4$
C =	67·13	65·93	65·77	66·67
H =	13·60	13·83	13·67	13·89

Analyses of silicic ethyd purified by a treatment with sulphuric acid. Boiling point=152°–153°.

I. *Sabstance taken*=0·1931 *grms.;* CO_2= 0·4705 *grms.;* H_2O= 0·2388 *grms.*

II. *Substance taken*=0·2192 *grms.;* CO_2=0·5358 *grms.;* H_2O= 0·2720 *grms.*

III. *Substance taken*=0·3015 *grms.;* SiO_2=0·1250 *grms.*

IV. *Substance taken*=0·3430 *grms.;* SiO_2=0·1395 *grms.*

	I.	II.	III.	IV.	$Si(C_2H_5)_4$.
C=	66·45	66·66	....	....	66·67
H=	13·74	13·79	....	....	13·89
Si=	....	....	19·34	18·98	19·43

The first determination of silicon was made by heating the substance in a sealed tube with nitric acid for several hours, at 180°–190°, dissolving the contents of the tube in caustic potash, and estimating the silicic acid in the solution in the ordinary way. In the second determination, dilute chlorhydric acid and chlorate of potassium were substituted for the nitric acid, and the operation was carried on in a sealed tube as before. It is somewhat difficult to dissolve all the silicic acid which adheres to the tube by caustic potash.

The vapor-density of silicic ethyd was deduced from the following data:

Difference of weights of the bulb - -	=0·5248 *grms.*
Temperature of the balance, - -	14°
Temperature of the oil-bath, - -	214°·2
Barometer, - - - - - -	761·4 *mm.*
Capacity of the bulb, - - - -	211·0 *cc.*
Air remaining in the bulb, - -	1·3 *cc.*

Vapor-density=5·141.

Calculated vapor-density, 4·986.

The results thus obtained show that silicic ethyd corresponds to the chlorid of silicon, and that the reaction by which it is formed may be expressed by the equation:

$$SiCl_4+2Zn(C_2H_5)_2= Si(C_2H_5)_4+ 2ZnCl_2.$$

Silicic ethyd is a liquid resembling bodies of the petroleum class in its aspect. It has an odor like the pure hydrocarbons of this series, and cannot be easily distinguished from them, except that instead of burning with a carbon-smoke, it gives a white smoke of silicic acid.

The density of the liquid at 22°·7 compared with that of water at the same temperature, is 0·7657.

In order to obtain considerable quantities of silicic ethyd, we used Frankland's copper digester instead of glass tubes, which add to the inconvenience resulting from their small capacity, that of being liable to explode from the pressure of the gases formed as secondary products of the reaction.

In the digester, 100 grms. of zinc-ethyd were operated upon at once, and an excess of chlorid of silicon was always added. The first portions, which distilled below 140°, were a mixture of chlorid of silicon with silicic ethyd, and they were always treated over again with a fresh quantity of zinc-ethyd in the next operation.

We usually heated the digester in an oil-bath, at 180°–200°, using a gas-regulator of similar construction to Bunsen's, and kept each charge at that temperature for about ten hours. After opening the digester and distilling up to 140°, water was added and the distillation continued as long as any silicic ethyd continued to pass over with the vapor; the apparatus was then cleaned and a new operation commenced. As an example of a number of such operations, we may cite one series in which 769 grms. of zinc-ethyd and 480 grms. of chlorid of silicon were employed. We obtained 225 grms. of perfectly pure silicic-ethyd—about half the theoretical quantity.

In order to understand the phase of the reaction, which results in the formation of gaseous products, and of metallic zinc, we examined the gases, which were given off on opening the digester, by passing them through bromine, freeing the gas, which was not absorbed, from the excess of bromine, and collecting it over mercury. The quantity evolved in an operation is very large, and there was no difficulty in using the first portions of the gas to remove the air from the apparatus containing bromine, and in collecting at the end a perfectly pure sample for analysis.

The analysis of the gas, which was not absorbed by bromine, gave:

Volume of the gas	= 5·17
Oxygen added	27·72
Contraction	13·10
Carbonic acid	10·26
Remaining oxygen determined by detonation with H	9·50

Consequently two volumes of the gas contained 6·12 volumes of H, and 1·98 volumes of carbon vapor.

A second analysis gave:

Volume of gas	= 6·66
Oxygen added	26·61
Contraction	17·30
Carbonic acid	13·02
Remaining oxygen	2·94

Two volumes of the gas contained 6·36 volumes of H, and 1·96 volumes of carbon vapor.

The gas was the hydrid of ethyl C_2H_6, two volumes of which contain six volumes of hydrogen and two volumes of carbon vapor.

The bromid which was obtained by the absorption of a part of the gas by bromine, after having been washed with a solution of caustic potash, dried and distilled at 132°–134°, was analyzed with the following result:

I. *Substance* =0·3695 *grms.*; *AgBr*=0·7315 *grms.*

II. *Substance*=0·2712 *grms.*; CO_2=0·1330 *grms.*; H_2O=0·0608 *grms.*

III. *Substance*=0·3259 *grms.*; CO_2=0·1600 *grms.*; H_2O=0·0690 *grms.*

	I.	II.	III.	Calculated for $C_2H_4Br_2$
C	....	13·35	13·42	12·76
H	....	2·47	2·35	2·12
Br	84·43	....	....	85·11

The body is consequently the bromid of ethylene, and the gases, which are formed during the preparation of silicic ethyd, are simply those which are known to arise from the decomposition by means of heat of zinc-ethyd.

The question suggests itself: are intermediate products, arising from an incomplete replacement of chlorine by ethyl in the chlorid of silicon, also formed? Such products should have the composition: $Si(C_2H_5)_3Cl$; $Si(C_2H_5)_2Cl_2$; $Si(C_2H_5)Cl_3$, and should boil at points intermediate between 152°, the boiling point of silicic ethyd, and 42°, the boiling point of chlorid of silicon. The question must be answered in the negative; for after a large number of fractionated distillations of the product obtained in an operation, where a considerable excess of chlorid of silicon was used, it could be separated almost completely into chlorid of silicon and silicic ethyd, and no bodies appeared with fixed boiling points between 42° and 152°. The small quantity of liquid, which was eventually obtained, distilling between these points, was treated with caustic potash, and gave no more of a compound containing oxygen than was ordinarily obtained in every preparation of silicic ethyd, and whose mode of formation is described below.

We failed also to obtain intermediate compounds on heating chlorid of silicon and silicic ethyd together in sealed tubes for 15 hours, at a temperature of 240°. The product could easily be separated by repeated distillation into the two bodies which were originally mixed.

It appeared probable that silicic ethyd, and perhaps intermediate bodies, might be formed by a process analagous to that employed by Frankland and Duppa* to obtain boric-ethyd, $B(C_2H_5)_3$, namely: by the action of zinc-ethyd upon normal silicic ether. We found, however, that the two bodies do not act upon each other, even at a very high temperature; indeed the tenacity with which silicon retains its hold upon oxygen renders this result not surprising.

A most interesting phase of the reaction between zinc-ethyd and chlorid of silicon, is the one which gives rise to the formation of the body containing oxygen, which has been noticed above. As has been already stated, concentrated sulphuric acids frees the crude silicic ethyd from a small quantity of a foreign body, which is soluble in the acid. This body separates from the solution when the sulphuric acid is diluted with water, and floats upon the surface of the solution. It burns with a siliceous smoke, has a disagreeable odor until it has been purified by repeated distillations, and boils at about 235°. The product, which was purified as far as possible by a number of distillations, was analyzed.

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* Annales de Chimie et Pharmacie, cxv, 319.

I. *Substance*=0·1861 *grms.; CO_2*=0·4090 *grms.; H_2O*=0·2068 *grms.*

II. *The product of another operation, whose boiling point was* 229°–235°. *Substance*=0·2535 *grms.; CO_2*=0·5363 *grms.; H_2O*= 0·2855.

I.	II.	Calculated for $O\left\{\begin{matrix}Si(C_2H_5)_3\\Si(C_2H_5)_3\end{matrix}\right.$
C=59·94	57·71	58·54
H=12·34	12·51	12·19

Only a small quantity of this body is produced in the preparation of considerable quantities of silicic ethyd, and we have not succeeded in isolating it in a perfectly pure state during that preparation; its composition is, however, not doubtful, and we were able to recognize its identity with the same oxyd, which can be easily obtained and purified by a method described below. This oxyd must be derived from the oxyd of zinc, which is formed by the action of the air upon zinc-ethyd while charging the digester. Friedel and Ladenburg* have found that free and combined oxygen can be substituted for a part of the chlorine in chlorid of silicon, with formation of the oxychlorid, $O\left\{\begin{matrix}SiCl_3\\SiCl_3\end{matrix}\right.$. This oxychlorid is probably formed in the digester from the oxyd of zinc, and acts upon the zinc-ethyd to form the oxyd $O\left\{\begin{matrix}Si(C_2H_5)_3\\Si(C_2H_5)_3\end{matrix}\right.$, whose analysis is given above.

This oxyd may be considered as the ether of the radical silicic-triethyd $O\left\{\begin{matrix}Si(Et)_3\\Si(Et)_3\end{matrix}\right.$, and as analogous with the simplest ether of the ordinary series, $O\left\{\begin{matrix}CH_3\\CH_3\end{matrix}\right.$, silicon replacing the carbon, and ethyl the hydrogen. It is more difficult to decompose than the ordinary ethers, and resists the action of many of the agents which remove radicals from common ether; one of us, however, has succeeded in obtaining a double decomposition by means of the chlorid of acetyl; but the study of the products of the reaction is not yet completed.

ACTION OF BROMINE ON SILICIC ETHYD.

Nothing distinguishes silicic ethyd more completely from the compounds hitherto obtained by the action of the chlorids of the elements on zinc-ethyd, than the manner in which it acts with bromine and chlorine; thus far the only reaction known for such bodies is that in which the union between the alcoholic radical and the other element is severed, and the bromine or chlorine takes the place of the alcoholic radical.

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* Comptes Rendus de l'Académie des Sciences, lxvi, 539.

The researches for instance, which have been made by several chemists[*] on stannic-ethyd, show that the action of bromine on that body is represented by the equation; $Sn(C_2H_5)_4+Br_2=Sn(C_2H_5)_3Br+C_2H_5Br$. We expected a precisely similar reaction with silicic ethyd, and were surprised to find that the ethyl was held so strongly by the silicon, that it could not be removed by bromine or by chlorine, and that the only action was a substitution of the bromine or chlorine for hydrogen in the organic radical.

Bromine does not attack pure silicic ethyd at the ordinary temperature; but when two atoms of bromine are heated in a sealed tube with silicic ethyd for 1½ hours at 140°, the color of the bromine disappears completely, and on opening the tube a large quantity of bromhydric acid escapes, while not a trace of bromid of ethyl can be obtained by heating the liquid. The contents of the tube distil at 160°—260°, leaving a small quantity of carbonized matter as a residue.

We could not succeed in isolating a product with a constant boiling point by fractionated distillation.

The portion of the liquid which distilled 230°—240° appeared to have nearly the composition of the mono-bromated compound, but, on redistilling, the portion which distilled at a lower temperature, 220°—230°, was found to contain less carbon and hydrogen, and there was an appearance of decomposition during the distillation.

I. *Boiling-point* =230°–240°. *Substance* =0·2990 *grms.*; CO_2= 0·4810 *grms*; H_2O=0·2400 *grms.*

II. *Boiling-point*=220°–230°. *Substance*=0·3245 *grms.*; CO_2= 0·4920 *grms*; H_2O=0·2400 *grms.*

III. *Same substance*=0·6250 *grms.*; *AgBr*=0·5051 *grms.*

	I.	II.	III.	Calculated for $SiC_8H_{19}Br$.
C	43·8	41·30	----	43·65
H	8·8	8·21	----	8·52
Br	----	----	34·38	35·87

We suspected that the portion of the liquid having the highest boiling point might contain a dibromated product, and, after a distillation in vacuo, analyzed a body which distilled at 120°–140°.

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[*] **Frankland, Annalen der Chem. und Pharm., lxxxv, p. 329; cxi, p. 44. Buckton, ibid, cix, p. 218; cxii, p. 220; Cahours, Annales de Chim. et Phys., III, lxii, p. 276.**

I. *Boiling-point*=120°–140° *in vacuo.* *Substance*=0·4600 *grms.* *AgBr*=0·5360 *grms.*

I.	Calculated for $SiC_8H_{18}Br_2$.
Br=48·70	52·98

Not succeeding in obtaining a pure monobromated silicic ethyd, we heated the first product analyzed with acetate of silver with the intention of obtaining an acetate, and finally of obtaining from the acetate by saponification with caustic potash an alcohol. The formulas of the bodies thus sought are, $SiC_8H\ C_2H_3O_2$ and $SiC_8H\ HO$. Neither of them, however, were produced, and we obtained no better result by heating the brominated product to 200° with an alcoholic solution of acetate of potassium. The treatment with caustic potash of the product which had been heated with salts of acetic acid failed to extract the slightest trace of acetic acid, showing that no acetate had been formed and the principal body which was obtained was the oxyd, $O\left\{\begin{matrix}Si(C_2H_5)_3\\Si(C_2H_5)_3\end{matrix}\right.$, whose formation we had already noticed in the preparation of silicic ethyd. This product boiling at 228°–231° was analyzed.

I. *Boiling-point*=228°–231°. *Substance*= 0·2222 *grms.*; CO_2= 0·4743 *grms.*; H_2O=0·2417 *grms.*

We found that the same oxyd was produced, whether the bromine were removed from the bromated silicic ethyd by treatment with acetate of silver, acetate of potassium or caustic potash. An analysis was made of a product which was obtained by heating the bromated silicic ethyd repeatedly with solid caustic potash, and which boiled at 230—235°.

II. *Boiling-point*=230°–235°. *Substance*=0·3305 *grms.*; H_2O =0·3660.

I.	II.	Calculated for $O\left\{\begin{matrix}Si(C_2H_5)_3\\Si(C_2H_5)_3\end{matrix}\right.$
C=58·21	58·75	58·54
H=12·09	12·30	12·19

The last product was not entirely pure but contained a trace of bromine, and it is very difficult to remove the bromine completely from the bromated product, even by a prolonged action of solid caustic potash. Several analyses which were made of the liquid, which had not been treated so long a time with caustic potash, showed that it still contained a considerable quantity of bromine.

It appears from these data that the bromated silicic ethyd, when treated with substances containing oxygen combined with metals having a strong affinity for bromine, loses the atom of ethyl in which the bromine was contained and takes up oxygen in its place. The reaction is probably represented by the equation:

$$2Si(C_2H_5)_3C_2H_4Br+2KHO=O\left\{\begin{matrix}Si(C_2H_5)_3\\Si(C_2H_5)_3\end{matrix}\right.+2KBr+H_2O+C_2H_4.$$

When a small quantity of iodine is added to the bromine, bromid of ethyl is formed by their joint action upon silicic ethyd at 140°.

The bromid of ethyl thus obtained, distilling at about 40°, was analyzed.

I. *Substance*=0·6755 *grms.*; CO_2=0·5565 *grms.*; H_2O=0·2855 *grms.*

	Calculated for C_2H_5Br.
C=22·42	22·01
H= 4·70	4·58

Iodine is almost entirely without action upon silicic ethyd: 2 equivalents of iodine were heated with one equivalent of silicic ethyd at 180° for 12 hours, very little iodhydric acid and not a trace of iodid of ethyl were formed. It is well known that all the other ethyds, obtained by a reaction similar to that which gives rise to silicic ethyd, are easily decomposed by iodine with the formation of iodid of ethyl.

THE OXYD OF SILICIC TRI-ETHYD.

The reaction given above offers the most convenient means for obtaining the oxyd in considerable quantity, while its solubility in concentrated sulphuric acid, which has already been used to separate it from the pure silicic ethyd, serves also to free it from the traces of the bromated product, which remain after the action of caustic potash has been carried as far as possible. On treating the product analyzed above, No. II, with sulphuric acid a small quantity of a gas, which did not contain bromine was given off, and a small quantity of silicic ethyd containing a little bromine remained insoluble in the sulphuric acid. The presence of silicic ethyd accounts for the slight excess of carbon and hydrogen of analysis II. The larger part was dissolved in sulphuric acid and was separated from its solution by diluting with water. After having been washed with water and dried, ¾ of it distilled at 228°—230°.

I. *Boiling-point* =228°–230°. *Substance*=0·1970 *grms.*; CO_2= 0·1495 *grms.*; H_2O=0·2160 *grms.*
II. *Same substance*=0·2860 *grms.*; SiO_2=0·1345 *grms.*

The determination of silicic acid was made by heating the liquid in a sealed tube with strong nitric acid.

I.	II.	Calculated for O $\left\{\begin{matrix}Si(C_2H^5)_3\\ Si(C_2H_5)_3\end{matrix}\right.$
C=58·07	----	58·54
H=12·18	----	12·19
Si=----	21·93	22·76

The density of vapor of the oxyd of silicic triethyd was obtained from the following data:

Difference of weights of the bulb=1·2245 *grms.*
Temperature of the balance, 13°
Temperature of the oil-bath, 285°
Barometer, 760·0$^{\mathrm{mm}}$
Capacity of the bulb, 284·0 *c. c.*
Air remaining, 1·3 *c. c.*
Vapor density, =8·698
Calculated for O $\left\{\begin{matrix}Si(C_2H_5)_3\\ Si(C_2H_5)_3\end{matrix}\right.$ 8·51

ACTION OF CHLORINE ON SILICIC ETHYD.

When dry chlorine gas is passed into silicic ethyd in a vessel surrounded by cold water, the liquid is at first colored yellow by the absorption of chlorine; suddenly this coloration disappears and chlorhydric acid is disengaged, and from that time forward chlorhydric acid is disengaged as fast as the chlorine is absorbed, and no further coloration takes place. Not even a trace of chlorid of ethyl is produced. In order to avoid the formation of products containing too large a proportion of chlorine, the operation is interrupted from time to time and all of the liquid, which boils at a temperature lower than 160°, is distilled off, and the distillate is treated as before with chlorine. Finally the residues thus obtained are subjected to a fractional distillation.

Although we operated on a considerable quantity of silicic ethyd, and made a large number of fractional distillations of the chlorated product, we were unable to isolate bodies having constant boiling points and corresponding in composition to the mono- and bi-chlorated silicic ethyd. Analyses were made of the products which distilled at different temperatures after a number of fractional distillations.

I. *Boiling-point* =180°–190°. *Substance*=0·2120 *grms.; CO_2*= 0·4210; *H_2O*=0·2085 *grms.*
II. *Same substance*=0·3370 *grms.; $AgCl$*=0·2670 *grms.*

Repeated distillation decomposes partially the chlorated compound, and products having the same boiling-point contain a smaller amount of carbon and hydrogen after they have been distilled a number of times.

III. *This analysis was made from a liquid whose boiling-point was* 180°—185°, *but which had been distilled a larger number of times than the last.*

Substance=0·2540 *grms.* CO_2=0·4750 *grms.*; H_2O=0·2432 *grms.*

The following analyses were made of products belonging to the same series of distillations as the first.

IV. *Boiling-point*=190°-200°. *Substance*=0·2585 *grms.*; CO_2 =0·4785 *grms.*; H_2O=0·2280 *grms.*

V. *Same substance*=0·3135 *grms.*; *AgCl*=0·3238 *grms.*

VI. *Another product boiling at* 190°-200°. *Substance*=0·2345 *grms.* CO_2=0·4400 *grms.*; H_2O=0·2145.

The composition of all these products approaches that of monochlorated silicic ethyd.

I.	II.	III.	IV.	V.	VI.	Calculated for $SiC_8H_{19}Cl$.
C=54·15	----	51·00	50·48	----	51·17	53·72
H=10·92	----	10·63	9·80	----	10·06	10·64
Cl=----	19·39	----	----	25·51	----	19·00

Corresponding results were obtained in the analysis of several other products distilling at about the same temperature.

Two products boiling at a higher temperature had nearly the composition of the dichlorated silicic ethyd.

I. *Boiling-point* =200°-210°. *Substance* =0·2245 *grms.*; CO_2= 0·3875 *grms.*; H_2O=0·1817.

II. *Boiling-point*=205°-210°. *Substance*=0·2435 *grms.*; CO_2= 0·4155 *grms.*; H_2O=0·2015 *grms.*

I.	II.	Calculated for $Si\,C^8H_{18}Cl_2$.
C=47·11	46·53	45·07
H= 8·99	9·19	8·45

The decomposition during distillation is so rapid in the neighborhood of 230°, that it was pushed no further. According to the above analysis the boiling point of monochhlorated silicic ethyd is about 180°, and the boiling point of dichlorated silicic ethyd is about 210°.

After having thus endeavored in vain to isolate the monochlorated and dichlorated products, we noticed that after a large number of fractional distillations a considerable portion distilled at 190°-195°, and we made an analysis of this product.

I. *Boiling-point* 190°-195°. *Substance*=0·2740 *grms.*; CO_2= 0·4930 *grms.*; H_2O=0·2380 *grms.*

I.	$SiC_8H_{19}Cl$.	Mean.	$SiC_8H_{18}Cl_2$.
C=49·07	53·72	49·39	45·07
H= 9·65	10·64	9·54	8·45

It appears that, in a mixture of equal equivalents, the two chlorated products have a tendency to distil together at a constant temperature. Bauer* has noticed an analogous fact in regard to the bromids of ethylene and propylene.

Distillation having been found ineffectual to separate the chlorated silicic ethyds, we sought to obtain a separation of these products, or better still, of their derivatives, by chemical means, and in this, after a number of experiments, we succeeded.

We noted that the chlorated products are not attacked by an alcoholic solution of acetate of potassium, except at a high temperature, and also that the one containing two atoms of chlorine is more easily attacked than the other. In fact, the product containing most chlorine is destroyed at a temperature of 130° to 140°, while the monochlorated product is not acted upon at that temperature, and it can be separated from the other by taking advantage of this property.

The chlorated bodies distilling at 180° to 200°, were heated for 3 to 4 hours, at 130° to 140°, in sealed tubes, with an excess of melted acetate of potassium dissolved in absolute alcohol. Chlorid of potassium is precipitated, and on opening the tubes a combustible gas is disengaged. On adding a considerable quantity of water to the contents of the tubes, the salts are dissolved, and an oily liquid is separated. This liquid, after having been washed with water and dried, was treated with strong sulphuric acid, in which the monochlorated silicic ethyd is insoluble, while the products of decomposition of the higher chlorated compounds are soluble. These last seem to consist principally of the oxyd of silicic triethyd.

The insoluble liquid was drawn off by means of a pipette, washed with water, dried and distilled. We did not attempt to free it from the slight quantity of silicic ethyd which it contained, for fear of losing too much of the product, but preferred to use it for the following reaction. The liquid obtained by the process described above, distilling 180° to 190°, was sealed in a tube, with an alcoholic solution of acetate of potassium, and heated at a higher temperature than before, namely, at 180°, for several hours.

THE ACETIC ETHER AND THE ALCOHOL OF SILICIC ETHYD.

At 180°, the monochlorated product is acted upon by acetate of potassium, chlorid of potassium is formed, and on opening the tube no disengagement of gas is noticed, as in the case of the higher chlorated products. After adding water to the contents of the tube, a liquid separates out, which is mostly soluble in concentrated sulphuric acid. The treatment with sulphuric acid was resorted to in order to purify the principal product from a small quantity of the undecomposed chlorated product, and of silicic ethyd, which remain insoluble. The solution in sulphuric acid was carefully decanted and slowly poured into a flask containing a considerable quantity of water in order to avoid an elevation of temperature. An oily liquid separated on the surface, which, after having been washed and dried with chlorid of calcium, distilled almost completely at 208° to 214°. It had a faint ethereal odor, and smelt also of acetic acid. It burnt with a luminous flame and a smoke of silicic acid.

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* Bulletin de la Société Chimique, i, p. 203.

This liquid proved to be the acetate derived from silicic ethyd by the reaction which is represented by the following equation: $SiC_8H_9Cl+KC_2H_3O_2=SiC_8H_{19}C_2H_3O_2+KCl$.

It may be considered as an acetic ether, in which the residue SiC_8H_{19} plays the part of monoatomic radical $\left.\begin{matrix}SiC_8H_{19}\\C_2H_3O\end{matrix}\right\}O$. At first the method of purification with sulphuric acid was not employed and the range of temperature at which the ether distilled was larger. The following analyses were made of such products:

I. *Boiling-point*=200°–210°—*Substance*=0·2225 *grms.*; CO_2= 0 4845 *grms.*; H_2O=0·2278 *grms.*

II. *Boiling-point*=209°–215°—*Substance*=0·2743 *grms.*; CO_2= 0·5900 *grms.*; H_2O=0·2755 *grms.*

III. *Boiling-point*=215°–225°—*Substance*=0·2940 *grms.*; CO_2= 0·6385 *grms.*; H_2O=0·3100 *grms.*

IV. *Boiling-point*=219°–224°—*Substance*=0·2013 *grms.*; CO_2= 0·4335 *grms.*; H_2O=0·2115 *grms.*

	I.	II.	III.	IV.	Calculated for $SiC_8H_{19}C_2H_3O_2$.
C =	59·38	58·66	59·23	58·73	59·40
H =	11·37	11·15	11·71	11·67	10·89

After the treatment with sulphuric acid a perfectly pure product was obtained.

I. *Boiling-point* =208°–214°—*Substance*=0·2190 *grms.*; CO_2= 0·4790 *grms.*; H_2O=0·2210 *grms.*

II. *Boiling-point* =209°–210°—*Substance*=0·2540 *grms.*; CO_2= 0·5560 *grms.*; H_2O=0·2580 *grms.*

I.	II.	Calculated for $SiC_8H_{19}C_2H_3O_2$.
59·65	59·69	59·40
11·21	11·28	10·89

The results of an elementary analysis are not very decisive of the purity of a compound of this nature, and the best means of obtaining the true composition of the ether of an acid is afforded by its saponification with caustic potash.

Our ether proved to be so stable, that its saponification could not be completely effected by heating it to 180° with an aqueous solution of potash, but it can readily be accomplished by heating to 120°–130° with an alcoholic solution of caustic potash.

I. 0·5954 grms. of the ether were sealed in a tube with four grms. of an alcoholic solution of caustic potash and with an additional quantity of alcohol. The saponification was effected by heating the tube to 130°. Four grms. of the potash solution required for neutralization 10·8 c.c. of a solution of sulphuric acid, containing 0·1118 grms. H_2SO_4 per cubic centimeter. The contents of the tube, after the potash had been partially neutralized by the acetic acid derived from the saponification of the ether, only required 8·6 c.c. of the sulphuric acid solution for complete neutralization; consequently the quantity of acetic acid present in the ether corresponded to 2·2 c.c. of the sulphuric acid solution. According to calculation it should have been 2·6 c.c. In order to prove that acetate of potassium had really been formed, the neutralized solution was evaporated on a water-bath, and the dry residue was treated with absolute alcohol. Acetate of potassium was dissolved by the alcohol, and could be easily recognized on evaporating the solution.

The other product formed during the saponification must be an alcohol of silicic ethyd containing the same radical as the above mentioned acetic ether, and, on repeating the experiment with a considerable quantity of substance, we succeeded without difficulty, on treating with water the alcoholic solution after saponification, in obtaining such a compound. The alcohol of silicic ethyd is a liquid lighter than water, and insoluble in it, and boiling at about 190°. Its formula is $SiC_8H_{19}OH$.

I. *Substance incompletely purified distilled* 185°–190°. *Substance* =0·1477 *grms.; CO_2*=0·3200 *grms.; H_2O*=0·1667 *grms.*

II. *Purified substance boiling* 185°–195° — *Substance*=0·2100 *grms.; CO_2*=0·4600 *grms.; H_2O*=0·2355 *grms.*

III. *Same substance*=0·1970 *grms.; CO_2*=0·4320 *grms.; H_2O*= 0·2210 *grms.*

	I.	II.	III.	Calculated for $SiC_8H_{19}OH$
C=	59·08	59·74	59·80	60·00
H=	12·54	12·46	12·46	12·50

The properties of this body, so far as we have studied them, are entirely similar to those of the alcohols of the ordinary series, which contain a large number of atoms of carbon.

We have already shown that it forms an ether with acetic acid; it also forms an alcoholate of sodium, dissolving that metal with disengagement of hydrogen to form a gelatinous mass, which regenerates the alcohol and gives a solution of caustic soda, when it is treated with water.

It is apparent, from the facts above cited, that, in a certain class of reactions, the group of atoms, SiC_8H_{19}, remains intact, and plays the part of a compound radical, in the same way that the ordinary alcoholic radicals, like ethyl, (C_2H_5), and amyl, (C_5H_{11}), remain undecomposed in the same class of reactions. The new body, in its relations to many reagents, is simply a new hydrocarbon, with silicon substituted for a portion of the carbon.

It is most remarkable that the presence of silicon in the hydrocarbon modifies its properties to so slight a degree: in fact we have considered silicic ethyd as belonging to the same class of bodies as the petroleum oils, and have applied to it the same methods of study which have been used so successfully by Pélouze and Cahours* in treating the American petroleums, and have obtained similar results. The want of the necessary material has prevented us from completing the study of the alcohol, but we hope to take up the research again.

We may embody this idea of the theory of the constitution of silicic ethyd, and recall its analogy with a well known class of hydrocarbons, by naming it the *hydrid of silico-nonyl.* The alcohol is *silico-nonylic alcohol* containing the radical silico-nonyl (SiC_8H_{19}).

Hydrid of nonyl, (C_9H_{20}), of which the silicic compound may be regarded as a product of substitution of silicon for one atom of carbon, should be obtained by the action of zinc-ethyd on the chlorid of carbon—$CCl_4 + 2Zn(C_2H_5)_2 = 2ZnCl_2 + [C(C_2H_5)_4 = C_9H_{20}]$, and we have made some experiments with a view to preparing it in this way. Thus far, however, we have failed to obtain the reaction with chlorid of carbon and zinc ethyl alone, and also with zinc ethyl to which sodium has been added.

Friedel and Ladenburg† have obtained a hydrocarbon by the action of zinc ethyd upon methyl-chloracetal, whose composition is represented by the formula $C(CH_3)_2(C_2H_5)_2$, and which must be regarded as analogous in constitution and mode of formation with the body, which we sought to obtain from the chlorid of carbon, so that the production of the latter may be considered as probable.

We have noticed, while studying the bromated products of substitution of silicic ethyd, that in them the residue SiC_8H_{19} combined with the bromine does not act like an alcoholic radical, but that, when they are brought in contact with acetate of potassium, or with caustic potash, the atom of ethyl containing the bromine is separated from it and replaced by oxygen, to form the oxyd of silicic triethyd; the same is true of the chlorated compounds, which contain more than a single atom of chlorine, and here the presence of silicon reveals itself in the hydrocarbon, determining a point of weaker cohesion, which results in the rupture of the union between itself and the carbon of the atom of ethyl containing the chlorine. In these last mentioned reactions, therefore, an analogy is apparent between silicic ethyd, and the other known compounds of organic radicals with metals.

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* Comptes Rendus de l'Académie des Sciences, lvi, p. 565, 1863.

† Comptes Rendus de l'Académie des Sciences, lxiii, p. 1083, and Bulletin de la Société Chimique, [2], vii, p. 65.

The chlorated silicic ethyd, whose boiling point was 210°–220°, was experimented upon, and the body was heated with an excess of acetate of potassium in alcoholic solution. On opening the tubes we found that the same gas was given off in greater abundance, whose production we have already noticed from the higher chlorated compounds which approach the monochlorated compound more nearly in their composition. In one operation we observed that the gas, which was first evolved, contained chlorine, and could be absorbed by bromine; this was not the case with the gas given off afterwards.

In another operation, in order to obtain a clue to the reaction, we passed the gas into a solution of subchlorid of copper in ammonia, and then into bromine. A small quantity of the cupric compound of acetylene and of the bromid of ethylene, or of chlorated ethylene, was formed.

The principal product of the reaction was the same oxyd of silicic triethyd, which was produced by the action of acetate of potassium on the bromated silicic ethyd.

This product was treated as before, with sulphuric acid to purify it, and the following analysis was made of it:

I. *Substance*=0·2210 *grms.*; CO_2=0·4745 *grms.*; H_2O=0·2460 *grms.*

I.	Calculated for $O\left\{\begin{matrix}Si(C_2H_5)_3\\Si(C_2H_5)_3\end{matrix}\right.$
C=58·55	58·53
H=12·36	12·19

It is possible that the formation of this compound may be represented by the equation:

$$2Si(C_2H_5)_3C_2H_3Cl_2+2KC_2H_3O_2=2KCl+O(C_2H_3O)_2+$$
$$2C_2H_3Cl+O\left\{\begin{matrix}Si(C_2H_5)_3\\Si(C_2H_5)_3\end{matrix}\right.$$

The formation of acetylene may be due to the action of chlorated ethylene upon the acetate of potassium, according to the equation:

$$C_2H_3Cl+K\ C_2H_3O_2=HC_2H_3O_2+KCl+C_2H_2.$$

This interpretation of the reaction can not be considered as established without further verification; but one fact is unquestionable, and appears to us very important, namely: that it is an atom of ethyl containing two or more atoms of chlorine, which separates from the silicic ethyd to give rise to the oxyd, and consequently the continued substitution of chlorine for hydrogen takes place in an atom of ethyl already containing chlorine, and not as might have been expected in an atom of ethyl, which contained no chlorine already. The fact is contrary to our usual ideas, founded upon the electro-chemical theory in regard to chemical affinity, and its significance in the study of organic radicals is easily appreciated. According to the theory, a group of atoms, already containing chlorine, should be less inclined to receive a further amount, and the substitution should take place by preference in the atoms of ethyl which are free from chlorine.

The observation which we have made is not without analogy, for Lieben* has shown that, in the action of chlorine upon ordinary ether, the substitution of two atoms of chlorine takes place in one of the atoms of ethyl to form the body, $\left.\begin{matrix}C_2H_3Cl_2\\C_2H_5\end{matrix}\right\}O$, while the other atom of ethyl remains unacted upon. It is remarkable that this analogy should exist between ether and silicic ethyd, a body resembling a hydrocarbon so much more clearly than ether does.

OXYDATION OF SILICIC ETHYD.

We have already stated that silicic ethyd is a very stable compound, and that in order to oxydize it completely with strong nitric acid or with a mixture of chlorhydric acid and chlorate of potassium, it is necessary to operate at a temperature of 180°. We endeavored to obtain a partial oxydation by heating with fuming nitric acid at a lower temperature, and for this purpose an apparatus was made entirely of glass, in which the silicic ethyd was boiled with the nitric acid, and the vapors were condensed and made to flow back into the vessel which was heated. After prolonged ebullition, during which nitrous fumes were given off, the product was washed with water and treated with ether, in which it was in great part soluble. On evaporating the etheric solution a viscous liquid was obtained, which could not be distilled, but which was dried in vacuo over sulphuric acid and analyzed.

I. *Substance*=0·2275 *grms.*; CO_2=0·3880 *grms.*; H_2O=0·2010 *grms.*

II. *Substance*=0·3205 *grms.*; SiO_2=0·1960 *grms.*

I.	II.	Calculated for $SiO(C_2H_5)_2$
C=46·51	----	47·05
H= 9·81	----	9·80
Si=----	28·53	27·45

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* Bulletin de la Société Chimique, II, viii, p. 429, and Annalen der Chem. und Pharm., cxli, p. 236.

These numbers accord very nearly with those required by the composition of an oxyd $SiO(C_2H_5)_2$ in which two atoms of of ethyl are replaced by one atom of oxygen, and the body would be the next more advanced product of oxydation to the oxyd of silicic triethyd, but, as we have not made sufficient experiments to determine the chemical properties of this substance, we are unable to form a decided opinion as to its true nature.

SILICIC METHYD.

We first attempted to prepare silicic methyd by means of mercuric methyd and chlorid of silicon. The two bodies were heated together in a sealed tube for 15 hours at 180°–200°; lamellar crystals of mercuric chlorid and methyd (HgC_2H_3Cl) were formed, and a gas which was not absorbable by bromine was given off on opening the tube. The liquid contents of the tube were distilled with caustic potash in order to destroy the chlorid of silicon, and were found to consist chiefly of undecomposed mercuric methyd together with a small quantity of a more volatile body, which seemed to be silicic methyd. This mode of preparation not being advantageous, we had recourse to zinc methyd in the next operation.

The mercuric methyd was transformed into zinc methyd by the process given by Frankland, and the latter was heated with an excess of silicic chlorid for several hours at 200°. A reaction commences at 180°, but a temperature of 200° is necessary to render it complete. The tube contained chlorid of zinc as a white powder, and we were able to obtain from it by distillation a volatile liquid, which, after having been washed with a solution of caustic potash, had the same properties as the silicic methyd obtained with mercuric methyd.

The preparation of zinc methyd by means of the mercuric methyd is tedious and disagreeable, and we therefore preferred to obtain it by the action of zinc upon the iodid of methyl, a method which has been already employed by Butlerow* on a small scale. Instead of the glass tubes which he employed, we used Frankland's digester, in which we heated zinc-turnings at 120° with iodid of methyl, taking the precaution to interrupt the operation from time to time in order to cool the digester with ice-water, and to open it to allow the escape of the gases† which form in large quantity. We obtained in this manner, by distilling directly from the digester, zinc methyd, which still contained a little iodid of methyl. This product was treated in the digester with zinc turnings and chlorid of silicon, after we had assured ourselves that chlorid of silicon does not act upon metallic zinc at the temperature employed, but only upon the zinc methyd which is formed from it. Indeed chlorid of silicon is not decomposed even by sodium except at a high temperature.

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* Bulletin de la Société Chimique, v, p. 582.

† Notwithstanding Mr. Butlerow's assertion (Annalen der Chem. und Pharm., cxliv, p. 39), that the zinc methyl gases are not poisonous, one of us has repeatedly experienced ill effects from breathing them, and it is advisable to set them on fire as they issue from the digester. It is possible that the difference between our own observations and those of Mr. Butlerow may be due to impurities in the zinc or to the fact that we used a copper digester.

In order to allow the zinc to act upon the small quantity of iodid of methyl contained in the zinc methyd, the digester was first heated for 12 hours at 120°, and then for 10 hours at 200° to effect the reaction between the chlorid of silicon and the zinc methyd. The digester was cooled with ice before it was opened. After the escape of the gas the product was distilled into recipients cooled with ice, and then treated at 0° with a solution of caustic potash in order to destroy any excess of chlorid of silicon which might be present. It is important to employ nearly equivalent quantities of zinc methyd and of silicic chlorid, because the heat which is developed by the action of the caustic potash upon the excess of either occasions a loss of the silicic methyd.

Silicic methyd obtained by this process is a clear transparent liquid, lighter than water and boiling at 30°–31°. It burns with a luminous flame and a smoke of silicic acid. The following analyses were made of this substance:

I. *Substance*=0·1855 *grms.*; CO_2=0·3660 *grms.*; H_2O=0·2275 *grms.*

The substance was burnt too quickly in the first analysis and a loss of carbonic acid was occasioned.

II. *Substance*=0·1940 *grms.*; CO_2=0·3875 *grms.*; H_2O=0·2350 *grms.*

I.	II.	Calculated for $Si(CH_3)_4$
C=53·81	54·47	54·54
H=13·63	13·46	13·63

The determination of silicon in this substance presents unusual difficulties, on account of its great stability in contact with oxydizing agents and its volatility. We employed the method, which was used for the silicic ethyd, but here it is necessary to enclose the substance in a bulb which is broken after sealing the tube. (Silicic ethyd boils at so high a temperature, that it can be weighed in a long narrow tube with a cork). Fuming nitric acid was the oxydizing agent used, and the silicic methyd was heated with it for two days at 200°, but was found on opening the tube not to be completely decomposed. In a second determination we used a very large excess of fuming nitric acid and heated for forty hours at 250°–300° and obtained the following result:

Substance=0·2330 *grms.*; SiO_2=0·1405 *grms.*
Si= 29·85 p. c. calculated=31·81.

It is difficult to remove the silicic acid completely from the tube by a treatment with caustic potash, and either there was a loss in manipulation, or the silicic methyd was not completely decomposed even at 300°.

Two other determinations made at a still higher temperature were lost from the bursting of the tubes.

The above analyses leave no doubt that the composition of silicic methyd is represented by the formula: $Si(CH_3)_4$, and this result is completely in accordance with the vapor density determination made by Gay Lussac's method.

Substance employed=0·1813 *grms.*
Temperature of the bath 100°.
Height of the barometer 751·7 *m.m. at* 8°.
Volume occupied by the vapor 85 *c.c.*
Height of the mercury in the measuring tube 194 *m.m.*
Vapor density by experiment =3·058
Vapor density by calculation =3·045

If the silicic ethyd represents the hydrid of silico-nonyl, the silicic methyd is the hydrid of silicopentyl, SiC_4H_{12}, or the silicated substitution product of the hydrocarbon of the amylic alcohol series.

The want of material has prevented us from carrying the study of this body farther; we will only call attention to the great difference in the boiling points of theses homologous silicated hydrocarbons.

Silicic ethyd boils at	152°·5	centigrade.
Silicic methyd "	30°·	"
Difference =	122°·5	"

This difference corresponds to 30°·5 for each increment of CH_2 and is quite at variance with Kopp's law. This fact is the more remarkable since we have shown that in the homologous silicic ethers of the normal series, the difference of boiling-point corresponding to an increment of CH_2 is 11° and in the disilicic ethers it is only 5°.

SILICIC ETHYD AND METHYD.

It is obvious that considerable interest attaches to the completion of the series of the silicated hydrocarbons, of which we have described the members corresponding to the pentyl and the nonyl group, and it appears probable that all the intermediatė ones may be obtained by the simultaneous action of zinc methyd and zinc ethyd, in different proportions upon the chlorid of silicon; we have not yet, however, been able to make any extended researches in this direction, but will give the result of a single experiment conducted on a small scale.

In order to prepare a mixture of zinc methyd and zinc ethyd, we heated in the digester for 24 hours at 100°, a mixture of iodid of methyl and of iodid of ethyl with zinc turnings. The liquid after distillation was found to contain iodine, and it was reheated for 48 hours with the pulverized alloy of zinc and sodium and with zinc turnings; we thus obtained 35 grms. of a product, whose boiling-point was lower than that of zinc ethyd. To this product 8 grms. of zinc methyd were added and 85 grms. of the chlorid of silicon, and the whole was heated for seven hours at 195°. The resulting product was distilled, washed with a solution of caustic potash, and treated with strong sulphuric acid, 10 grms. of a liquid, insoluble in sulphuric acid were obtained, and were submitted to a fractional distillation. The greater part passed at 63°–67°. This portion was analyzed.

I. *Substance*=0·1776 *grms.*; CO_2=0·4240 *grms.*; H_2O=0·2284 *grms.*

	$Si(CH_3)(C_2H_5)_3$	$Si(CH_3)_2(C_2H_5)_2$
C=65·11	64·61	61·20
H=14·28	13·84	13·79

It would appear from the analysis that this body is silicic methyd-tri-ethyd nearly pure, but its low boiling-point renders this highly improbable, and it is possible that the high percentage of carbon and hydrogen may be due to the presence of some saturated hydrocarbon. According to its boiling-point it should be silicic tri-methyd ethyd, but we had not a sufficient quantity of material to make further experiments in order to determine its true composition. It is certain that the body was neither silicic methyd nor silicic ethyd but an intermediate product.

The bodies studied in this research belong to the same type as chlorid of silicon.

Chlorid of silicon,	$SiCl_4$.
Silicic ethyd,	$Si(C_2H_5)_4$.
Chlorated silicic ethyd or chlorid of silico-nonyl	$Si(C_2H_5)(C_2H_4Cl)$.
Acetate of silico-nonyl, .	$Si(C_2H_5)_3(C_2H_4C_2H_3O_2)$.
Silico-nonylic alcohol,	$Si(C_2H_5)_3(C_2H_4OH)$.
Dichlorated silicic ethyd,	$Si(C_2H_5)_3(C_2H_3Cl_2)$.

Except the oxyd which has a strong tendency to form in so many reactions, and which belongs to a different type: namely, to one in which two groups of molecules are linked together through the medium of oxygen. It may be referred to the same type as the disilicic ethers.

Disilicic hexethylic ether, $O\left\{\begin{array}{l}Si(C_2H_5O)_3\\ Si(C_2H_5O)_3\end{array}\right.$

Oxyd of silicic tri-ethyd, $O\left\{\begin{array}{l}Si(C_2H_5)_3\\ Si(C_2H_5)_3\end{array}\right.$

The oxychlorid of silicon, $O\left\{\begin{array}{l}SiCl_3\\ SiCl_3\end{array}\right.$

belongs also to the same type.

In some of these compounds the chlorine is represented as combined with carbon in the place of hydrogen, and in others as combined directly with silicon. The difference in formulas is justified by the marked difference in properties which the chlorine exhibits in the two kinds of combination.

Compounds containing chlorine combined directly with silicon are readily decomposed by water with formation of chlorhydric acid; while the chlorine in the other class of compounds displays the same inertness, which characterizes it in hydrocarbons, and is not acted upon by water, and only at a very high temperature by acetate of silver. Exactly the same difference of properties obtains for the radical of acetic acid contained in the acetins of silicic ether, where it is in direct combination with the silicon, and the same radical contained in the acetate of silico-nonyl, where it is in combination with carbon.

Only one point remains in which the analogy between silicon and carbon is incomplete. We have said that the most characteristic property of carbon in organic bodies is its power of combining directly with itself to form a complex molecule, capable of combining still farther with other elements, as when two atoms of carbon, C_2, combine with six atoms of hydrogen to form the hydrid of ethyl, C_2H_6. All the bodies thus far discussed belong to the simplest type of carbon compounds, as CH_4 and its analogue SiH_4; and in silicic ethyd 4 times C_2H_5 occupy the place of H_4; but Friedel and Landenburg* have lately completed the analogy and obtained the body, $Si_2(C_2H_5)_6$, belonging to the same type as Si_2H_6 and C_2H_6, showing that even in its quality of forming condensed compounds silicon resembles carbon.

* Comptes Rendus de l'Académie des Sciences, lxviii, p. 920, 1869.

Art. XXXIII.—*Description and Analysis of the Franklin County Meteoric Iron; with remarks on the presence of Copper and Nickel in meteoric irons; the method of analyzing the same; and the probability of the Lead in the Tarapaca iron having been originally foreign to that mass;* by J. Lawrence Smith, Louisville, Ky.

1. *The Franklin County Meteoric Iron.*

The Franklin County meteoric iron was first brought to my attention in a blacksmith shop in Frankfort, Kentucky. It was carried there to be tested in regard to its quality as iron; being supposed by its discoverer to indicate an iron mine. Mr. Nelson Alley became possessed of it, and kindly presented it to me.

It came from a hill eight miles southwest of Frankfort, lat. 38° 14′, long. 80° 40′ (Greenwich), and was discovered in 1866. It passed into my possession in 1867, and was then described by me, but the manuscript was lost after its leaving my hands, and the original notes were displaced; the notes have been recently discovered and the iron again analyzed.

Its form is somewhat globular, with a highly crystalline structure. Its weight was twenty-four pounds, and this appears to have been its original weight, only a few flakes having become detached by the rusting through of some of the fissures —sp. grav. 7·692.

Its composition when perfectly freed from rust and earth is

Iron,	90·58
Nickel,	8·53
Cobalt,	0·36
Copper,	minute quantity
Phosphorus,	0·05
	99·52

Having, as it will be seen, the usual composition of meteoric irons.

While on the subject of this iron, I will add some remarks.

2. *On the presence of Cobalt in Meteoric Irons.*

My attention has been directed again and again to meteoric irons, whose analyses are given without mention of the presence of cobalt, and in some instances, with the distinct statement that it is absent, as in the recent examination of a meteoric iron from Auburn, Macon county, Alabama, by Professor Shepard, who states that "neither cobalt, tin nor copper was detected in this iron." I cannot but suggest the importance of making a most critical examination of these irons before pronouncing this fact; for in every analysis that I have made of meteoric irons, (over one hundred different specimens), with this in view, cobalt has been invariably found, along with a minute quantity of copper. A great many of the analyses made were of irons that had been previously examined without a recognition of the cobalt.

The presence of these ingredients, even in small quantities, is a matter of considerable mineralogical interest, as is the case of the presence of small quantities of other elements in many minerals; a fact that I will have occasion to refer to at some future time, in connection with leucite and other silicates.

As a guide to those who may wish to know the manner of my examination of meteoric iron, I will give a little in detail the method adopted in separating the metals.

Method of analysis.—A small piece of the iron is selected, perfectly free from crust and earthy matter. I sometimes plunge the fragment into nitric acid, somewhat diluted, warm the acid and continue the action for a few seconds, withdraw the iron, wash well and dry it. The piece selected for analysis should be about one gram, or a little over, (except where the copper is sought for quantitatively, and then at least ten grams should be used for the copper estimate alone). Treat the iron in a porcelain capsule, or glass flask, with a mixture of hydrochloric and nitric acids, consisting of four parts of the former to one of the latter, and about as much water as acid; dissolve over a water-bath; if a capsule be used, invert a funnel over the mixture, the edges of the funnel entering the capsule, but not touching the mixture. Continue the action on the water-bath until the solution is complete, evaporate to dryness (having washed what may adhere to the inner surface of the funnel into the capsule), then add a little more hydrochloric acid and evaporate again nearly to dryness; this is done to insure driving off the last portion of nitric acid, and rendering the iron easily soluble. Add to the contents of the capsule an ounce or two of water, and, if there be a residue, it must be collected on a weighed filter, dried, weighed, and reserved for future examination; if the quantity be too small for examination, a larger portion of the iron must be examined with special reference to this residue—which most commonly is a silicate, but may contain carbon or chromic iron (chromite).

If, however, there is no residue, proceed to the next step at once without filtering; if the solution has been filtered, the following steps are the same. Examine first for sulphur. This is done by adding a few drops of chlorid of barium; if there is a precipitate, it is collected on a filter, and the sulphate of baryta obtained furnishes the amount of sulphur present. Next pass a stream of sulphuretted hydrogen through the filtrate to complete saturation, previously adding a drop or two of sulphuric acid; much sulphur will be deposited and a very minute quantity of copper (not traceable by the color of the precipitate, but only recognized by the most delicate tests after the sulphur of the collected precipitate is burnt away); the solution, thrown on a filter, leaves the precipitated sulphur, the little trace of copper, and all the excess of the baryta that had been added, if the excess had been slight. The filter is then ignited in a porcelain crucible—the residue treated with a few drops of nitric and sulphuric acids, which suffices to dissolve the copper, and when evaporated to dryness leaves the copper in the form of sulphate, slightly acid, as there is no necessity of heating so high as to drive off the very last trace of sulphuric acid. The presence of the trace of copper is easily shown by adding a drop or two of water to dissolve the sulphate, and with the end of a glass rod, placing a little of the solution on a clean and bright surface of iron, as the blade of a knife, for instance. In no examination of a meteoric iron have I failed to detect copper by this means.

When more iron is used, and there is consequently more copper, it can be separated and weighed. Tin and lead will also be found in the precipitate, if they be present, but I have never detected either, except lead in the case of the Tarapaca iron, which I have every reason to believe was originally foreign to the iron.

The sulphuretted hydrogen precipitate, which I have always obtained, is so minute in a gram of iron that it may be dispensed with, and the iron, nickel and cobalt be separated, which is accomplished as described a little farther on. If, however, sulphuretted hydrogen has been used, the iron in solution is in the form of protoxyd, and must be converted into the peroxyd, which is accomplished by adding a little chlorate of potash and hydrochloric acid that have been made to react on each other by heating, before adding it to the boiling solution of iron, &c.

The solution of iron should have a bulk of ten or twelve ounces; to it is added a solution of carbonate of soda in sufficient quantity to nearly neutralize the free acid; the iron is now precipitated by acetate of soda, with all the well known precautions. I wash this precipitate only partially, and detach it from the filter by washing it into a beaker, and re-dissolving it by hydrochloric acid, and precipitate it a second time by acetate of soda; I then subject it to complete washing, and estimate the iron in the way usually employed. This second precipitation is necessary to separate an appreciable quantity of nickel remaining in the acetate of iron after the first precipitation; and after considerable experience, I must say that it is the only method of separating, with any degree of accuracy, iron from nickel.

The solution separated from the iron, and containing nickel and cobalt, is concentrated down to four or five ounces, then treated with caustic potash or soda, thrown on a filter, and washed with hot water, with all the usual precautions when nickel is precipitated by an alkali. The precipitate is dried, ignited and weighed; after which it is dissolved in nitric acid over a water-bath, and the acid solution evaporated nearly to dryness; about two or three drams of water are added, then a concentrated solution of nitrite of potash, then an excess of acetic acid; this is now set aside for forty-eight hours, during which time the nitrite of cobalt is completely separated; it is next thrown on a filter, washed and estimated in the manner proposed by the author of this method; I usually employ a concentrated solution of the sulphate of potash to wash with. This method, to say the least of it, has in my experience proved fully equal to the cyanid method, and is much more simple; and by it I have never failed to detect and estimate cobalt in every meteoric iron that has come under my examination.

In examining for *phosphorus*, the following method is adopted: To three or more grams of the iron in a porcelain capsule, add nitric acid diluted with water; then invert a funnel over the mixture, to protect from loss during the action, evaporate to dryness over a water-bath, and then on a sand-bath to a temperature of 500° or 600°; the iron is thus converted into an oxyd with little or no nitric acid remaining, and the phosphorus is transformed into phosphoric acid that is now combined with oxyd of iron. The residue is detached as thoroughly as possible from the capsule, and mixed with twice its weight of carbonate of soda, or, better still, with a mixture of carbonates of soda and potash; a little carbonate of soda added to the capsule, and rubbed with a pestle, detaches the last portion of oxyd of iron, or rather leaves so small an amount as to make no error in the future steps of an analysis where the original quantity of phosphorus is so small.

The mixture of oxyd of iron and phosphate is now to be heated in a platinum crucible to the point of fusion of the carbonates for about twenty minutes; then heat the mass with water, when the excess of carbonates will be dissolved, and what phosphate may have been formed; the phosphates will represent all the phosphorus in the iron. Now neutralize the carbonate with hydrochloric acid, and estimate the phosphorus in the ordinary way, by a magnesia salt.

With regard to the detection of chromium, and other special constituents of some meteoric irons, especially those containing some siliceous minerals intimately mixed in the iron, it is not the province of this paper to discuss.

3. *Lead in Meteoric Irons.*

The only instance of the finding of lead in meteoric irons is that of the Tarapaca iron, found in 1840, in Chili, which was examined by Mr. Greg; the metallic lead was detected by him in small masses of varied dimensions.

I have examined several specimens cut from the original mass of iron, two of which are in my possession, and my conviction is, that the metallic lead was altogether foreign to the iron when it originally fell, and has been doubtless derived from lead with which the mass was probably treated by the original discoverers for the purpose of extracting some precious metal, they being ignorant of its true nature. My reasons for coming to this conclusion are, that the lead is found in cavities near the surface of the iron, these cavities having channels of more or less size leading to the exterior of the mass; the iron is honey-combed in its character in many places, which is evident to the eye, and is also indicated by its specific gravity 6·5. In pieces of the iron, detached from the interior of the mass and examined with the utmost care by a magnifying glass to see that there is no possible fissure in it, no lead has been found. These pieces are exceedingly difficult to obtain, and can only be had in very small pieces.

The crust of the iron having the most cavities furnishes most lead, and is in some parts covered by a fused yellow crust of oxyd of lead; this last fact has no significance, however, in the present consideration of the matter. Without venturing to insist too sharply on the view here taken, after the careful examination of so distinghished an observer as Mr. Greg, I recommend this view of the subject to those having larger specimens of the iron than myself.

ART. XXXIV.—*Remarks on the alkalies contained in the mineral Leucite;* by J. LAWRENCE SMITH.

IN examining recently many of the silicates containing alkalies, my attention has been called to Leucite, and it is on that mineral especially that I would now remark, reserving for another time my observations on the other silicates.

The specimens of leucite examined came from four localities, Vesuvius, Andernach, Borghetta, and Frescati. They were about as good specimens as are obtained from those localities, although all of them were not equally pure. The alkalies found in each calculated as potash were—

Vesuvius,	21·85
Andernach,	20·06
Borghetta,	20·68
Frescati,	20·38

The specimen from Andernach was analyzed for the silica, &c., and found to contain silica 54·75, alumina 23·08, and 1·55 of oxyd of iron; this last seemed to be mechanically disseminated through the crystals.

I say above in relation to the alkalies "all calculated as potash," for the reason that there is a notable quantity of rubidium and cæsium present in all the specimens above mentioned. In fact, by the method adopted in testing for these alkalies abundant indications are obtained of the presence of rubidium, cæsium (the last not so readily), even when operating on but half a gram of the mineral. I am now engaged in working out a method of estimating quantitatively rubidium and cæsium in the presence of other alkalies; by this method, not yet perfected, the quantity of these alkalies in leucite is found to be about $\frac{9}{10}$ of one per cent of the entire mineral.

Of course it is not at all remarkable that the potash in the different specimens of leucite should be the same; but it is a matter of interest to know that, from whatsoever locality it comes, this minute quantity of rubidium and cæsium occurs with it. On some future occasion I hope to be able to bring together certain generalities in this connection of more or less interest to mineralogists.

I have also detected rubidium in half a gram of margarodite and Warwick mica, and have failed to detect it in apophyllite, thomsonite, pectolite, elæolite, chesterlite, cancrinite and other silicates.

ART. XXXV.—*Examination of a new and extraordinary Gas Well in the State of New York;* by Professor HENRY WURTZ.

[Read to the New York Lyceum of Natural History, March 14, 1870.]

A NEW and copious outburst of gas has recently been observed in the township of West Bloomfield, county of Ontario, and State of New York, about twenty miles south of Rochester, and sixteen miles west of Canandaigua.

It is now about four years since the owner of the ground, Mr. Beebe, while boring with the hope of getting petroleum, struck the cavity from which the gas flows, at a depth, as he states, of 500 feet. The bore-hole is tubed down to, and into, the solid rock, and the tube stands about ten feet above the surface. This tube is five inches in diameter; and the issuing gas, when *burning*, as it was when I saw it, gives in a still atmosphere a flame some thirty feet in height. The flow has been stated independently by two parties, who have measured it with large balloons of known capacity attached to the outlet, to be from four to five feet per second, equivalent to from 15,000 to 18,000 feet per hour, or, in the mean, at the rate of about 400,000 cubic feet of gas per day. From observation on the spot, without any means of exact measurement, however, I am prepared to believe the probability of this enormous evolution of combustible gas from the bowels of the rocks. Such a flow really corresponds to a pressure of but a few feet of water. Ten inches should, according to calculation, drive through a pipe 500 feet long and five inches in diameter 22,000 feet per hour of gas, of the density which I have found for this, namely, 0·7. There is, however, here an important residual projectile force, in addition to this. This flow has now gone on for more than four years, and according to the testimony of residents of the vicinity, without any perceptible diminution of energy; indicating, in the aggregate, an escape of some 600,000,000 of feet, about half the yearly make of our largest gas manufacturing company, the Manhattan. The most remarkable feature is the absence of diminution of flow for so long a time,in connection with the low pressure indicated. I hence infer the probability of an indefinite continuance; as the gas must originate not from a reservoir in a state of compression, but from huge masses or surfaces of rock, from which it oozes out gradually at every pore. This inference is justified from the phenomena of other fountains of natural gas, of which so many are known to have flowed from time immemorial without exhaustion. As to the geological age of the bed from which this gas comes, I was told that Professor Hall, having been consulted, considers it to be most probably the Marcellus shale; and on consultation with Dr. R. P. Stevens, whose acquaintance with both the geology and topography of this section is minute, I find him to agree that a bore-hole 500 feet deep, in this locality, would be very likely to terminate in the Marcellus, the beds of which are here probably a hundred feet in thickness. The outcrops of the Genesee slate (which cross to the southward of this locality, its horizon being some twelve hundred feet higher than that of the Marcellus) also emit combustible gas copiously in places. I cannot dwell at this time, however, on the geological question, but must pass to the chemical examinations. These are still in progress, but much that has interesting has nevertheless been developed.

The points, which I attempted to determine on the ground, though with very imperfect means, were the temperature with which it issues, the photometric power (which is quite appreciable), and the effect of intense cold upon the latter.

The temperature.—A small hollow semi-cylinder of wood, closed at the bottom, was cemented with beeswax against the side of the well-tube near the ground, filled with quicksilver, and the thermometer inserted. It was found, however, that the temperature was not constant, showing a heating of the iron by radiation and conduction from the flame above. At one time, however, the temperature sunk to 59° F., so that the true temperature is below this, it may be as low as 50°; very low for a depth of 500 feet. Doubtless the gas is cooled by expansion from a state of compression. (A curious suggestion occurs here regarding causes of irregularity in increase of temperature in descent in some fossiliferous rocks.)

The candle power was determined with a standard candle by contriving a small dark room with a large blanket shawl, using of course the Rumford or shadow test. The gas was burned from a five-foot steatite-tip bats-wing burner, being first passed through a glass tube so stuffed with cotton as to reduce the pressure just to that which gave the maximum of light. The result was about *six candles.* I had not with me an Argand burner, which, especially if with a very contracted throat, would doubtless afford with this gas a considerably higher candle power. It is well known that the effect of carbonic acid in diminishing illuminating power is very far less in the Argand than in flat flame burners.

The condensation test was made by immersing in snow and salt, in a common water bucket, some sixty feet of small india rubber tubing that I had with me. The thermometer stood at 8° F. in the mixture. No change of the candle power occurred during half an hour, and hence the light-giving hydrocarbons present seem to be permanent gases, or at least practically incondensable. Lime-water showed carbonic acid to be largely present.

Samples were taken for analysis in accurately-ground glass-stoppered bottles, with which, and some quicksilver, I had provided myself. These bottles were stopped under quicksilver, the stoppers having been previously smeared with some thick glycerine. These bottles were then carried to the laboratory of the Manhattan Gas Light Co., in this city, and some analyses made; new methods and manipulations being used which were devised by myself, in conjunction with Professor Silliman, and which we have not yet published.

Results of the analyses.

Marsh gas	82·41
Carbonic acid	10·11
Nitrogen	4·31
Oxygen	0·23
Illuminating hydrocarbons,	2·94
	100·00
Density,	0·693

Another density determination gave a considerably higher figure; but, wishing not to exhaust all my material, I have not repeated it, but have adopted 0·7. Calculation gives 0·7043, assuming the three volumes of unknown illuminants to have a density of 1·5.

With regard to these three volumes per cent of illuminant hydrocarbons; as they are absorbed by Nordhausen acid they cannot belong to the saturated hydrocarbons $C_{2n}H_{2n+2}$, and cannot therefore be hydruret of ethylene $C_4H_4H_2$, as found recently according to Fouque and Gorceix in the gases of the Appenines; in most cases in traces only, but in one case to the extent of nearly 18 per cent! (See *Comptes Rendus*, lxix, 946). This seems at first glance in accord with the opinion put forth by Fouque, founded, as he says, on the study of the gases from American petroleum wells, that the gases of the series $C_{2n}H_{2n+2}$ are especially characteristic of sources of petroleum; but as the almost universal *marsh gas* is itself of this series, I cannot see how any such generalization can be accepted. I am still engaged in further examinations of these hydrocarbon constituents, but my material is at present insufficient.

The 1-400th volume of free *oxygen* found is *really present*, as I am convinced, in the gas as it issues, and is not accidental, as Fouque and other analysts have deemed the traces of oxygen they find in such gases. The extreme precautions taken by me, both in collection and transportation, as well as in analysis, make me confident of this; but as important chemical and geological conclusions are here involved I shall make further and repeated tests of this point.

ART. XXXVI.—*On Flame Temperatures, in their relations to Composition and Luminosity;* by B. SILLIMAN and HENRY WURTZ. FIRST PART.

Read to the American Association at Salem, August, 1869.

1. *Calorific powers or effects of gases.*—The calorific powers or effects of gases lie, in our belief, at the very basis of the true theory of the phenomena of luminiferous gases, and have practical bearings that can scarcely be overrated. In fact, our studies of the subject have led us in the direction of the general conclusion that, all other conditions being equal, the *temperature*, in a given flame, is the main factor of luminosity. This, however, may as yet be regarded merely as a hypothesis; in consequence of the imperfection of our present means of actual experimental demonstration of the temperatures of flames. It is a hypothesis, nevertheless, which is in general

accordance with known facts. By the spectroscope, for example, which can recognize only luminous rays, we find that the higher the temperature the greater the number of these luminous rays. The recent results of Frankland upon the development of luminosity by increased pressure, in flames which are non-luminous under atmospheric pressure, are in accordance with this view; increase of temperature necessarily following increase of pressure.

Very vague views have been rife, even among chemists, with regard to the temperatures of luminiferous flames. Some have been satisfied with believing crude hypotheses; such as that the heat-power of a flame is always proportional to the *density* of the gas or vapor undergoing combustion; or that it is proportional to the *amount of oxygen consumed* by a given volume of the gas; and so on. This latter hypothesis has been one of very common acceptation. A view which is even now entertained by some skilful chemists (than which, however, nothing, as will be shown, could be more fallacious) is, that those individual gaseous compounds, which impart the highest luminosity under ordinary conditions, are also the most productive of heat.

The admirable researches of Bunsen, of Heidelberg, placed in our possession some years ago the means of computing, at least with approximate accuracy, the heat of flames of gases of known composition. Few however have properly and successfully applied Bunsen's methods in practice. We consider it quite time that these methods should be introduced to the knowledge of gas engineers, in forms available to them.

Bunsen's formulæ for these computations are based upon the actual experimental determinations of the *total* amounts of heat developed by the combustion of different pure combustible gases with pure oxygen, made by Favre and Silbermann; and upon Regnault's determinations of the specific heats of gaseous products of combustion.

It is not to be maintained that Favre and Silbermann's numbers are strictly correct, but they are doubtless approximate, and at least proportionally correct among themselves. At any rate, they are the best data we have. Those employed here are included in the following table. They are usually given in the text-books for equal *weights* of the gases, but we have reduced them to the standard of equal *volumes* also, as more suitable to our present purpose. This reduction is made simply by multiplying the equivalents for weights, by the densities as given in the third column.

TABLE I.

	Total calorific equivalents. Of equal weights.	Total calorific equivalents. Of equal volumes.	Densities Hydrogen =1.
Carbonic oxyd	34,462° C.	34,462° C.	1
Hydrogen	2,403°	33,642°	14
Marsh gas	13,063°	104,504°	8
Olefiant gas	11,858°	166,012°	14

The meaning of this table is simply that equal weights of water would be heated by the several gases to temperatures proportional to the numbers in the first column, when equal *weights* of the gases are burned; and proportional to those in the second column, when equal *volumes* are burned.

A cursory glance at the figures in the second column of this table might seem to justify the notion, hitherto entertained by many, of the comparatively low calorific powers of hydrogen and carbonic oxyd; and it was doubtless as a consequence of such a comparison as this, that statements have been put forth and widely accepted among American gas engineers to the effect, that the weight of water heated from the freezing to the boiling point by one cubic foot of the four main components of illuminating gas, respectively, is as follows:

Hydrogen	2·22	lbs.	water.
Carbonic oxyd	2·16	"	"
Marsh gas	6·17	"	"
Olefiant gas	10·74	"	"

The figures here being obviously about in the same ratio as those in the second column of Table I. Several most grave errors, however, are here involved. To get at the true relative calorific effects of the above gases, when burned in the open air, in heating water below its boiling point, deductions must be made, not only for the specific heats of the products of combustion of the gas, but also, more important still, for the specific heat of the nitrogen of the air required to burn the gas. In fact, when we consider that for each volume of oxygen required to burn a given volume of a gas, about four volumes of nitrogen must be heated up to the temperatures of the flame, it becomes easy to conceive, what is actually the fact, that within certain limits the waste of heat due to this cause alone counterbalances altogether the advantage that would be supposed to result from the crowding of combustible matter into so condensed a form as in the illuminating hydrocarbons. The result of our investigations of this matter is that the heating powers of the flames of pure hydrogen and pure olefiant gas, even when used to the greatest advantage, to heat water below its boiling point, are almost or quite identical.

In this discussion we have occasion to use the numbers representing the specific heats of but three gases, the three, namely, which remain after complete combustion, *steam*, *carbonic acid* and *nitrogen*; as we must assume that in the hottest and most luminous zone or shell of the flame, there is no oxygen in excess to be heated. These three numbers are, according to Regnault's latest determinations, for equal weights of

Steam	0·4805
Carbonic acid	0·2163
Nitrogen	0·2438
(*Liquid water* being	1·0000)

This means that the amounts of heat which would raise one pound of water and steam *to the same degree* are in the ratio of 0·4805 for the pound of steam, and 1 for the pound of water.

2. *Calculation of the calorific effects of Hydrogen burning in air.*—Let us take, first, the simplest case possible, that of hydrogen with exactly the right admixture of pure oxygen to burn it, which, by Table I, develops a total heat of 34462° C.; that is, would heat a certain weight of *liquid* water to this temperature. In order to find the actual amount of heat contained in the products of combustion, we must first take into account the fact that one pound of hydrogen burns to *nine pounds* of steam, and then obtain the ratio between the above number, 34462, and the amount of heat necessary to heat nine times the weight of steam; that is, nine times the specific heat of steam. Calling the total residual heat in the produced steam x, we have the simple proportion:

$$9\times(\text{sp. heat of steam}=0{\cdot}4805):34462^\circ::(\text{sp. heat of water}=1):x$$

$$\text{or, } x=\frac{34462^\circ}{4{\cdot}3245}=7969^\circ\text{ C.}^*=14376^\circ\text{ F.};$$

a number which, we may add, represents the *maximum* of heat capable of being imparted theoretically *to liquid water* by the flame of Hare's oxyhydrogen blow-pipe.

Still, we have by no means here the actual temperature of the free or open flame of Hare's blow-pipe, which is generally lower than this figure; as we have not yet taken into account the latent heat, or heat of vaporization, of the 9 lbs. of steam formed. The Centigrade temperature necessary to convert 1 lb.

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* Bunsen, in his Gasometry (English edition of 1857, p. 242), gives this number as 8061° C., the difference being due to his using a different number for the specific heat of steam, namely, 0·475, apparently an earlier determination of Regnault. Bunsen makes here the singular oversight of regarding this figure as the temperature, when "the gases can freely expand, as is the case in an open flame," overlooking the correction necessary in this case for the *latent heat of steam of combustion*, as is explained in the text above. This oversight has doubtless been corrected by the distinguished author, but we have been unable to ascertain where the correction has been published.

of water into steam being 537°; to get the actual temperature of the oxyhydrogen flame, we must modify the above equation, so that

$$x=\frac{34462°-(9\times 537°)}{4{\cdot}3245}=6851° \text{ C.} = 12364° \text{ F.};$$

which is the temperature actually possible in the flame of the compound blow-pipe, *were the combustion instantaneous and complete.*

When hydrogen gas burns *in air*, however, as has been before stated, another deduction of enormous amount must be made from the above figures, due to the heat required to expand the nitrogen. This is obtained simply by adding to the divisor, as above, the weight of the nitrogen of the air employed, multiplied by its specific heat. The weight of the nitrogen in air= 3·318 times the oxygen; so that the latter of the above equations becomes

$$x=\frac{34462°-(9\times 537°)}{4{\cdot}3245+(8\times 3{\cdot}318\times 0{\cdot}2438)}=2744{\cdot}5° \text{ C.}=4972° \text{ F.}$$

We have here a full explanation of the extraordinary loss of power in illuminating gas by admixture of air, which we have discussed elsewhere.* The nitrogen of such air is not merely a diluent, or even a mere deductive quantity; its specific heat is an actual *divisory* function in diminishing the flame-temperature.

This, then, is the actual temperature to which the flame of hydrogen gas burning in the atmosphere might attain to, supposing complete and instantaneous combustion. If it is desired to obtain instead, the total calorific effectiveness, as in heating water below its boiling point—in which case the latent heat of the steam of combustion becomes also available—the above expression is changed by simply omitting the subtrahend in the numerator:

$$x=\frac{34462°}{4{\cdot}3245+6{\cdot}4714}=3192° \text{ C.} = 5778° \text{ F.}$$

3. *Calculation of the calorific effect of carbonic oxyd burning in air.*—As the product of combustion is here solely carbonic acid, no latent heat of steam enters, and the calorific effectiveness is the same, under all circumstances, in air. In the numerator we substitute of course the calorific equivalent of one volume of carbonic oxyd from Table I; and in the denominator, for the specific heat of 9 lbs. of water, that of 22 lbs. of carbonic acid, being the weight of the latter formed by the combustion and combination of 14 lbs. of carbonic oxyd, with 8 lbs. of

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* This Journal, II, xlviii, 40.

oxygen. The number for the specific heat of nitrogen is the same as before; and the equation is now

$$x=\frac{33642^\circ}{(22\times0{\cdot}2163)+6{\cdot}47=11{\cdot}23}=2996^\circ\text{ C.}=5425^\circ\text{ F.}$$

4. *Marsh gas and Olefiant gas.*—In these two cases, we have as products of combustion both carbonic acid and water; and, therefore, when the calorific effects are sought for, we have not only the latent heat of steam entering as a subtrahend into the numerator; but also into the denominator, as divisors, all three of the specific heats of steam, carbonic acid, and nitrogen.

Then, as 8 lbs. of marsh gas consume 22 lbs. of oxygen, and produce 22 lbs. of carbonic acid, and 18 lbs. steam; and as 14 lbs. of olefiant gas consume 48 lbs. of oxygen, producing 44 lbs. of carbonic acid, and 18 lbs. of steam, the equations for the calorific powers of their flames in air become—

For marsh gas:

$$x=\frac{104504^\circ-(18{\cdot}\times537^\circ)}{(18{\cdot}\times{\cdot}4805)+(22{\cdot}\times{\cdot}2163)+(32{\cdot}\times3{\cdot}318\times{\cdot}2138)}=2414^\circ\text{ C.}=4386^\circ\text{ F.}$$

And for olefiant gas:

$$x=\frac{166012^\circ-(18\times{\cdot}537^\circ)}{(18{\cdot}\times{\cdot}4805)+(44{\cdot}\times{\cdot}2163)+(48{\cdot}\times3{\cdot}318\times{\cdot}2438)}=2743^\circ\text{ C.}=4970^\circ\text{ F.}$$

When the deduction for the latent heat of the steam of combustion is not made, the results in these two gases are considerably higher, as will be obvious from mere inspection of the formulæ.

We shall now give, in tabular form, all the results of our calculations of the calorific powers when burning in the air, of the four gases we have to deal with.

TABLE II.

For equal volumes of the gases burning in air.	Calorific effects in heating liquid water.		Calorific effects, above 100° C.	
	Centigrade degrees.	Fahrenheit degrees.	Centigrade degrees.	Fahrenheit degrees.
Hydrogen (sp. heat HO=·4805)	3192°	5778°	2744°	4971°
Hydrogen (sp. heat HO=·4750)	3204°	5799°	2755°	4991°
Hydrogen (mean	3198°	5788°	5749°	4980°
Carbonic oxyd	2996°	5425°	2996°	5425°
Marsh gas, (sp. heat HO=·4805).	2660°	4820°	2414°	4386°
Olefiant gas, " "	2916°	5481°	2743°	4970°

5. *Computation of calorific effects of mixed gases.*—The above table renders simple the calculation of the calorific effects of any given gaseous mixture, whose centesimal composition is known. It is only necessary to obtain the sum of the multiples

of the percentage of each component gas into its calorific capacity, as given in this table, and divide by 100.*

To serve as examples of these modes of computation we here cite, in tabular forms, the results of some analyses of a number of gaseous mixtures, made by us during the winter of 1868–9. [These analytical results, it may be remarked, possess points of novelty and importance. both scientific and practical, which will bring them up again hereafter, in other connections. They are here placed on record.]

Table III, gives the results of two analyses of gaseous mixtures obtained by passing steam *superheated to incandescence* upward through a mass of *anthracite coal* heated to a high degree in a clay retort of a novel construction, according to what is now known as the "Gwynne-Harris," or American Hydrocarbon Gas System. In this table the results are calculated without carbonic acid and sulphuretted hydrogen, which, with traces of nitrogen and sometimes of oxygen, are found in the unpurified anthracite gas.

TABLE III.

	No. 1.	No. 2.	Mean.
Hydrogen	60·43	59·32	59·87
Carbonic oxyd	35·44	37·14	36·29
Marsh gas	4·13	3·54	3·84
	100·00	100·00	100·00

In Table IV, column 1 gives the results of the analysis of the street gas served out at this period by the New Haven Gas Light Company; made from Westmoreland coal enriched with about six per cent of Albertite. Column 2, the mean of four analyses of the completed hydrocarbon gas made by us at Fair Haven during the same time, by combining gas from the same Westmoreland coal (with 10 per cent of Albertite) with about half its volume of the Anthracite gas. Columns 3 and 4 are obtained from 1 and 2 by centesimal reduction, after deduction of the illuminant ingredients, being what we propose to designate as the non-illuminating *substrata* of illuminating gases.

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* Prof. Bunsen, in the masterly discussion of the subject presented in his *Gasometry*, not having in view the exact object we propose. has used a train of reasoning and a mode of formulation of some complexity, to follow which requires some little mathematical skill—part of his object having been to construct a formula so general and comprehensive as to cover the direct computation, from any gaseous mixture independently of its special calorific intensity. We have here aimed at so simplifying as to bring the whole subject within the capacity of all. Our above tabulation of the individual gaseous components, as a starting point, seems to us to accomplish this most effectually, so far as illuminating gases are concerned.

Table IV.

	(1.) New Haven City Gas.	(2.) Fair Haven Hydrocarbon Gas.	(3.) Substratum of New Haven Gas.	(4.) Substratum of Fair Haven Gas.
Hydrogen	43·58	46·77	46·79	50·27
Carbonic oxyd	2·14	9·56	2·31	10·27
Marsh gas	47·42	36·71	50·90	39·46
Illuminants	6·86	6·96	-----	-----
	100·00	100·00	100·00	100·00

Table V gives the results of the computation, from our formulæ, of the calorific powers of these five gaseous mixtures, for communicating temperatures both above and below that of aqueous ebullition. We should remark that we have here been obliged to regard the volumes of illuminant hydrocarbons as representing olefiant gas solely; both because we have no certain data as to their real nature, and particularly because, if we actually knew, or should assume, the nature of the hydrocarbon vapors present, still we have no experimental calorific equivalents, as we have for olefiant gas, from which to start in such a computation. We have reason to believe nevertheless that the errors thus introduced are not important in amount.

Table V.

	Weights of water equally heated below boil'g; by equal volumes.	W'ts of water equally heated above boil'g; by equal volumes.	1st column reduced to N. Haven gas =100.	2d column reduced to New Haven gas =100.
Anthracite gas	3100	2823	104·2	109·2
Substratum of the New Haven street gas	2917	2581	98·1	99·6
Substratum of the Fair Haven hydrocarbon gas	2962	2640	99·6	102·0
New Haven gas; with the illuminants assumed=olefiant	2974	2592	100·0	100·0
Fair Haven gas; with the illuminants assumed=olefiant	2959	2647	99·5	102·1

3. *Conclusions.*—Some of the practical conclusions to which we are led, by the results of the above investigations, are somewhat remarkable, so that we feel diffident regarding them. It is however always safe to follow the leading of truth, however astray she may conduct us from our preconceived notions.

From Table II it is apparent:

1. That of all known gases, the highest calorific effects, under ordinary atmospheric conditions, are obtainable from *carbonic oxyd;* whose calorific value, above 100° C., is about 3000° C.

2. That, in absolute calorific value, below 100° C., in the atmospheric medium, *hydrogen* surpasses its volume of any other gas; giving a temperature of about 3,200° C.

3. That for all modes of application—that is, for producing both high and low temperatures—the total maximum calorific effectiveness of carbonic oxyd is a *constant quantity.*[*]

4. Compound condensed submultiple volumes of hydrogen, like that in marsh gas, have much *less* total calorific value in air than their volume of free hydrogen.

5. Condensed compound submultiple volumes of gaseous carbon, like that in olefiant gas, have no greater total calorific value, in air below 100° C., than their own volume of carbon gas in the form of carbonic oxyd; while above 100° C. their value is even considerably less.

ART. XXXVII.—*On the Laurentian and Huronian Series in Nova Scotia and New Brunswick;* by HENRY YOULE HIND, M.A.

CONTENTS:—1. Introduction; 2. General Sketch of the Distribution of the Huronian and Laurentian Series in Nova Scotia; 3. Sequence of Formations—The Upper Silurian; 4. The Lower Silurian; 5. The Gold-bearing Rocks; 6. The Cambrian or Huronian Series; 7. The Laurentian Series; 8. The *Eozoön Canadense;* 9. Cape Breton Island.

1. *Introduction.*

THE descriptions contained in this paper, so far as they relate to Nova Scotia, are in the main the results of observations during the summers of 1868 and 1869, while making geological surveys for the Nova Scotian government, in the gold districts of Waverley and Sherbrooke. The comparisons with New Brunswick are based on my official Report on the geology of that Province;[†] and the references to Cape Breton, when not otherwise stated, are from MS. notes of explorations in that Island during 1866.

The object of this paper is to show that two gneissoid series, supposed to be the equivalents of the Huronian and Laurentian of Sir W. E. Logan, are exposed over very large areas in Nova Scotia, the Island of Cape Breton and in New Brunswick.[‡]

The boundaries of the series have been traced through parts of Halifax, Hauts and Guysborough counties in Nova Scotia. In New Brunswick [‡] numerous narrow belts extending from the bay of Chaleurs to the boundary line between New Brunswick and Maine are supposed to represent the Laurentian and are described in my Report on New Brunswick, published in 1865 (pp. 42–52).

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[*] Metallurgists, especially, will appreciate the suggestive import of the truth, presented under the first and third heads; here enunciated, as we think. for the first time. It is to be noted that all the above effects belong to the *maximum* kinds and, of course, reach their development only under the most favorable conditions in each case respectively.

[†] Preliminary Report on the Geology of New Brunswick: Fredericton, 1865.

[‡] In southern New Brunswick Prof. Bailey and Mr. Mathew have discovered and described rocks of Laurentian and Huronian age. "Observations on the Geology of Southern New Brunswick:" Fredericton, 1865. Also see an able paper by Mr. Mathew in the Journal of the Geological Society of London for 1865.

Geological maps of Nova Scotia were published by Dr. Abraham Gesner in 1836,* by Dr. Dawson in 1865,† and in 1868,‡ and by Sir William E. Logan in 1865§ and in 1869.‖

Sir William Logan states, in the introduction to his Atlas of Maps and Sections, that for the geology of Nova Scotia "a manuscript map by Dr. J. W. Dawson, compiled from his own resources, and those of Messrs. B. Brown and H. Poole, has been the source of information." Hence, in making the necessary comparisons between the subject of this paper and the published descriptions and maps of Nova Scotia, I shall have to refer almost exclusively to Dr. Dawson's map of 1868, accompanying the 2d edition of his beautiful work on Acadian Geology.

In a Preliminary Report¶ on the supposed Laurentian of Nova Scotia, I have quoted some passages from Dr. Dawson's work, especially the explanation to the geological map, in which the uncertainty of the boundaries of formations, and the doubtful age of some strata, is adverted to. The recognition of a very large gneissoid area in Nova Scotia, supposed to represent two series not hitherto described as occurring in the Province, will enable some of the changes in part anticipated by Dr. Dawson to be foreshadowed with some degree of accuracy; and it is proper to repeat here Dr. Dawson's first paragraph of the "Explanations to the Geological Map":— "The map in this edition, though greatly improved, is still to be regarded as merely a rude approximation to the truth, and the coloring in many places, more especially in the interior, remote from the coast lines, is little more than conjectural."

In various parts of "Acadian Geology" reference is made to rocks which were suspected by Dr. Dawson to be older than the Lower Silurian slates and quartzites. (See particularly page 620, Acadian Geology, 2d edition). These will probably now be classed with the Huronian series; and the massive porphyroid granitoid gneiss on which they rest, with the Laurentian.

Dr. Sterry Hunt visited Nova Scotia in November, 1867 "for the purpose of making some observations on the gold-bearing rocks of that Province, with the view of comparing them with those of other parts of the dominion, and also of obtaining such information as might be useful in the event of a geological survey of Nova Scotia itself."

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* Remarks on the Geology and Mineralogy of Nova Scotia: by Abraham Gesner; Halifax, 1836.

† Acadian Geology, 1st edition.

‡ Acadian Geology, 2d edition; Macmillan & Co., London, 1868.

§ Atlas of Maps and Sections; Montreal, Dawson Brothers, 1865.

‖ Geological Maps of Canada and the adjacent regions, 1869: London, Edward Standford.

¶ Preliminary Report on a Gneissoid Series underlying the Gold-bearing Rocks of Nova Scotia, and supposed to be the equivalent of the Laurentian System; Halifax, N. S., January 5, 1870.

Dr. Hunt's stay in the Province was limited to four weeks in the months of November and December, and in the descriptions which he has given in his official report to Sir W. E. Logan* he quotes the following as the principal sources of information about the geology and mineralogy of Nova Scotia: Dr. Dawson's Acadian Geology, 1st ed.; Mr. Poole's Report, 1862; Mr. J. Campbell's Reports, 1862 and 1863; Professor B. Sillman's Reports on Tangier, Waverley and Montague Gold Fields, 1864.

Allusion is made in the Atlas of Maps and Sections of the Geological Survey of Canada to the opinions expressed in my Report on New Brunswick that much of the granites of that Province consist of altered sedimentary strata. "Much of what in Nova Scotia, New Brunswick and Maine, is represented on the map as intrusive rock (chiefly granite) probably consists of paleozoic strata altered *in situ* as already suggested by Dawson and Hind. See the latter's Report on New Brunswick, 1865, page 50." (Atlas of Maps and Sections, Geological Survey of Canada, 1865, page 20).

The remarkable similarity which exists between the rocks constituting part of the great gneissoid axis of New Brunswick and the gneissoid series now described of Nova Scotia, coupled with the equally marked similarity which obtains between the paleozoic strata resting on these gneisses in both Provinces (on the Nipisiquit in N. B.), satisfies me that they are of the same age.

2. *General Sketch of the Distribution of the Huronian and Laurentian Series in Nova Scotia.*

In this general sketch of the old gneissic rocks of Nova Scotia, they are grouped together. In succeeding paragraphs it is stated where the Huronian or Cambrian gneiss and schist rest on the old Laurentian gneiss as far as known.

The outcrop of the Laurentian and Huronian rocks in Halifax and Hauts counties, N. S., has been traced from a point seven miles west of Windsor on the Basin of Mines (Bay of Fundy) to the Atlantic coast at Cape Sambro, a distance of forty-eight miles in an air line, and sixty-four miles on the margin of the outcrop. This is the northeasterly boundary of an immense area of the same rock series which from information hereafter noticed I believe continues with variable breadth to the Tusket Islands, near Yarmouth, a distance of about one hundred and thirty-five miles in an air line.

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* Report of Dr. T. Sterry Hunt, F.R.S., on the Gold Region of Nova Scotia; Ottawa, 1868.

The area above described forms the western development of the Laurentian and Huronian gneisses and schists in Nova Scotia. It is separated from the eastern development by a narrow but profound valley occupied by Silurian strata, whose least breadth is eight miles. The outcrop of the southwestern boundary of the eastern development is not continuous, but embraces two areas near Grand and Fletcher's Lakes, and an area of unknown but very considerable and of variable width, stretching from Parker's Lake, with some narrow interruption of Silurian strata which have escaped denudation, probably all the way to the Strait of Canso and Chedabucto Bay, a distance of one hundred and twenty miles in an air line: so that, generally speaking, a Laurentian axis, capped here and there by strata of Huronian age, occupies Nova Scotia, certainly in one place at least forty-eight miles in breadth.

The existence in Nova Scotia of all formations from the Trias to the Laurentian, with the exception of the Permian,* may now be considered as established. Whether the rocks noticed in the foot-note are of Permian or Triassic age I am not able to say, but judging from the descriptions given of the relations of the Triassic to the Carboniferous by Dr. Dawson, I have hitherto considered small unconformable patches in Cape Breton as of Triassic age, and regarded them as the continuation of the Prince Edward Island series, resting on Lower Carboniferous rocks.

The entire series from the Lower Carboniferous downward, with the exception of the Devonian, is passed over in a journey by rail from Windsor to Halifax, in a distance of fourteen miles. The Devonian occurs at Nictau, and rest there on Upper Silurian slates† which probably sweep round the Falmouth mountains and connect with the Upper Silurian near Windsor.

3. *Sequence of Formations—The Upper Silurian.*

On the St. Croix river, eight miles from Windsor, the Lower Carboniferous grits are seen to rest on Upper Silurian argillites.

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* In Cape Breton, at Jumping Brook, seven miles northeast of Chetican Island on the Gulf coast, and at Trout Brook, five miles northeast above Chetican, mottled sandstones and conglomerates rest unconformably on white and mottled sandstones and bituminous shales, supposed to be of Lower Carboniferous age. These latter rest unconformably, the first on red metamorphic rocks, the second are seen in close proximity to red, green, and black corrugated schists, supposed to be of Lower Silurian age. In Dr. Dawson's tabular view of the geological formations of the Acadian Provinces (page 19, Acad. Geology) the Permian is stated to be "not represented unless by the lower part of the sandstones of Prince Edward Island." May not the unconformable patches in Cape Breton be a continuation of these Prince Edward Island deposits?

† Page 498 Dawson's Acadian Geology, 2d edition.

The grits dip N. 60° W. ∠5°; the argillites S. 70° E. ∠50°. The argillites are generally very fine grained, green internally, but red on weathering; they are interstratified with thin beds of quartzites, and have a breadth near the railway of 170 chains; their dip being tolerably uniform, and no repetitions visible; their thickness may approach 9,000 feet.

The argillites resemble in every particular argillites seen on the Tobique in New Brunswick, and there associated with their calcareous beds, holding *Favosites Gothlandica.* These are described in my report on New Brunswick.*

Toward the upper portion of the series the argillites are conformably succeeded by bluish-black slates, holding cubical crystals of iron pyrites, and resembling roofing slates. A similar change occurs on the Tobique in New Brunswick. These bluish-black slates are exposed to a great extent on the Ardoise hill range, N. S.

4. *The Lower Silurian.*

A good exposure of the blue-black Upper Silurian slates is visible at the 13th telegraph post south of Ellerhouse station on the Halifax and Windsor Railway, dipping S. 20° E.; and at the 38th telegraph post brilliant micaceous schists, interstratified with black corrugated slates, dip N. 40° E., the intermediate space being covered with boulder drift. The brilliant micaceous schists, as well as the corrugated slates, are much contorted, and overlie conformably the gold-bearing quartzite series.

The micaceous schists and the corrugated black slates cannot be distinguished from similar schists and slates described in my New Brunswick Report as occurring on the Nipisiquit, and near Dumbarton station on the New Brunswick and Canada Railroad (pp. 147 and 154) where they are associated with the red slates supposed to be the uppermost member of the Quebec group of Sir W. E. Logan. The black corrugated slates contain conformable auriferous beds of quartz, but no mining is at present carried on in these deposits. They are about 3,000 feet in thickness.

5. *The Gold-bearing Rocks.*

The known gold-bearing rocks of Nova Scotia consist of quartzites, sandstones and grits, interstratified with argillaceous slates, and thin conformable and intercalated beds of auriferous quartz. The portion has an ascertained thickness of 9,000 feet, and, between the base and a vertical thickness of about 3,000 feet from the summit, the thin beds of quartz are found yielding gold, and are worked in different districts in the Province, so that a mass of strata having a thickness of six thousand

* Page 131.

feet, or more than a mile, yields gold from quartz beds of contemporaneous age with the quartzites and slates with which they are interstratified; and it is from these quartz beds that the greater part of the gold of Nova Scotia is obtained.* The total thickness of the gold-bearing series, including the corrugated black slates and the brilliant micaceous schists, is about 12,000 feet.

6. *The Cambrian or Huronian Series.*

In some parts of Nova Scotia the known gold-bearing rocks rest unconformably on a gneissoid series, which are well exposed to view on the Halifax and Windsor Railway, between Stillwater and Mount Uniacke Station, near the village of Sherbrooke, in Guysborough county. This series is composed of beds of gneiss, interstratified with micaceous schists, schist conglomerate, beds of true quartzite, and grits. The gneiss is sometimes porphyritic, and the upper beds are almost always conglomerate, holding pebbles and masses of schist, grits, and conglomerates, which are found in this series. Some of the gneissic strata are garnetiferous, as are also the micaceous strata. Between Stillwater and Mount Uniacke stations, the general strike of the Lower Silurian is N. 80° E., dip N. ∠ 80°; the prevailing strike of the Huronian is S. 50° E., the railroad track running for two or three miles on the strike of these rocks. Near to their junction with the Huronian, the Silurian schists are more altered than when remote, and hold numerous crystals of andalusite. This series has been very extensively denuded; and in some places Silurian, Huronian and Laurentian are seen in close juxtaposition. The thickness at Sherbrooke is about 1,300 feet.

When the preliminary report already referred to was in the hands of the printer, I satisfied myself by repeated observations, that a very decided unconformability existed between these supposed older strata and the gold-bearing series; also between the older strata and the Laurentian; and I have succeeded in discovering in various places:

1st. The unconformable contact of the Lower Silurian gold-bearing strata with the underlying gneissoid and schistose series;

2d. The unconformable contact of this gneissoid and schistose series with the old porphyritic gneiss which I had before described as Laurentian;

3d. The unconformable contact of the gold-bearing series with the old Laurentian gneiss, showing the absence of the intermediate gneissoid series, or the Huronian.

These several points of contact are visible at both extremities of a patch of Huronian strata about four miles broad, overlying the Laurentian on the Windsor and Halifax railway, commencing one mile or thereabouts southeast of New Stillwater station, and terminating at Uniacke's second lake, and more than half a mile west of Mount Uniacke station.

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* See my report on the Sherbrooke District for a description of the auriferous lodes, distinguishing between contemporaneous and intercalated lodes.

7. *The Laurentian Series.*

The rocks last described are visible, as already stated, in unconformable contact with a coarse porphyroid, granitoid gneiss near Stillwater station. The strike of the granitoid gneiss is N. 10° W.; dip W., at an angle of about 48 degrees. Five miles farther south, and within a third of a mile of Mount Uniacke station, the Silurian quartzites rest on the Laurentian gneiss—the quartzites having a strike N. 75° W., and the old gneiss N. 20° W. Between Stillwater and Mount Uniacke, the Huronian series rests on the old gneiss, and the Silurian on the Huronian; but north of Stillwater and south of Mount Uniacke the Silurian strata are in contact with Laurentian gneiss, and so continue until another patch of Huronian is reached,—this last named series appearing to cover comparatively small areas in the great Laurentian valley between Halifax and Windsor; but in the more western counties it is exposed, I have reason to believe, to a very considerable extent.

In the county of Guysborough the gold-bearing rocks at Sherbrooke rest on the Huronian, which again is seen close at hand in contact with the old Laurentian gneiss. In the middle and eastern part of Nova Scotia the thickness of the Huronian does not appear to be very considerable, but no complete section has yet been crossed except at Sherbrooke. Between Halifax and Windsor the Lower Silurian series occupies a great valley or synclinal fold in the old Laurentian gneiss. The average breadth of the valley is nine miles, and its depth must exceed two miles. Its general course is northwest (true); and the gold districts of Mount Uniacke and Hammond's Plains are arranged on its western boundary; and those of Lawrencetown, Montague, Waverley and Renfrew on the eastern boundary of the valley, occupying crowns of anticlinals, which have a general northeast-by-east direction.

In one part of the county of Guysborough, the Laurentian, with a narrow band (as far as known) of Huronian, forms a nucleus, having an area of about 120 square miles. Around this nucleus the gold districts of Cochrane's Hill, Sherbrooke, Wine Harbor, Isaac's Harbor and County Harbor, are arranged, also on the crowns of anticlinals, which have a general easterly and westerly direction.

The profound Silurian valley between Halifax and Windsor, divides the Atlantic portion of Nova Scotia proper into two distinct geographical areas, in both of which the old porphyroid

Laurentian gneiss forms the axis around which the Huronian and Silurian series are arranged; but with respect to the precise limits of their formations little is known to the west or to the east.

From Dr. Dawson's published maps and descriptions, Mr. Poole's manuscript maps of the western part of the peninsula, and the numerous rock specimens collected by that gentleman, and placed at my disposal by the Commissioner of Mines, coupled with valuable information derived from other sources, I infer that this coarse Laurentian gneiss extends in one unbroken sheet of strata, but of variable width, a distance of ninety miles west of Windsor, and occupies a large portion of the uninhabited wilderness in that part of the Province. Much of the gneiss, schist and mica slate, seen by Mr. Poole, and described in his Report, and illustrated by his specimens, together with the gneiss, mica schist, and chloritic beds alluded to by Dr. Dawson, and by that geologist long spoken of as probably older than the Lower Silurian, are probably representations in many instances of the Huronian in the district where they occur.

8. *The Eozoön Canadense.*

In the autumn of 1868, Dr. Honeyman, then engaged on the geological survey of Canada, discovered on the Gulf coast of Nova Scotia, in the Arisaig district, and near the base of the Antigonish mountains, syenites, diorites, and crystalline limestone, with serpentine, which he then supposed were of Laurentian age, as he informed me subsequently to the publication of my Preliminary Report on the Nova Scotia Laurentian. Specimens were sent to Montreal for examination, and instructions were given by Dr. Hunt, who also shared Dr. Honeyman's opinion, to the lapidary, to prepare sections of the serpentinous rock, for microscopic examination. By some mischance this was neglected, and the specimens remained unexamined and indeed forgotten, until quite recently, as Dr. Hunt informs me, under date Feb. 3d, 1870. When submitted to the microscopic test, the *Eozoön Canadense* was distinctly seen, and Dr. Dawson has confirmed the observations.

This fortunate occurrence has a two-fold bearing. It not only affords most satisfactory testimony to the existence in Nova Scotia of the Laurentian limestones, but it enables geologists to recognize the truth of Dr. Honeyman's opinions, although by an accident these opinions were not made known or confirmed until after the publication of my Report. It is with pleasure I take advantage of the permission Dr. Hunt has given me in the letter above referred to, to use the information respecting the recent discovery of the *Eozoön* in the Antigonish limestones as I may think fit; and I am rejoiced that the publication of my official

Report announcing the series over a wide extent of country has resulted in the just though tardy recognition of the correctness of Dr. Honeyman's views with reference to the age of the limestones and diorites of Arisaig.

In other parts of Nova Scotia the Laurentian is yet known only in the form of coarse porphyroid gneiss; but the area occupies is a lake and forest wilderness frequented only by the lumberman and the hunter.

The descriptions given by Sir W. E. Logan of a similar rock in the Laurentian of Canada apply exactly to the characteristic strata in Nova Scotia. "The coarse-grained granitoid and porphyritic varieties, which often form mountain masses, sometimes have, at first sight, but little the aspect of stratified rocks, and might be mistaken for intrusive granites."*

9. *Cape Breton Island.*

The undoubted recognition of the Laurentian limestones and serpentines in the Arisaig district of Nova Scotia is of great significance, and an important link in the chain of evidence which will ultimately establish the correctness of my supposition that a great belt of rocks, of the age of the Laurentian series, is exposed at short intervals apart from Newfoundland in latitude 51° to Shelbourne in Nova Scotia, latitude 44°.†

In Cape Breton Island I saw in 1866 the black corrugated slates forming the summit of the gold-bearing series in Nova Scotia, about five miles north of Chetican Island on the Gulf coast; and on the Mackenzie river, near Red Cape, I crossed part of a great gneissoid series.

In other parts of Cape Breton I have seen similar gneisses as, for instance, near the mouth of North river, St. Ann's Bay, and on the peninsula opposite Baddeck.

Dr. Honeyman informs me that he considers the gold-bearing rocks of Middle River in Cape Breton to be of the same age as those of Nova Scotia. Hence it becomes more than probable that a large portion of the area colored by Dr. Dawson to represent Upper Silurian in the northern part of the Island is occupied by rocks of Huronian and Laurentian age.

Around the outcrops of these rocks, wherever the Lower Silurian reposes on either one or the other, we may expect to find new workable gold deposits occupying the crowns of anticlinals which intersect the country with remarkable uniformity.

Hence we may legitimately anticipate that the correct mapping of these ancient rock series may be attended with considerable economic advantage to the Province.

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* Page 587, Geology of Canada, 1863.

† Page 11, Preliminary Report on a Gneissoid Series in Nova Scotia, by the author.

ART. XXXVIII.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School.* No. 10.—*On certain Double Sulphates of the Cerium Group;* by CHARLES H. WING.

SOME years since Dr. Wolcott Gibbs found that, if to a solution of crude ceroso-ceric sulphate, containing also lanthanum and didymium, solutions of luteocobaltic or roseocobaltic sulphate be added, crystalline precipitates are formed, even from quite dilute solutions, insoluble in an excess of sulphuric acid. Different preparations of these have since been analyzed by different persons in this laboratory without obtaining rational formulas or accordant results. I therefore, at the suggestion of Dr. Gibbs, undertook the investigation of these compounds.

Swedish cerite was first heated to redness then quenched in water, after which, it was readily pulverized. The powdered mineral was heated with sulphuric acid, with tumefaction and solidification of the mass; this was pulverized, more sulphuric acid added, and the mixture placed in a crucible, which was then set in a deep furnace and supported by a fire-brick in such a manner as to be raised above the fire; it was thus heated by reverberation, to low redness, for four or five hours.

The resulting mass was of a reddish tint and crumbled readily to powder; this was diffused gradually in cold water and the solution filtered. The filtrate was precipitated by sulphydric acid,* filtered, oxydized by chlorine and precipitated by oxalic acid. The resulting oxalates, well washed and dried, served as the material for the following work.

To prepare perfectly pure cerium from these oxalates I used the method employed by C. Wolf. [This Journal, vol. xlvi, May, 1868.] The oxalates were decomposed by heat with constant stirring to favor oxydation. The resulting brown powder was dissolved in nitric acid; the solution concentrated, and precipitated by pouring it into a large quantity of boiling water containing 2 c. c. of sulphuric acid to the liter. The supernatant liquid had no trace of yellow, but an amethystine tint, and was reserved for the preparation of lanthanum and didymium.

The basic sulphate thus precipitated was washed by decantation with hot water, until the amount of soluble substance that remained in the precipitate had been reduced, (after Bunsen) to $\frac{1}{10,000}$.

The precipitate was then dissolved in nitric acid, the solution concentrated and precipitated by pouring it into a large quantity of boiling water. This precipitate was washed as before, redissolved, again precipitated, and this process repeated till six successive precipitations as a basic salt had been effected. The final precipitate was dissolved in sulphuric acid, reduced by sulphurous acid, the solution evaporated and the excess of acid expelled by heating in, what I shall, for brevity, term a "radiator." This is a crucible of sheet iron in form like the ordinary porcelain crucible, midway in which is fixed a triangle of platinum wire by which a smaller crucible may be supported so as to leave a space of about 2 c. m. intervening between the walls of the two crucibles. The substance being placed in the inner crucible and the heat of a Bunsen gas lamp applied to the outer one, a very uniform heat may be maintained, variable at will, to a temperature considerably above the boiling point of sulphuric acid.

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* This was the process of Marignac, modified by Bunsen and others.

The neutral cerous sulphate thus obtained was dissolved in cold water, filtered, and crystallized by slow evaporation on the water bath.

A saturated solution of this salt was placed in a 12-inch saccharometer tube before a powerful spectroscope, and light, condensed by a "bulls eye" lens and transmitted through this tube, showed no traces of didymium.

The cerium also from the mother liquor from the last precipitation as basic salt, subjected to the same rigid test proved free from didymium.

The first precipitation of basic sulphate was of a dirty buff, every succeeding one lighter in color, the last three being almost white.

The cerous oxalate from both precipitate and mother liquor of the last precipitation was of a snowy whiteness, with no trace of amethystine tint by reflected light, and gave on ignition a white ceroso-ceric oxyd.

The cerous sulphate, the preparation of which has just been described, was, after repeated crystallizations, subjected to analysis, as follows. Portions of the salt dried at 100° C. were heated in the "radiator" until a weight was obtained which remained constant at an increased temperature. The anhydrous salt dissolved in a considerable amount of water was precipitated, hot, by a strong boiling solution of oxalic acid, and allowed to stand for twenty-four hours.

For the reduction of the weight of the ceroso-ceric oxyd, obtained by ignition of the oxalate to the corresponding amount of cerous oxyd, the experiments of Jegel* and Wolf† made in Bunsen's laboratory, give the following factors.

Jegel,	·9504	·9507	----	----	----	mean	·95055
Wolf,	·9507	·9518	·9494	·9509	·9518	mean	·95089

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* Annalen d. Chem. u. Pharm., cv, 1858. † This Journal, vol. xlvi, May, 1868.

As the mode of preparation was that employed by Wolf, I have used the factor deduced from the mean of his analyses.

I. 1·2885 grms of substance gave ·1707 $H_2\Theta$ and ·6732 $Ȼe_3\Theta_4$= ·6400 $Ȼe\Theta$.

II. 1·4090 grms. " " " ·1857 $H_2\Theta$ and ·7372 $Ȼe_3\Theta_4$= ·7009 $Ȼe\Theta$.

These results give the following percentages, to which I have, for the sake of comparison, appended those obtained by Wolf from his purest cerous sulphate.

	I.	II.	Wolf.	
$Ȼe\Theta$	57·28	57·31	57·294	57·310
$S\Theta_3$	42·72	42·69	42·706	42·690
equivalent corresponding	45·64	45·69	45·66	45·69

Taking the mean of these 45·67 as the true equivalent of cerium or 91·34 as the atomic weight and the formula

$$3(Ȼe, S\Theta_4)+5H_2\Theta$$

we have

	I.	II.	Wolf.		Calculated.
$Ȼe$	42·26	42·35	-----	-----	42·03
$S\Theta_4$	44·50	44·47	-----	-----	44·17
$H_2\Theta$	13·24	13·18	13·318	12·803	13·80

From the results obtained we can assume the cerium used in this investigation to be as pure as any yet prepared.

The following method was employed in the analysis of these compounds. The substance dried at 105° C. was heated in the "radiator" to decompose the ammonio-cobalt base. The residue was dissolved in a considerable amount of boiling water, and precipitated by a hot solution of oxalate of ammonia containing free oxalic acid. Care must be taken to add so much oxalate as shall serve to keep the cobalt in solution as a double salt. After standing for twelve hours or more, the solution was filtered off, the precipitate ignited, and weighed as ceroso-ceric oxyd of a composition as before stated. The blowpipe failed to detect cobalt in the ignited precipitate. The filtrate containing the cobalt was supersaturated with ammonia and precipitated by sulphydric acid at a boiling heat. The precipitate dried, roasted in a porcelain crucible, converted into sulphate in the crucible by means of aqua-regia and a few drops of sulphuric acid, was heated in the "radiator" until sulphuric acid fumes ceased to be evolved, afterwards over the naked flame, for a few moments, to low redness. The crucible was then allowed to cool, a single drop of aqua-regia and one of sulphuric acid added, again heated as before and finally weighed; care was taken to guard against absorption of moisture, while weighing, which the anhydrous salt is very prone to, and in no case did the sulphate, on subsequent treatment with water leave an insoluble residue.

In order to test the accuracy of weighing cobalt as sulphate, a method of determination employed and recommended by Gibbs and Genth,* some analyses were made of purpureo-cobaltic chlorid, an anhydrous salt of perfectly definite composition and one easily obtained in a state of purity. This salt, dried over sulphuric acid, was weighed in a porcelain crucible, sulphuric acid added and the cobalt converted into anhydrous sulphate by heating as above.

	I.	II.	III.	IV.	V.
Substance taken	·9005	·4041	1·2340	1·0478	1·0413
$CoSO_4$ obtained	·5573	·2498	·7625	·6497	·6443
Percentage $CoSO_4$	61·90	61·81	61·80	62·01	61·89
			Mean		61·88

Gibbs and Genth obtained as a mean of four analyses of this salt 61·90 per cent of $CoSO_4$.

Taking the atomic weight of cobalt as 59· which number is indicated by these analyses, we have as the percentage of cobalt found—

I.	II.	III.	VI.	V.	Mean	Calculated.
23·56	23·53	23·53	23·60	23·56	23·556	23·56

From the above it appears that this method for determination of cobalt is all that could be desired.

In determining the sulphuric acid in the substance a portion was digested, on the water bath, with a little strong hydrochloric acid for a short time, then dissolved in water and precipitated by acetate of baryta, with addition of chlorid of ammonium, at a boiling heat, and allowed to stand for twelve hours or more before filtering.

The total amount of hydrogen was determined by combustion of the substance mixed with chromate of lead, placing in the anterior portion of the tube, first oxyd of copper, then metallic copper freed from hydrogen.

The following preliminary experiments were made. To an acid solution of crude ceroso-ceric sulphate was added a solution of luteo-cobaltic sulphate in excess. To the solution filtered from the resulting precipitate, permanganate solution was added when a further precipitation of cerium ensued. This seemed to indicate the compound formed to be a ceric salt.

The filtrate from this last precipitate showed powerful didymium bands. (I think as many as thirteen bands were made out, certainly eleven) and was proved free from cerium by Gibbs' test,† with peroxyd of lead.

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* "Researches on the ammonia-cobalt bases." Smithsonian Contributions to Knowledge, 1856.

† This Journal, vol. xxxvi.

The precipitates, being found to contain didymium, were converted into sulphate, oxydized by boiling with peroxyd of lead and again precipitated by luteo-cobaltic sulphate. This last precipitate was also found to contain traces of didymium.

Weighed quantities of crude ceroso-ceric oxyd were converted into sulphate, luteo-cobaltic sulphate and potassic permanganate added, and the yellow precipitates collected on weighed filters and, after weighing, converted into ceroso-ceric oxyd.

I. ·5638 grms. substance gave 2·0395 grms. of yellow salt, yielding ·4780 grms. ceroso-ceric oxyd.

II. ·6960 grms. substance gave 2·3670 grms. of yellow salt, yielding ·5940 grms. ceroso-ceric oxyd.

The filtrates were free from cerium.

	In original substance.		In yellow salt.	
	I.	II.	I.	II.
Per cent of Ce_3O_4	84·78	85·35	23·44	25·09

It appears from this, that the yellow salt, formed in this manner, cannot be relied upon, as possessing a uniform constitution.

A quantity of pure ceroso-ceric sulphate was precipitated by adding a slightly acid solution of luteo-cobaltic sulphate; the precipitate was washed with hot water. The washings were slightly colored to the end, indicating partial decomposition of of the salt. The precipitate on drying adhered to the filter and was of a rather pale yellow color.

III. 2·1175 grms substance gave ·5640 Ce_3O_4 and ·4405 $CoSO_4$
1·2725 " " " 1·4035 $BaSO_4$

		Ratio.
SO_4	45·43	9466
Ce	21 55	4717
Co	7·92	2685

This analysis leads to no rational formula.

A new preparation of this salt was made from a somewhat acid solution of pure ceroso-ceric sulphate and washed with hot water containing acetic acid. The precipitate was of a deeper color than the preceding and did not adhere so strongly to the filter.

IV. 1·8615 grms. substance gave ·4060 Ce_3O_4 and ·5050 $CoSO_4$

		Ratio.
Ce	17·65	3862
Co	10·33	3501

Another preparation made from nearly neutral ceroso-ceric sulphate was of much paler color, and adhered more strongly to the filter, than any previously made: this was not analyzed.

The above results led to the belief that the salt was contaminated with basic ceric sulphate.

A highly acid solution of pure ceroso-ceric sulphate was then heated and precipitated by a hot saturated acid solution of luteo-cobaltic sulphate. A bright yellow highly crystalline precipitate was formed, which was slightly washed with cold water, and which did not adhere to the filter on drying.

This precipitate when viewed under the microscope appeared as cleanly crystallized hexagonal prisms, of uniform size, and without admixture of other forms. Three preparations were, in this manner, made, which resembled one another in color and general character and which were separately analyzed.

V. 1·0345 grms. substance gave ·2160 Ce_3O_4 and ·2935 $CoSO_4$
1·0240 " " " 1·2875 $Ba SO_4$
1·7650 " " " ·5670 H_2O
VI. 1·0075 " " " ·2150 Ce_3O_4 " ·2840 $CoSO_4$
1·1665 " " " 1·4680 $Ba SO_4$
1·1645 " " " ·3690 H_2O
VII. 1·1330 " " " ·2455 Ce_3O_4 " ·3108 $CoSO_4$
1·0017 " " " 1·2695 $Ba SO_4$
1·3353 " " " ·4148 H_2O

These results lead to the simple formula

$$12\ NH_3Co_2 3SO_4 + Ce_2\ 3SO_4 + H_2O$$

This compound is of interest as being a purely ceric salt. It endures a temperature of 150° C. without decomposition.

	V.	VI.	VII.	Calculated.
SO_4	51·80	51·83	52·22	52·42
Ce	16·90	17·27	17·54	16·63
Co	10·80	10·74	10·44	10·74
NH_3	18·66	18·56	18·05	18·57
H_2O	2·47	2·21	2·39	1·64
	100·63	100·61	100·64	100·00

We now pass to the consideration of the corresponding cerous salt, which I found could be prepared by heating a nearly neutral solution of cerous sulphate and adding a hot concentrated solution of luteo-cobaltic sulphate so long as the supernatant liquid remained colorless. A yellow precipitate was formed, a little paler in color than the ceric salt, but showing to all appearances the same crystalline form. I regret much that I have, as yet, been unable to obtain crystals of such dimensions as would allow me to decide the question of isomorphism by actual measurement. I can at present only say, that the two substances cannot be distinguished from one another under the microscope.

VIII. 1·0240 grms. substance gave ·3050 Ce_3O_4 and ·2505 $CoSO_4$
·8500 " " " ·9858 $BaSO_4$
·9588 " " " ·2650 H_2O

The formula indicated is $12NH_3\text{Ꞓ}o_2 3\text{Ꞩ}\Theta_4+3(\text{Ꞓ}e\text{Ꞩ}\Theta_4)+H_2\Theta$.

	VIII.	Calculated.
$\text{Ꞩ}\Theta_4$	47·77	48·40
$\text{Ꞓ}e$	24·10	23·04
$\text{Ꞓ}o$	9·31	9·91
NH_3	16·10	17·14
$H_2\Theta$	2·07	1·51
	99·35	100·00

I succeeded also in forming the analogous salt of lanthanum. To a concentrated solution* of the sulphate, luteo-cobaltic sulphate was added, and the solution heated. The precipitate of a buff yellow color showed under the microscope crystals apparently of the hexagonal system with the edges so rounded as almost to conceal the hexagonal form.

IX. 1·0648 grms. substance gave ·3085 $\text{Ƚ}a\Theta$ and ·2495 $\text{Ꞓ}o\text{Ꞩ}\Theta_4$
·7852 " " " ·9072 $\text{Ƀ}a\text{Ꞩ}\Theta_4$
·8918 " " " ·2452 $H_2\Theta$

Assuming the atomic weight of this lanthanum to be 94, the probable formula is $12NH_3\text{Ꞓ}o_2 3\text{Ꞩ}\Theta_4+3(\text{Ƚ}a\text{Ꞩ}\Theta_4)+H_2\Theta$.

	IX.	Calculated.
$\text{Ꞩ}\Theta_4$	47·58	48·08
$\text{Ƚ}a$	24·76	23·54
$\text{Ꞓ}o$	8·92	9·85
NH_3	15·42	17·03
$H_2\Theta$	3·01	1·50
	99·69	100·00

Unsuccessful attempts were made to form double phosphates and chromates of cerium and luteo-cobalt.

Cerium was found to form with roseo-cobaltic sulphate compounds analogous to those formed with luteo-cobaltic sulphate. They are, however, easily decomposed by water, so much so, that an attempt was made to convert the ceric salt into basic ceric sulphate, by prolonged washing with hot water, with partial success.

X was formed by adding, to an excess of hot strongly acid solution of roseo-cobaltic sulphate, a solution of ceroso-ceric sulphate; this yielded a crystalline orange-brown precipitate which was slightly washed with cold water. The configuration of the crystals could not be well made out, but was apparently one of the complex forms of the regular system.

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* This solution contained also didymium and was obtained from the mother water of the first precipitation of basic sulphate before described, by "Watt's" process of boiling the ignited oxyd with chlorid of ammonium. This process affords a ready and satisfactory means of preparing a solution of lanthanum and didymium free from cerium, but has failed in my hands to remove all didymium from the cerium, even after the oxyds, prepared in various ways and ignited at different temperatures, had been boiled for hours, the solution of chlorid of ammonium being from time to time renewed, the residual oxyd converted into oxalate, ignited and again treated as before.

XI was formed by adding the roseo-cobalt solution to an excess of the cerium solution. The crystals formed were similar to X but larger.

X.	1·0661	grms. substance gave	·2395 gr. $\text{Ȼe}_3\Theta_4$ and ·2908 $\text{Ȼo}S\Theta_4$
	1·3270	" " "	1·6025 $BaS\Theta_4$
	1·2715	" " "	·3910 $H_2\Theta$
XI.	1·9660	" " "	·4702 $\text{Ȼe}_3\Theta_4$ and ·4960 $\text{Ȼo}S\Theta_4$
	1·8938	" " "	2·2970 $BaS\Theta_4$
	2·0620	" " "	·6037 $H_2\Theta$

These results lead to the formula: $10\,NH_3\,\text{Ȼo}_2\;3S\Theta_4 + \text{Ȼe}_2\,3S\Theta_4 + 5H_2\Theta$.

	X.	XI.	Calculated.
$S\Theta_4$	49·73	49·97	50·66
Ȼe	18·18	19·36	16·08
Ȼo	10·39	9·60	10·39
NH_3	14·96	13·83	14·96
$H_2\Theta$	7·00	7·31	7·91
	100·26	100·07	100·00

With cerous sulphate a homogeneous crystallization of the precipitate could be obtained only by adding the cerium solution to a large excess of roseo-cobaltic sulphate. The crystals under the microscope appeared in form like those of the ceric salt; and the remarks in regard to the isomorphism of the two luteo salts apply also to the roseo salt though with less force, insomuch as the appearance of the crystals is not so characteristic.

The analyses show the ceric and cerous salts to contain the same number of atoms of water which is in accordance with the supposition of isomorphism.

XII.	1·8118	grms. substance gave	·4700 $\text{Ȼe}_3\Theta_4$ and ·4713 $\text{Ȼo}So_4$
	1·1235	" " "	1·2877 $BaS\Theta_4$
	1·6596	" " "	·4880 $H_2\Theta$

Which seems to indicate the formula: $10NH_3\,\text{Ȼo}_2\;3S\Theta_4 + 3(\text{Ȼe}S\Theta_4) + 5H_2\Theta$.

	XI.	Calculated.
$S\Theta_4$	47·23	46·90
Ȼe	20·99	22·31
Ȼo	9·90	9·61
NH_3	14·27	13·85
$H_2\Theta$	6·75	7·33
	99·14	100·00

I did not succeed in forming an analogous salt of lanthanum.

It appears that the proportional amount of cerium, in all the compounds discussed, varies, within certain limits according to the mode of preparation: which is well shown by a comparison of the results X, XI, XII. This is as we might expect in compounds, the nature of which precludes the possibility of purification, either by recrystallization or by thorough washing.

The roseo-ceric salt appears to contain some admixture of the cerous salt.

The sulphuric acid found is no doubt a little low, as the filtrate, after standing a week or more, deposited a slight additional precipitate of baric sulphate to the amount of ·05 per cent.[*]

The variations are, it seems to me, not so great as to cast doubt upon the correctness of the formulas deduced.

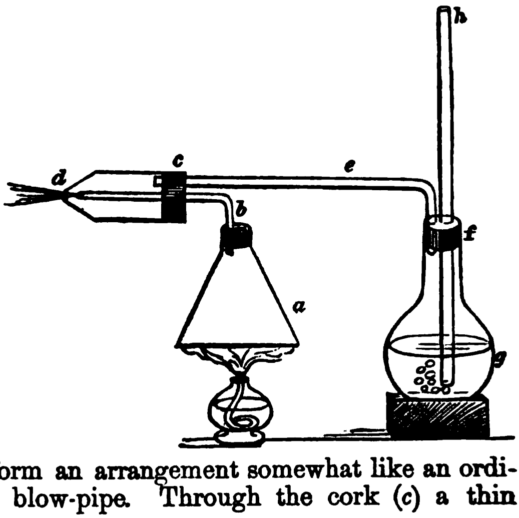
In conclusion, my best thanks are due to my kind teacher and friend, Dr. Wolcott Gibbs, to whom I am indebted for many and valuable suggestions, and also for the material used in this investigation.

Lawrence Scientific School, Cambridge, Jan. 15, 1870.

ART. XXXIX.—*On a new Aspirator*; by JOHN C. DRAPER.

I SEND herewith a description of an apparatus that answers admirably as an aspirator when it is desired to submit a large amount of atmospheric air to examination.

It consists of a tin boiler (*a*) for the generation of steam; to this a bent tube, $\frac{1}{8}$ inch bore (*b*) terminating in a pointed extremity, or jet, is attached. The tube (*b*) passes air-tight through a cork (*c*) into a second tube (*d*), about one inch in diameter also drawn down to a jet as at (*d*) in the figure.



The two tubes form an arrangement somewhat like an ordinary oxy-hydrogen blow-pipe. Through the cork (*c*) a thin tube (*e*), $\frac{1}{4}$ inch bore, passes and terminates in the upper part of the flask (*g*) after perforating the cork (*f*). In the flask, any suitable test liquid, as lime-water, is placed. A tube (*h*) $\frac{1}{2}$ inch in diameter, freely opened at both ends, is then passed through the cork (*f*) to give the air access to the bottom of the flask when the apparatus is in operation.

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[*] Compare Marignac, Ann. d. Chem. u. Pharm., lxviii, 212.

The method of action is as follows: heat being applied to the boiler (*a*), a strong jet of steam issues through (*d*), when if the tubes (*b*) and (*d*) are properly adjusted, a powerful draught is established through (*e*), which draws air rapidly through (*h*). I have in this manner drawn air at the rate of a gallon a second through a column of lime-water eight inches in height.

By attaching an ordinary gas suction to this apparatus, the air could be measured and passed through an extensive system of flasks or tubes containing suitable re-agents for the determination of minute traces of various ingredients.

College of the City of New York, Jan. 14th, 1870.

ART. XL.—*On two peculiar products in the Nickel manufacture;* by JOSEPH WHARTON.

I.

SEVEN years ago, when I was about to commence operations at Gap Nickel mine and furnace, I noticed among the fragments left by my predecessors a piece of nickel matte which contained occasional plates apparently of a metallic substance, tough, pliable, and elastic; these plates were about as thick as fine writing paper, from $\frac{1}{16}$ in. to $\frac{1}{8}$ in. wide and about twice as long. No chemical examination of them was made except a slight qualitative examination showing the presence of nickel, iron, and copper.

Having on several occasions subsequently noticed some tendency to a similar appearance in the matte, I gave directions that, when it should next be observed, a close examination should be made, after the extinction of the furnace producing the peculiar matte, of the solid mass which always remains in the bottom of the furnace.*

This solid mass consists in part of lumps of ore, flux, and fuel of the first charge, which reached the hearth imperfectly melted or consumed and so remained, and in part of accretions from the thoroughly fused matte which, as the furnace worked, formed a pool over and enclosing those lumps.

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* It should be explained that the furnaces in question are small upright blast furnaces, in which the ores of Gap Mine (Nicolliferous Pyrrhotite with 2 per cent Ni and Co), previously roasted, are smelted for the first time, the product being a matte containing about 12 per cent Ni and Co, with about 4 per cent Cu.

The cavities of such a mass seemed to me favorable for the production of crystals when a tendency to crystallize existed.

Last midsummer very interesting groups of crystals were in fact found upon breaking up one of these masses to fit it for remelting; they were so small, however, that, except for search being made in consequence of the matte of that furnace having exhibited the plates above named, the crystals would probably not have attracted attention.

Some of these crystals are cubical, with a bright metallic luster, the groups closely resembling miniature geodes of galena; others are minute octahedrons, arranged in spiculæ, and in dendritic or plumy forms, resembling the fern-like aggregations of zinc crystals which I sometimes found in the prolongs of my spelter furnaces at Bethlehem, Pa. Specimens of both cubes and octahedrons are forwarded with this paper.

These crystals are very tough, and are highly magnetic. A spicula of the octahedrons can be bent many times without breaking, and one which was floated upon water, after being lifted a few times by a magnet, pointed steadfastly to the north, and showed attraction and repulsion to the poles of a magnet just as a steel magnetic needle would under like circumstances.

A specimen of the octahedral crystals, and a specimen of the granular or almost crystalline solid matter to which they were attached, were submitted for analysis to the chemist of my establishment with the following results:—

	Crystals.		Granular.	
Cu	1·85	0·466	1·74	0·438
Ni and Co	25·22	6·837	28·20	7·640
Fe	64·10	36·622	62·50	35·861
S	8·90	43·925	7·60	43·939
	100·07	1 : 4·93	100·04	1 : 5·78

The subordinate column in each case shows the quantity of S which would be requisite to form, with the metals found, the compounds Cu_2S, Ni_2 and Co_2S, FeS; the ratio, below it, is that of the S found to that thus calculated.

If we conceive the copper to exist as Cu_2S, we then have 89·32 parts Fe, Ni, and Co in combination with 8·43 parts S: taking then the average atomic weight of the metals according to the proportions found, as 56·85, the atomic ratio of the metals to that of the sulphur is as 31·4 to 5·27, corresponding very closely to the formula R_6S.

Should the copper be included in the average, we get the figures R 32·00 S 5·56, indicating, though less accurately, the same formula.

II.

Desiring last year to make, in a granulated form, an alloy consisting of ¾ nickel, ¼ copper, I caused a mixture of the oxyds of those metals in the due proportions to be heated in closed crucibles with charcoal in a blast furnace; by this means reduction and fusion resulted, and the fused alloy was poured into water at a high white heat.

Among the granulated metal were found large numbers of hollow spheroids varying in size from peas to large chestnuts, many of them imperfect and torn, but many of them tolerably regular in shape, one side being usually bright and smooth, while the other was rough and pimpled.

As, upon crushing these with a hammer, the anvil was moistened, I examined a considerable number of them and found that they were nearly full of water, so that the water distinctly rattled within them when shaken, and showed itself in quantity when the larger spheroids were carefully broken. Fluid metal, poured white hot into water, had formed metallic bulbs filled with water.

For several days I carried some of these bulbs in my pocket, occasionally rattling the water in them, before the manner of their formation occurred to me. My first idea—that drops of metal falling upon the water were flattened by the blow, and that the edges then instantly clasped together and became welded, enclosing water within their grasp—was obviously untenable, for the eye could detect no seam or crevice, and besides, how could water exist as a liquid shut in by walls of this refractory alloy at a welding heat? Apple dumplings are formed in a manner somewhat similar to that, but these bulbs were not so formed.

The true solution is doubtless this: The metal when poured was in a state of ebullition, was giving off gas; not probably metallic vapor, but perhaps carbonic oxyd.

The separate globules into which the thin stream divided upon entering the water, were each emitting gas when contact with the water produced upon their surfaces an impervious film of solid or pasty metal. The continued evolution of gas in the fluid interior of such globules could have then no other effect than to distend the globule into a bulb, whose upper side might well be pimpled by the effort of tiny gas bubbles in the pasty shell to escape upward, while similar tiny bubbles working upward in the crust of the under side would reach the interior cavity, thus leaving the lower surface smooth and bright.

Multitudes of incipient globules were of course torn and distorted by reason of the internal gas finding a vent, and of course any which were rent must necessarily be filled by the water in which they were plunged. Those however in which the eye found no aperture were doubtless filled, after the coldness of the external water had so contracted the heat-rarified gas as to produce an approximate vacuum within the bulb, by the slow imbibing of water through minute pores to supply that vacuum. That such pores existed was shown by the fact of the bulbs all in time losing their water by exposure to a dessicating atmosphere.

Not all the pots of metal produced when poured, such globules, for not all were in the fit state of ebullition.

The nature of the disengaged gas might perhaps have been determined, if a sufficient quantity had been collected by breaking the globules under a receiver, but this was not done. I send specimens of the globules with this paper.

Philadelphia, March, 1870.

ART. XLI.—*On the action of sunlight on Sulphurous Acid;* by O. LOEW, Assistant in the College of the City of New York.

(Read before the "Lyceum of Natural Science," New York.)

WE know that plants under the influence of the sunlight reduce carbonic acid and water to organic compounds, and organized parts; we know further, that the albuminous principles, as well as some ethereal vegetable oils, contain sulphur which doubtless comes from the sulphates contained in the soil. As regards this reduction of sulphuric acid, it seemed to me of interest to ascertain whether sunlight possesses any reducing power upon the oxygen compounds of sulphur out of the tissues of the plant. For this purpose I exposed diluted sulphuric acid, solutions of sulphates and sulphites and aqueous sulphurous acid under various conditions, in sealed tubes to the sunlight during the last summer.

It was only with the sulphurous acid that any change was noticed. The tubes containing this substance *remained clear during two months*, but after that time a disturbance set in which slowly increased, and sulphur was deposited in a finely divided state.

The sulphurous acid was thus gradually reduced to sulphur, but the oxygen was not liberated, another part of the acid having been oxydized by it to sulphuric acid. It seems very singular that a space of two months elapsed before any change was observed; it appears that the absorption of a great amount of light was necessary for the separation of the first atom of sulphur, which was followed then by more atoms in much shorter intervals of time.

New York, December, 1869.

ART. XLII.—*On the formation of Ozone by rapid combustion;* by O. LOEW, Assistant in the Chemical Department of the College of the City of New York.

(Read before the "Lyceum of Natural Science," New York).

ACCORDING to the view of Schönbein, every slow oxydation is accompanied by formation of ozone, common oxygen not being able to combine directly with the elements. In 1858 Clausius advanced the hypothesis that ozone is oxygen in the state of *free atoms*, while common oxygen consists *of a molecule of two combined atoms.* But the later investigations of Andrews and Toit, Babo and Soret upon the volume of ozone have not supported this notion, and Clausius has modified his hypothesis accordingly, now believing that ozone is a combination between *an atom and a molecule of oxygen.* This combination is but a loose one, and the power of oxydation resides in the third atom of oxygen, which combines directly with other substances, leaving common oxygen behind. This constitution of ozone may be represented by the following formula:

$$3([OO])=2([OO]O)$$

The oxydation of a metal by ozone is shown by the equation $([OO]O)+M=M\Theta+([OO])$.

When we now take into consideration that ozone and antozone together give common oxygen, we must conclude that antozone is oxygen in the state of free atoms. Furthermore we see that common oxygen can be converted according to circumstances either into ozone or antozone. It seemed to me that in every combustion even the most rapid and energetic, an intermediate decomposition of the molecule of common oxygen must take place if the single atoms will enter into combination with the elements, and that ozone or antozone would be detected in a flame if the high temperature would not destroy it again as quickly as it is formed.

To prove this conclusion, I blew a strong current of air through a tube into the flame of a Bunsen's burner and collected the air in a beaker glass or balloon. I was thus able in a few seconds to collect enough ozone to readily identify it by its intense odor and by the common tests.

This observation shows that not only in slow oxydation but also in rapid combustion, an intermediate formation of ozone takes place,* and that it can be separated in the proper way.

New York, December, 1869.

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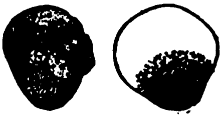
* Compare the observation of Pincus in the article on Nitrification, this Journal, II, vol. ii, p. 238.—EDS.

ART. XLIII.—*Contributions to Zoölogy from the Museum of Yale College.* No. 7.—*Descriptions of New Corals;* by A. E. VERRILL.

MADREPORARIA.

Heteropsammia geminata, sp. nov. Figure 1.

1.



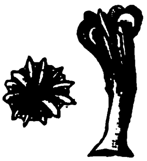
Corallum completely encrusting and enclosing a small, dead, univalve shell, which had been occupied by a worm (probably *Sipunculus*) by which a small circular opening was maintained. Base of various irregular forms, according to the form of the shell encrusted. Cup at first simple, slightly raised; it very early becomes elongated and finally divides by fissiparity, so as to form two equal and similar corallites, which are sometimes well separated and divergent, but more frequently closely contiguous. These corallites are but little elevated, becoming smaller toward the cup, which is circular, oval, or more or less irregular, and not very deep. Septa in five cycles, the last cycle more or less imperfect or wanting in some of the systems. The primary and secondary septa are narrow, much thickened and spongy outwardly, a little projecting; the septa of the fourth cycle are narrow and thin, outwardly joining those of the first and second, opposite the inner edges of the latter joined together in pairs, the inner and basal portion becoming broader before joining the columella, which is moderately developed and fine spongy. The coral has an open vermicular and quite porous texture, more compact beneath. In all the specimens there are several small holes, like pin-holes, near the base of the corallites, often forming a nearly complete circle around the base, which are, perhaps, made by parasites.

The base of the largest specimen is ·65 long; ·50 broad; ·45 high; corallites ·12 high; ·35 broad; depth of cell ·12 of an inch. Burmah,—W. H. Dall.

Of this species there are eighteen specimens of various sizes. It is remarkable as the second living species of this curious genus, and still more so as furnishing another instance of fissiparity in the family of *Eupsammidæ*,—a feature not hitherto recognized as normal in any genus except *Lobopsammia* of the Eocene. Doubtless the other species divide in the same way, for *H. Michelinii* E. and H. is described as having the cup shaped like the figure 8, which is the form in this species just before dividing. *H. Michelinii* appears to differ not only in dividing less freely, but in its smaller cup and a different arrangement of the septa, those of the three first cycles being described as about equal, while in *H. geminata* those of the third cycle are very small. The columella in the latter appears to be much less developed.*

Desmophyllum simplex, sp. nov. Figure 2.

2.



Corallum elongated, slender, turbinate above, rapidly enlarging to the edge of the cup; lower half of column smooth and round, upper part, toward the cup, somewhat angular with twelve thin, sharp, crest-like costæ, which become much elevated near the summit; surface finely granulous. Cup distinctly angular, usually somewhat hexagonal, deep, very open, with a thin wall. Septa in three cycles; those of the third cycle very small, thin, very narrow, and but little elevated; those of the first and second broadly rounded at summit and a little excurved, perpendicular within, thin, the surfaces finely granulated, the primaries considerably the largest. Six corallites grow up together in an irregular cluster, several of these uniting together at base. The largest is ·80 of an inch high; ·12 in diameter at middle; the cup ·38; primary septa ·14 broad; height above edge of cup ·10 of an inch.

St. Thomas, West Indies,—Mrs. E. H. Bishop.

HETEROZOANTHUS, gen. nov.

Polyps creeping on the surface of sponges, etc., by thin, basal, stolon-like expansions of the base, from which the polyps arise in linear series. The polyps are short, capable of contracting nearly to a level with the basal membrane. Tentacles few, 12 to 24. Integument stiffened by foreign bodies imbedded in the skin, such as sponge-spicula, etc.

Besides the following, this genus appears to include several other known species: *H. Swiftii* (*Gemmaria Swiftii* D. and M.) is parasitic upon a sponge in the West Indies; *H. Axinellæ* (Schmidt sp.) on two species of *Axinella*, Adriatic Sea.

Heterozoanthus scandens, sp. nov.

Polyps small and low, connected by a narrow basal membrane, which is a little wider than their bases and creeps over and is partially imbedded in the surface of a branching sponge, rising to the tips of all the branches, some of which are eight inches long, and forming irregular reticulations over the surface, though at times ascending for two inches or more with a linear series of polyps; rarely with double series. The polyps are near together, seldom more than their own diameter apart, and often in contact; in contraction they rise but little above the basal membrane in the form of low, flattened warts, with a depression at summit from which radiate 12 to 15 sulcations. Internal lamellæ 12. Integument firm, filled throughout with small, glistening, white spicula, probably derived from the sponge. Diameter of contracted polyps ·08 to ·10; height ·02 or ·03; breadth of basal membrane about ·10 of an inch.

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* *H. eupsammides* (Gray sp.) from China, may be nearer this if distinct from the former, to which Edwards and Haime refer it.

Sherbro Island, West Africa,—Prof. A. Hurd.

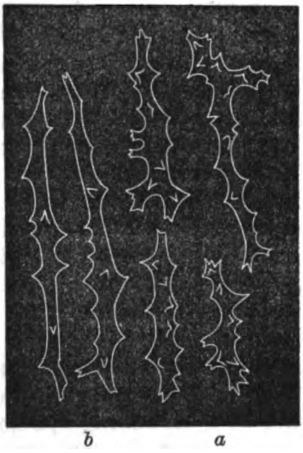
ALCYONARIA.

Telesto Africana, sp. nov. Figure 3.

Corallum forming a cluster of long, rather slender, branched, tubular stalks connected by a creeping base. The stalks are 2 to 4 inches long, with eight longitudinal ridges or low costæ. The polyps are arranged irregularly along the sides of the branches and main stalks, mostly at distances of ·10 to ·25 of an inch apart, sometimes opposite, and in contraction are prominent, tubular, and placed obliquely, sulcated with eight grooves. Color when dried yellowish white. Diameter of stalks ·06 to ·08; of polyp-tubes ·05; length of polyp-tubes ·10 to ·12 of an inch when contracted.

The walls of the stalks and branches consist of rather slender rudely, irregularly, but sparingly branched and spinulose spicula of various forms (fig. 3, a), which are closely interlaced, as in the other species of the genus. Other forms of long, very slender, more distantly spinulose, often bent spicula are also abundant (fig. 3, b).

3. $\frac{100}{1}$



The stouter spinulose spicula measure ·252^{mm} by ·036^{mm}, ·192 by ·060, ·192 by ·048; the long slender spicula ·528 by ·024, ·492 by ·036, ·456 by ·066, ·432 by ·036, ·420 by ·036, ·408 by ·048, ·366 by ·048.

Sherbro I., on the base of *Muricea granulosa*,—Prof. A. Hurd.

This species is closely allied to *T. fruticulosa* Dana, of the Carolina coasts, and, like that species, is encrusted by a parasitic sponge. But it is a more slender species and the spicula are longer and more attenuated.

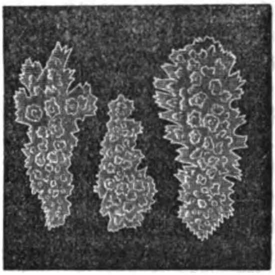
T. Riisei V., of St. Thomas (*Clavularia Riisei* D. and M.), is also nearly allied. *T. trichostemma* V. (Dana sp.) and *T. aurantiaca* Lamx. have stouter and more warty spicula.

Muricea granulosa, sp. nov. Figure 4.

Corallum rather slender, somewhat fan-shaped, branching in a plane; branches and branchlets irregularly sub-pinnate; branchlets slender, ·5 to 1·5 inches long. Verrucæ small, crowded, prominent, somewhat nariform, opening upward, the lower lip rounded, not prolonged. Cœnenchyma granulous with small, stout spicula. Color, when dried, dull yellowish or grayish brown. Height 6 inches; breadth 4; diameter of trunk ·15; of branchlets ·06 to ·08; of verrucæ ·03; height of verrucæ ·02 of an inch.

The spicula (fig. 4) are yellowish white and quite small for the genus; the larger ones are elongated, often curved, coarsely and roughly warted, stout spindles, tapering to acute ends; short, stout, roughly warted, irregularly oblong spicula with obtuse ends; and various short, stout, rough, club-shaped forms, which are often as broad as long, with large rough warts or spinules on the larger end. With these are various other forms of irregular spicula, with many smaller, rather slender, more regular, distantly and more evenly warted spindles.

4. $\frac{100}{1}$



The larger rough spindles measure ·648^{mm} by ·156^{mm}, ·504 by ·108; the oblong spicula ·408 by ·144, ·396 by ·168, ·312 by ·108, ·264 by ·108, ·264 by ·156, ·216 by ·096; the larger clubs ·396 by ·156, ·336 by ·144, ·288 by ·132, ·276 by ·120, ·264 by ·084, ·192 by ·108, ·180 by ·072; the largest of the slender spindles ·492 by ·096, ·444 by ·072, ·336 by ·072.

Sherbro Island,—Prof. A. Hurd.

This species is remarkable for the small size and roughness of the spicula and the form of the verrucæ. Its branches are also more slender than in most species.

Muricea vatricosa Kölliker (*Gorgonia vatricosa* Val.) from the Bizagos Archipelago is the only African species recorded previously. Of that we have received authentic spicula through Dr. Kölliker, from the original specimen. The spicula of *M. vatricosa* are much coarser and the corresponding forms are about twice as large; some of the larger spindles measure ·840^{mm} by ·180^{mm}, ·780 by ·240, ·744 by ·204; oblong spicula ·696 by ·240, ·456 by ·192; clubs ·660 by ·156, ·456 by ·180; the slender spindles ·720 by ·108. The lower lip is also said to be prolonged in the form of a small horn.

Leptogorgia robusta sp. nov.

Corallum consists of several stout principal branches arising from near the broadly expanded base; these main branches fork at irregular distances, and the secondary branches are irregularly pinnate, with branchlets mostly alternate on opposite sides, and ·5 to 1·5 inches apart. The branchlets are curved at base, then ascend at an angle of about 45°; they are rarely coalescent, stout, rigid, obtuse, 1 to 3 inches long, a little compressed, with a broad band of polyp-cells on each side, with narrow, depressed, sterile median bands, often with a distinct groove. The polyp-cells are numerous, close, rather large, oblong or oval, usually at the summit of large, low, rounded verrucæ, sometimes scarcely raised; they form about 4 to 6 irregular alternating vertical rows on each side of the branchlets, and 8 to 12 on the main branches, and are usually separated by distances about equal to their own diameter. Cœnenchyma moderately thick, finely granulous. Axis stout, round or a little compressed, nearly smooth and brownish black in the larger branches, the axils flattened; in the branchlets firm and rigid, tapering, dark reddish brown, slightly translucent; base thick, spreading, yellowish wood-brown.

Color of cœnenchyma dull dark yellow, tinged with purplish brown on the verrucæ. Height 12 inches; breadth 5; diameter of main branches ·22 to ·25; of axis ·12 to ·15; diameter of terminal branchlets ·13 to ·16; of their axis at base ·04 to ·05; diameter of verrucæ ·04 to ·05; height ·01 to ·03; diameter of cells ·02 to ·03 of an inch.

The spicula are small, bright yellow, intermingled with a few more slender ones that are bright purple; the latter mostly come from the verrucæ. Most of the yellow spicula are acute double-spindles, regularly tapering to each end, with three or four well separated whorls of warts on each half; some shorter and stouter forms occur, with more crowded warts and obtuse ends. The purple spicula are mostly slender, acute, sparingly warted spindles and double-spindles. Small, rounded, closely warted double-heads occur sparingly.

The larger yellow double-spindles measure ·216^{mm} by ·072^{mm}, ·204 by ·060, ·180 by ·072, ·168 by ·060, ·168 by ·066, ·156 by ·048; the purple spicula ·216 by ·036, ·204 by ·018, ·192 by ·030, ·180 by ·024, ·156 by ·030, ·144 by ·036.

Sherbro Island,—Prof. A. Hurd. Two specimens attached to the shell of an oyster, with *L. sanguinolenta* V.

This species somewhat resembles in mode of growth and general appearance *L. rigida* V. from the Gulf of California, etc., but has even stouter branches. The spicula and verrucæ are very different. The color of *L. rigida* is almost always uniform dark purple.

Leptogorgia dichotoma, sp. nov.

Corallum tall, slender, sparingly dichotomously branched. The trunk divides at about three inches from the base into two main branches; these fork at about 1 and at 2·5 inches, and part of the secondary branches divide again at about three inches from their origin, but others remain undivided for 6 or 8 inches. The branches and branchlets are long, rather slender, slightly tapering, obtuse, flexuous, spreading at base, compressed or somewhat quadrangular, with a narrow sterile band on each side, and broad lateral bands of crowded polyp-cells. The cells are rather large, oval, mostly raised on low, rounded, rather large, often crowded verrucæ, sometimes flat. Cœnenchyma moderately thick. Axis round, dark brown in the larger branches, light yellowish and setaceous in the terminal branchlets. Color bright lemon-yellow, the cells mostly surrounded by purplish.

Height 15 inches; breadth 4; diameter of main branches ·15; their axis ·09; diameter of branchlets ·11 to ·13; their axis at base ·05, in middle ·02 to ·03; diameter of verrucæ ·03 to ·04; height ·01 to ·02.

The spicula are yellow, with some more slender purple ones, as in the preceding species, but the average size is nearly twice as great. The most common forms are long, very acute, double-spindles, with four or five loose whorls of rough warts on each end; others are shorter and stouter, with more crowded whorls of warts. Some of the purple spicula are regular double-spindles, like those first described, but others, from the polyps, are slender spinulose spindles; some of the double-spindles are half purple and half yellow.

The larger double-spindles measure ·264^{mm} by ·060, ·252 by ·072, ·252 by ·060, ·240 by ·066, ·228 by ·084, ·228 by ·060, ·228 by ·054, ·216 by ·072, ·216 by ·060, ·204 by ·072, ·204 by ·060, ·192 by ·078; the purple polyp-spindles ·228 by ·024, ·186 by ·030, ·180 by ·018, ·168 by ·030, ·156 by ·030, ·144 by ·015.

Sherbro Island,—Prof. A. Hurd.

The new species herein described from the west coast of Africa are of peculiar interest, both as furnishing additional evidence of the richness of that little explored region in Gorgonacea, and as showing peculiar relations to the faunæ of the West Indies and Pacific coasts of America. They were collected by Mr. D. W. Burton, missionary, for the museum of Knox College, and sent to me for examination by Professor Hurd.

ART. XLIV.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School.* No. 11; by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.

1. *On a simple method of avoiding observations of temperature and pressure in gas analyses.*[*]

In absolute determinations of nitrogen and other gases, accurate observations of temperature and pressure are, in the ordinary methods of analysis, necessary, and when made require subsequent calculations which, when the analyses are numerous, become rather tedious. By the following simple method these observations may be altogether dispensed with, and the true weight or the reduced volume of the observed gas obtained at once by a single arithmetical operation. The volume of any gas at the temperature 0° C. and pressure 760 may be deduced from its volume at the temperature t and pressure p by the familiar expression:

$$V_0 = V_1 \frac{1}{1+0{\cdot}00367t} \cdot \frac{h-h'-h''}{760} \qquad (1)$$

in which h is the observed height of the barometer (reduced to 0° C.), h' the tension of the vapor of water at $t°$ when the gas is moist and h'' the height of the column of mercury in the collecting tube above the level of the mercury in the cistern. For any other gas under precisely the same circumstances of temperature and pressure we have the equation:

$$V'_0 = V'_1 \frac{1}{1+0{\cdot}00367t} \cdot \frac{h-h'-h''}{760} \qquad (2)$$

Whence dividing the first equation by the second we have:

$$\frac{V_0}{V'_0} = \frac{V_1}{V'_1} \qquad (3)$$

or as a proportion $\quad V_1 : V'_0 :: V_0 : V'_0 \qquad (4)$

from which it appears that the reduced volume (vol. at 0° and 760^{mm}) of the second gas may be found without observations of temperature and pressure, provided that the unreduced volume be observed under the same circumstances of temperature and pressure as the volume of the first gas, the reduced volume of which has been previously determined. Let the first or standard gas be air; then if the weight of one cubic centimeter of dry air at 0° and 760^{mm} be w the whole weight will be wV_0. In like manner we shall have for the weight of the gas to be measured $w_1V'_0$, and since the weights do not change with the temperature and pressure, we have finally:

$$w\,V_1 : w_1\,V' :: w\,V_0 : w_1 V'_0.$$

If now we suppose that the gas in the first tube, or standard gas is, for example, nitrogen, the volume remaining the same, and that the gas to be measured is also nitrogen, we have

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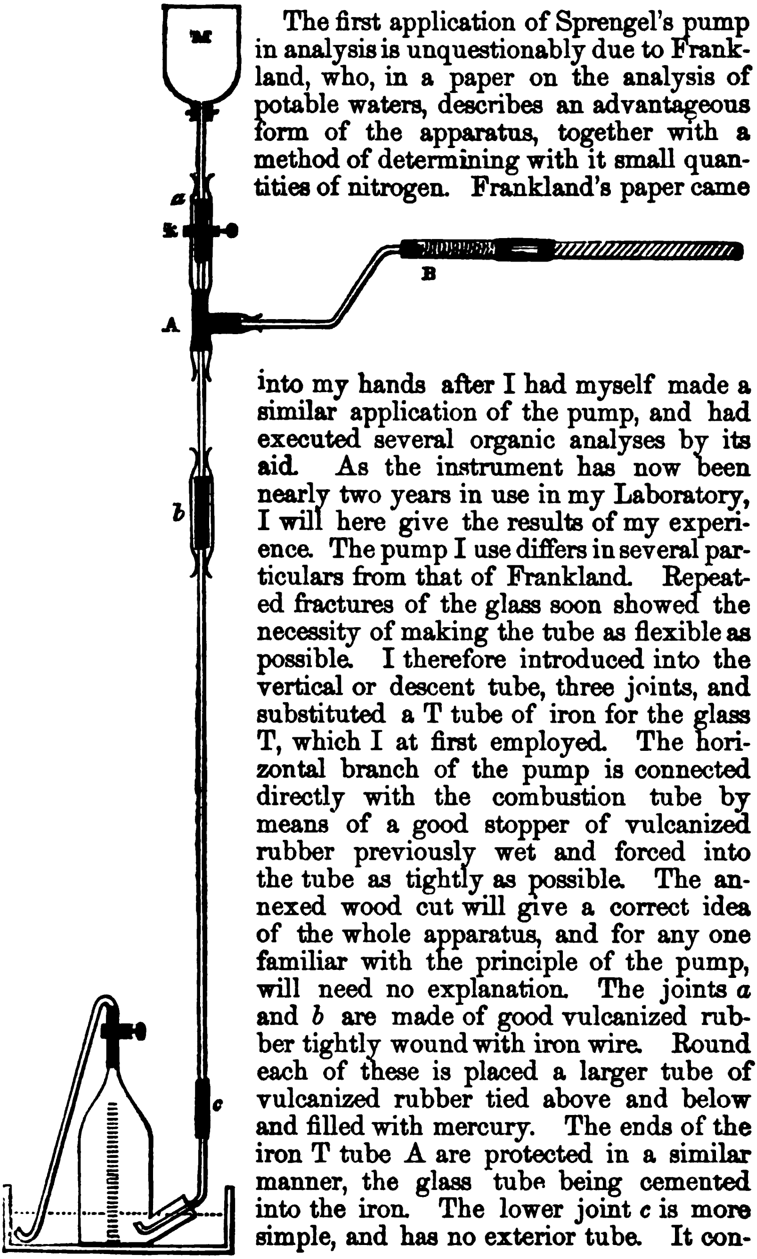
[*] Read before the National Academy of Sciences, Sept., 1869.

$$w_1\ V_1 : w_1\ V'_0 :: w_1\ V_0 : w_1\ V'_0$$

or simply $$V_1 : V' :: w_1 V_0 : w_1 V'_0. \qquad (5)$$

The application of this formula in practice is as follows: A graduated tube holding about 150 cubic centimeters is filled with mercury and inverted into a mercury trough. Two thirds or three fourths of the mercury are then displaced by air, care being taken to allow the walls of the tube to be slightly moist, so as to saturate the air. This tube may be called the companion tube; the volume of air which it contains must be carefully determined in the usual manner by five or six separate observations, taking into account, of course, all the circumstances of temperature and pressure. The mean of the reduced volumes is then to be found, and forms the constant quantity V_0. The gas to be measured is transferred from the receiver in which it is collected, into a (moist) eudiometer tube, which is then suspended by the side of the companion tube, and in the same trough or cistern. Both tubes being supported by cords passing over pullies, it is easy to bring the level of the mercury in the two tubes to an exact coincidence. The pressure on the gas is then the same in each tube. The temperature is also the same as the tubes hang side by side in the room set apart for gas analyses, and are equally affected by any thermometric change. It is then only necessary to read off the volumes of the gas in the two tubes to have all the data necessary for calculating the weight of the gas to be measured. This calculation may be effected in two ways, each of which will be found of use. Thus proportion (4) reads in words: as the observed volume of the air in the companion tube is to the observed volume of the gas in the measuring tube, so is the reduced volume of the air in the first—previously determined as above—to the reduced volume of the gas to be measured. This method of course applies to the reduction of any gaseous mixture whatever to the normal pressure and temperature. In absolute nitrogen determinations, however, proportion (5) gives the weight of the nitrogen measured at once, since the term $w_1 V_0$ is found by multiplying the weight of 1 c.c of nitrogen at 0° and 760^{mm} by the reduced volume of air in the companion tube, and is a constant which can be used as long as the companion tube lasts. In practice, a companion tube filled with mercury will last with a little care for a very long time. Even when filled with water I have found that excellent results may be obtained, and that the tube will last for some weeks. Williamson and Russell, in their processes for gas analysis, have employed a companion tube for bringing a gas to be measured to a constant pressure, but the application made above is, I believe, wholly new.

2. *On the application of Sprengel's mercurial pump in analysis.*



The first application of Sprengel's pump in analysis is unquestionably due to Frankland, who, in a paper on the analysis of potable waters, describes an advantageous form of the apparatus, together with a method of determining with it small quantities of nitrogen. Frankland's paper came into my hands after I had myself made a similar application of the pump, and had executed several organic analyses by its aid. As the instrument has now been nearly two years in use in my Laboratory, I will here give the results of my experience. The pump I use differs in several particulars from that of Frankland. Repeated fractures of the glass soon showed the necessity of making the tube as flexible as possible. I therefore introduced into the vertical or descent tube, three joints, and substituted a T tube of iron for the glass T, which I at first employed. The horizontal branch of the pump is connected directly with the combustion tube by means of a good stopper of vulcanized rubber previously wet and forced into the tube as tightly as possible. The annexed wood cut will give a correct idea of the whole apparatus, and for any one familiar with the principle of the pump, will need no explanation. The joints *a* and *b* are made of good vulcanized rubber tightly wound with iron wire. Round each of these is placed a larger tube of vulcanized rubber tied above and below and filled with mercury. The ends of the iron T tube A are protected in a similar manner, the glass tube being cemented into the iron. The lower joint *c* is more simple, and has no exterior tube. It consists of thick vulcanized rubber covered on the outside with a varnish, suggested to me by Prof. Crafts, and consisting of a mixture of pitch, gutta percha and wax melted together. A strong steel screw clamp *k* serves to regulate the flow of the mercury in the pump tube, the diameter of which is about 1½ millimeters. The mercury reservoir M consists simply of a large green glass bottle, with the bottom cut off, inverted and provided with a good stopper of vulcanized rubber, through which the upper part of the pump tube passes.

The use of this apparatus in absolute determinations of nitrogen is as follows: The substance to be analyzed is mixed in a rather short combustion tube with chromate of lead and a small proportion, five or six grams, of chromate of suboxyd of mercury. The anterior portion of the tube is then nearly filled with freshly reduced, finely divided metallic copper, in front of which a few grams of carbonate of manganese is placed. The connection with the pump being made, the apparatus is first tested by running the pump for a few minutes and allowing the whole to stand for a short time, to see if the level of the mercury in the pump tube remains unchanged. The combustion tube is then to be completely pumped out—an operation which requires from five to ten minutes only—after which the carbonate of manganese is to be cautiously heated until a sufficient quantity of carbonic acid is evolved to completely fill the apparatus and restore the equilibrium of pressure within and without. The combustion is then conducted in the usual manner, care being taken to keep the column of metallic copper at a full red heat, and to proceed slowly. When the combustion is finished, the pump is again set in operation until a perfect vacuum is obtained. The receiver which I employ for collecting the gas, is that of Simpson. It is first filled with mercury; afterwards about fifty cubic centimeters of a solution of caustic potash, of density 1·2, are introduced to absorb the carbonic acid. The nitrogen is then to be transferred to a eudiometer tube and measured. The following analyses will serve to show the degree of accuracy attainable by this process:

0·1380 gr. crystallized asparagin gave 21·98 c.c. nitrogen (moist) at 13°·75 and 759·6^{mm}=18·69 per cent. The formula requires 18·66 per cent.

0·2910 gr. sodic stryphnate gave 125·86 c.c. nitrogen at 18° C. and 559^{mm}=36·28 per cent. nitrogen. The formula $Na(\text{Є}_4H_2H_5\Theta_2)+H_2\Theta$ requires 36·26 per cent.

0·8593 gr. potassic oxalurate gave 115 c.c. nitrogen at 6°·25 C. and 762·7^{mm} =16·50 per cent. The formula $K\text{Є}_3H_3N_2\Theta_4$ requires 16·47 per cent.

0·6730 gr. allantoin gave 189 c.c. nitrogen at −1° C. and 761·5^{mm} =35·40 per cent. The formula $\text{Є}_4H_6N_4\Theta_3$ requires 35·40 per cent.

In working with Sprengel's pump in the manner indicated, many observations of practical value have been obtained. In the first place it has been clearly shown that the products of combustion executed in vacuo frequently differ in a remarkable manner from those conducted under ordinary atmospheric pressure. Thus ammonic nitrate at common pressures is decomposed by heat with simple resolution into nitrous oxyd and water. But in the nearly perfect vacuum produced by the mercurial pump, the reaction is entirely different, very large quantities of red nitrous or hyponitric vapors being evolved. In like manner Mr. Sharples has observed that cupric oxalate, which is usually decomposed by heat into carbonic acid and metallic copper, in vacuo always yields more or less carbonic oxyd. Gunpowder burns slowly and without explosion in vacuo—an observation which, however, is by no means new—but gun cotton explodes as violently as in air.

In my first experiments, in which I attempted to collect nitrogen by conducting the analysis without previously filling the apparatus with carbonic acid, I almost invariably found that a considerable quantity of oxyd of nitrogen—chiefly $N\Theta$—was mixed with the nitrogen collected. In addition it was extremely difficult to bring the analysis to a successful termination, because the pressure of the atmosphere often compressed the anterior portion of the combustion tube into a solid mass. I at first used cupric oxalate to furnish the required carbonic acid, but this was soon rejected for the reason given above, and I fell back upon magnesite or manganous carbonate. When the first successful experiments in determining nitrogen had been made, I was sanguine of being able to determine both nitrogen and carbonic acid in an organic body by a single analysis in which the gases should be collected and measured together, the carbonic acid being afterwards absorbed by potash. In this, however, I failed entirely on account of the compression of the combustion tube alluded to above, when the combustion is made in vacuo. I also attempted to absorb the carbonic acid by means of a tube filled with soda lime, as recommended by Mulder, but without success, the absorption being always incomplete.

Mr. Sharples has been more successful in determining nitrogen and hydrogen by a single analysis. By placing a weighed chlorid of calcium tube in front of the combustion tube, he has found it possible to determine the water formed in the combustion directly by weight, and at the same time to collect the nitrogen with all requisite accuracy. In this manner 1·3285 gr. of asparagin gave 0·8028 gr. water=6·71 per cent. hydrogen and 222·75 c.c. nitrogen at 15°·5 C. and 734^{mm}=18·66 per cent. The formula requires 18·66 per cent nitrogen and 6·66 per cent hydrogen.

It can hardly be doubted that further improvements in the process will render it possible to determine in a single analysis, carbon, hydrogen and nitrogen with greater facility and accuracy than nitrogen alone can be determined by the older methods. In conclusion I may state that the Sprengel pump may be applied with great advantage to the determination of the amount of gas given off from various substances by the simple application of heat.

ART. XLV.—*On a peculiar form of the discharge between the Poles of the Electrical Machine;* by ARTHUR W. WRIGHT, Prof. of Physics and Chemistry in Williams College.

WHEN the Holtz electrical machine is working at a high tension, but without the condenser, if the distance between the poles be gradually increased, the discharge successively varies from the ordinary spark of an inch or two in length to a diffuse, much branched, and feebly luminous spark, which under high tension may attain a length of several inches, though it gives but a slight detonation. If the interpolar space be still further widened, either the discharge becomes finally silent, or, more commonly, one or more small jets or brushes issue from the negative pole, with a hissing or fizzing sound, if the discharge is very energetic. When this is the case the positive pole is covered with a diffuse glow, resembling that of a phosphorescent substance, or rather, so thin is the luminous stratum, the pole appears as if it were *illuminated* by a light shining from the direction of the opposite pole. The polar interval, under which the glow appears to best advantage, varies somewhat with the condition of the atmosphere, but, with the machine used in these experiments (a Holtz machine with 20-inch revolving disk), extends from three or four to seven or eight inches, depending also to a certain degree upon the tension of the machine.

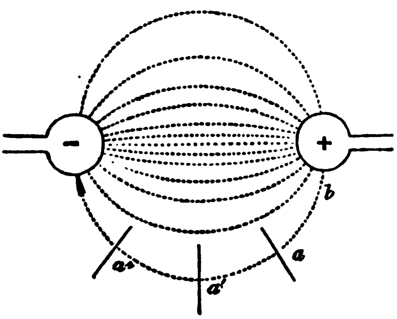
If now the finger or some other object be interposed between the poles, the glow is interrupted, and a silhouette of the object is formed upon the brass ball, which strongly resembles the shadow cast by the body when placed in a beam of luminous rays, moving as the body moves, expanding and contracting as the distance varies, and the like. So striking is the resemblance of this appearance, which may be called an *electrical shadow*, to a real shadow, that I had many times noticed it casually, when using the machine in a dimly lighted room, without suspecting that it was not actually a shadow due to light reflected from objects in the room upon the ball, and it was only when the experiments were made in a room completely darkened that its different character became clearly apparent.

The experiment succeeds best when the air is not too humid, and a single jet issues quietly from the negative. With a little care and after a few trials the shadows can be obtained with great distinctness and remarkable regularity. It is better in general to use for the interposed body some non-conducting substance, or an imperfect conductor, like paper, or wood, as when metallic objects are used they become electrified by induction, and disturb the regularity of the phenomena, or, when the tension is very high, cause the passage of diffuse sparks and brushes. With proper care, however, good results can be got with metals, though usually for much smaller polar intervals than with poor conductors.

In order to ascertain with how great accuracy of detail the figures are formed, a piece of wire gauze was used, the meshes of which were about $2{\cdot}5^{\mathrm{mm.}}$, and the wires forming them about $1^{\mathrm{mm.}}$ in diameter. The poles being separated about four and a half or five inches, the shadows were formed with striking distinctness, even where the gauze was two inches distant from the positive pole, growing smaller and smaller as it receded. Every peculiarity of the texture was faithfully represented, the irregularities of the wires, breaks in the gauze, and the like, being accurately reproduced, and moving with the gauze, just as in the case of true optical shadows. With a piece of still finer wire-cloth, the meshes of which were about $1{\cdot}3^{\mathrm{mm.}}$, similar results were obtained, the distances, however, being smaller than in the previous case. When the gauze was an inch or an inch and a half from the positive, the meshes in the shadow were seen with some difficulty on account of the feeble illumination, but still appeared quite well formed and regular. Their size could not readily be ascertained by measurement, but they were estimated at from one-third to one-half a millimeter in diameter.

More interesting and varied results were obtained with a grating made of common writing paper by cutting out square apertures about $3^{\mathrm{mm.}}$ in diameter, and the same distance apart. The piece used contained twenty-five such apertures, arranged in the form of a square with five on a side. Shadows of great sharpness and accuracy were obtained with ease even when the poles were seven or eight inches apart. As the paper was moved toward the positive side they grew smaller, and again enlarged as it approached the negative, becoming, however, somewhat indistinct when nearer the latter. When the negative jet and the center of the grating were in the line joining the two poles, the images were formed symmetrically about the vertex of the positive pole. The corner apertures in this case being more distant from the axis, had their images somewhat distorted, so that the sides of the square were represented by lines curved in such a way as to make the angles at the corners acute, just as it would happen if the square should be stretched in the direction of its diagonals.

Very frequently, in fact usually, the jet does not issue from the vertex of the negative, but is displaced to the one side or the other. In such cases the glow is also displaced in a similar manner, that is, so that its axis is inclined at an equal angle to the line between the poles with that of the negative jet. If the grating is placed before the latter, and perpendicular to it, the shadows are still formed as before, though not in general quite as perfectly. Thus if placed at *a*, as in the cut, the image appears at *b*. On moving it away from the pole to a position represented by *a′* the image still appears at *b*, but is larger, and so on at *a″*, or until it is brought within an inch or two of the negative. In this way the lines traversed by the electricity, or along which it acts, may be readily traced, and they are found to have a conformation similar in some respects to that of the voltaic arc, but far more regular and symmetrical. In their general character they are represented by the dotted lines in the cut. Issuing normally from some point of one of the poles, a line passes in a curve to the homologous point on the other pole, where also it meets the surface normally. When the jet is not far from perpendicular to the line of the poles, the curve may have an amplitude of nearly or quite half the distance between the latter, even when these are eight inches, or more, apart.



An interesting case occurs when the negative discharge takes the form of a ring. The glow likewise assumes the form of a narrow and sharply defined ring, whose size and position of course depend upon those of the other ring. The approach of any intercepting or disturbing body either breaks or bends the luminous line, and this affords, perhaps, the best means of determining the form of the curves between the poles, as the limits where the disturbance begins are pretty definitely marked, and their position can be readily determined by measurements. An easy way to secure a steady and well-defined ring, is to place upon the negative pole, first removing the ball, a common bottle cork an inch or two in diameter, well shaped and carefully smoothed, with the larger face towards the positive pole, and perpendicular to the polar axis. On putting the machine in action, the electricity issues from the sharp edge of the cork in the form of a regular and steady fringe-like ring, the radial elements of which stand at a nearly constant angle with the axis. A beautifully distinct ring is thus formed upon the opposite pole.

Comparing the forms of the curves indicated by the above experiments with the ordinary sparks and brushes, or even with the voltaic arc, a striking difference in respect to symmetry and constancy is observed; and the distinctness and beauty of some of the phenomena are very remarkable, especially when it is considered how lawless and irregular the ordinary spark and brush discharges are as to form. The glow, like the brush, is obviously caused by the discharge of electricity at the positive pole, and apparently a discharge from one pole to the other takes place, silently and without luminous effect, through or by means of the intervening air. But that the lines along which it takes place should be so constant and regular is somewhat surprising. It is to be observed that the perfection of the results depends to a considerable degree upon the condition of the air, as when this is too moist, the discharge is not sufficiently energetic, or the resistance is not sufficiently great, to bring out the glow, or to form the images with distinctness.

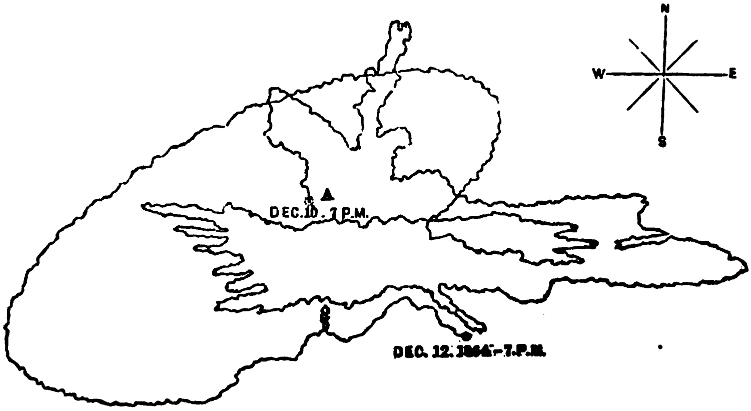
Williamstown, Mass., April 5, 1870.

ART. XLVI.—*Movement of the Dome of the Capitol at Washington, during the gale of December* 10–12, 1869. From a letter to the editors.

WE all know, by having seen the thing itself, or representations of it, the form of the dome of the capitol at Washington. It is of cast and wrought iron throughout. Its architectural beauty is only equaled by the truly wonderful combinations of its multitudinous parts. Had it been erected at any other time than during the late war, when men's views were absorbed in watching the vicissitudes of the contest, it would have attracted the eager attention of hundreds of scientific observers, who would have noticed, with the deepest interest, the ingenuity and skill with which all the exigencies of the structure were provided for, until a rail road, built, as it were, in the skies, was used to place the statue on the summit.

The exterior diameter of the dome, where the iron work rests, as it were, on the roof of the building, is somewhere about one hundred and thirty-seven feet; and the height, from the roof to the feet of the statue about two hundred—the statue is between nineteen and twenty feet high.

The distinguished architect, Mr. Thomas M. Walter, supposed naturally enough, that this enormous amount of iron would be more or less affected by the action of the sun's rays—causing an expansion, to meet which he had been making, throughout his protracted labors, all possible provision. To ascertain what this effect would be, he suspended a wire from the center of the ceiling of the Tholus, or crowning cupola, under the feet of the statue. At the extremity of this wire nearest the ground, or pavement of the rotunda, he arranged a delicate mechanism, that carried a pencil, whose point rested on a sheet of paper, on which it was expected that expansion and contraction would record their effects. It was Mr. Walter's expectation, probably, that, as the sun moved from the east to the west, something of a uniform curve would be traced by the pencil's point upon the paper, furnishing, in that way, data that might be as useful as they would be original.



It happened, however, that the wind, and not the sun made use of Mr. Walter's preparations, and recorded its vagaries, through the agency of the vast mass of the dome. One would have thought, looking to the breadth of the base, and the form of the architecture above it, that the dome would remain unmovable against any action of the wind. This was not so, however; and the diagram above shows what a gusty day was capable of effecting, in giving motion to the mass. Beginning at A, and following the lines, it can be seen when the wind blew fitfully but moderately, and then, when a blast of unusual violence occurred; and when, too, the direction was changed, as the wind veered round the points of the compass. That chimneys and shot towers wave in the wind is well known; but the movements that take place in rough weather in the dome of the capitol could hardly have been reasonably expected.

The diagram above is a tracing carefully made from the work done by the pencil on the occasion now referred to. L.

I. PHYSICS AND CHEMISTRY.

1. *On the heat of combination of boron with chlorine and oxygen.*—**Troost** and **Hautefeuille** have communicated to the Academy of Sciences a memoir on the heat of combustion of boron, a subject which we believe has not hitherto received attention but which the rapidly increasing importance of thermo-chemistry invests with great interest. The authors employed in their investigations the muffle calorimeter of Favre. Dry chlorine was allowed to act upon amorphous boron placed in the muffle, the chlorid of boron formed being condensed in the water of the calorimeter. The heat measured was thus the sum of the heat of combustion proper and of the heat evolved in the combination or double decomposition of the chlorid with water. The heat due to the last mentioned reaction was then determined by a special experiment. In this manner the authors obtained as a mean of 6 experiments 104000 units of heat developed in the combination of one atom of boron with three atoms of chlorine, and 79200 units of heat disengaged by the reaction of one atom of boric chlorid, BCl_3, with 140 times its weight of water. From the heat developed in the last case the authors deduced the heat of combination of boron and oxygen by subtracting first the heat due to the formation of three equivalents of chlorhydric acid as determined by Favre, and secondly the heat produced by dissolving an equivalent quantity of boric acid in an equal weight of chlorhydric acid of the same degree of dilution, as determined by direct experiment. In this manner the heat of combustion of boron in oxygen was found to be 158600 units for one equivalent (old style) of boron. The boron employed was in the amorphous modification; the authors promise similar determinations for the other forms of this element. —*Comptes Rendus*, lxx, 185. W. G.

2. *On the heat of combination of silicon with chlorine and oxygen.*—The same authors in a second communication have given a determination of the heat of combustion of silicon with chlorine and oxygen. To effect the union of chlorine with amorphous silicon it was found necessary to mix the silicon with $\frac{1}{10}$ of its weight of boron, the heat of combination of which was of course subtracted. The mode of experimenting and the corrections were the same as in the case of boron described above. It was found that one equivalent of chlorid of silicon acting upon 140 times its weight of water disengages 40825 units of heat and that one gram of silicon in burning in chlorine evolves 5630 units, which amounts to 78820 units for one equivalent ($Si=14$), or to 118230 units for one equivalent ($Si=21$). By a process exactly analogous to that employed in the case of boron the authors found for the heat of combination of silicon and oxygen 7830 units per gram, which amounts to 109620 units per equivalent ($Si=14$) or 164430 units for $Si=21$. In addition the heat of transformation of amorphous into crystalline silicon was determined by dissolving the different varieties of silicon in nitro-fluohydric acid and thus obtain-

ing the difference in the quantities of heat disengaged by their oxydation. In this manner it was found that amorphous silicon in becoming crystalline evolves 290 units of heat per gram, or 4060 per equivalent ($Si=14$).—*Comptes Rendus*, lxx, 252. W. G.

3. *On some remarkable spectra of compounds of zirconium and the oxyds of uraninm.*—Mr. Sorby has found that the remarkable absorption bands exhibited by certain specimens of zircon and which he had attributed to the presence of a new metal—Jargonium—are in reality due to uranium, which, under certain circumstances, gives bands of a very remarkable character wholly unlike those yielded by the ordinary salts of the metal. Thus the salts of uranium usually exhibit a variable but small number of moderately broad absorption bands at the *blue* end of the spectrum, while the zircons exhibit numerous narrow black lines, fourteen of which are quite distinct, together with other fainter lines and a broad black band extending from the *red* end, so that nearly all occur in that part of the spectrum which is entirely free from bands in the other uranic compounds. Mr. Sorby has detected in some zircons erbium, didymium, yttrium and another substance which exists in such small quantity that the author has not been able to decide whether it is a new earth or not. In fact the spectral analysis in this case fails to give any decisive evidence, since some other known oxyd may give with zirconia a spectrum as abnormal as that produced by the presence of a trace of uranium. There is therefore at present no evidence that zircons contain any new metallic oxyd.—*Chemical News*, *Feb.* 18, 1870. W. G.

4. *On the dissociation of ammoniacal compounds.*—In investigating the dissociation of ammoniacal compounds Isambert has found that the tension of the ammonia evolved is constant at a given temperature and increases progressively with it. The author obtains the anhydrous ammonia-sulphates of zinc, cadmium, &c., by passing a current of ammonia for several hours over the salt, which swells up, becomes hot and falls to powder. The ammonia-sulphates of zinc and cadmium thus formed correspond respectively to the formulas $ZnSO_4+10NH_3$ and $CdSO_4+6NH_3$. In like manner calcic and strontic chlorids unite with ammonia to form $CaCl_2+8NH_3$ and $SrCl_2+6NH_3$; hot boric chlorid does not unite with ammonia. By heating the compound formed in vacuo, the tension of the ammonia may be measured. Cadmic sulphate gave the following results, care being taken after each experiment to expel the gas which fills the apparatus:

Temperatures.	Tensions.
48°·5	368^{mm}
51°·5	439^{mm}
100° (mean of 6)	$1366·3^{mm}$

The salt remaining in the tube consisted of $CdSO_4+2NH_3$ and gave off no more ammonia at 100°. The author infers that the cadmic salt should be regarded as $(CdSO_4 2NH_3)4NH_3$ and finally that the pulverulent compounds formed always absorb a little ammonia mechanically, so that the tensions do not become constant until only the combined gas remains. The general results of this investigation agree with those which the author had previously obtained with the ammonia-chlorids. Lamy has based the construction of a new thermometer upon the tension of the ammonia evolved in heating the ammonia-chlorids and especially that of calcium. Between 0° and 46° the tension increases from 120 to 1431 millimeters, and will therefore give an extremely delicate measure of increments of temperature.—*Comptes Rendus*, lxx, p. 456 and p. 393. W. G.

5. *On a new method for the synthesis of organic acids.*—Berthelot has found that the hydrocarbons of the acetylene series are capable in the presence of alkalies of uniting directly with water, oxygen and the alkaline base to form acetic acid and its homologues. Thus in the case of acetic acid we have,

$$\text{€}_2\text{H}_2+\Theta+\text{H}_2\Theta=\text{€}_2\text{H}_4\Theta_2.$$

Acetylene may be converted with acetic acid more completely by means of pure chromic acid, the action of which, according to Berthelot, is different from that of a mixture of potassic bichromate and sulphuric acid, and much more moderate. Thus chromic acid converts ethylene into aldehyd, propylene into acetone and camphene into camphor, which reactions are not effected by sulphuric acid and the bichromate which act too powerfully. Pure chromic acid acts on the carburets at ordinary temperatures and its action may be increased by heat or moderated by dilution. Allylene under the same circumstances of oxydation yields propionic acid. Propylene, $\text{€}_3\text{H}_6$, yields propionic acid, acetic acid and acetone, the propionic acid being probably derived from propionic aldehyd, isomeric with acetone. Chromic acid even attacks carbon in the cold. By operating with pure carbon Berthelot succeeded in obtaining a small quantity of oxalic acid,

$$2\text{€}+3\Theta+\text{H}_2\Theta=\text{€}_2\text{H}_2\Theta_4.$$

The oxydation of allylene by means of chromic acid appears first to yield an aldehyd $\text{€}_3\text{H}_4\Theta$, which then by taking up the elements of water becomes propionic acid. The author sums up his results as follows:

1. A first oxydation gives oxygen by simple addition, with formation of an aldehyd or acetone:

thus $\text{€}_2\text{H}_4+\Theta=\text{€}_2\text{H}_4\Theta$ (aldehyd)

$\text{€}_3\text{H}_6+\Theta=\text{€}_3\text{H}_6\Theta$ (acetone and propionic acid.

2. A further action—always on the free hydrocarbon—generates monobasic acids;

$$\text{€}_2\text{H}_4+\Theta+\text{H}_2\Theta=\text{€}_2\text{H}_4\Theta_2$$
$$\text{€}_3\text{H}_4+\Theta+\text{H}_2\Theta=\text{€}_3\text{H}_6\Theta_2.$$

3. Finally, as already shown, the same hydrocarbons by the action of the alkaline hypermanganates yield bibasic acids:

$$\text{€}_2\text{H}_4+2\Theta=\text{€}_2\text{H}_4\Theta_4$$
$$\text{€}_3\text{H}_4+2\Theta_2=\text{€}_3\text{H}_4\Theta_4.$$

Thus the direct and regular oxydation of the hydrocarbons yield successively aldehyds, monobasic acids and bibasic acids.—*Comptes Rendus*, lxx, 256. W G.

6. *On the Ethyl compounds of Thallium.*—Hansen has succeeded in replacing two atoms of chlorine in thallic terchlorid by ethyl and obtaining the chlorid of thallium-diethyl with corresponding oxygen salts. When thallous chlorid, $TlCl$, is suspended in ether and a current of dry chlorine gas is passed in, a yellow fuming liquid is obtained which becomes brown in the air. Zinc-ethyl acts powerfully on this liquid with strong evolution of heat—and when an ethereal solution of zinc-ethyl is mixed with one of thallic terchlorid and the product of the reaction shaken with dilute chlorhydric acid a white curdy crystalline mass is obtained after the distillation of the ether which after purification is the chlorid of thallium diethyl, $Tl(C_2H_5)_2Cl$. The new chlorid is soluble in hot water, ether and alcohol and crystallizes in silky scales. It blackens in the light like argentic chlorid—a property which it also according to the author—shares with thallous chlorid. When suddenly heated it explodes with formation of thallous chlorid and a gas, which is a mixture of ethylene and ethyl-hydrid so that we have

$$Tl(C_2H_5)_2Cl = TlCl + C_2H_4 + C_2H_6.$$

By double decomposition with argentic sulphate and nitrate the author obtained the sulphate, $[Tl(C_2H_5)_2]_2S\Theta_4$, and the nitrate $Tl(C_2H_5)_2N\Theta_3$ which crystallize in leaves and are soluble in water, alcohol and ether. The author promises a further investigation of this very interesting and theoretically important subject. —*Berichte der Deutschen chemischen Gesellschaft*, No. 1, 1870, p. 9. W. G.

7. *Spectrum Analysis: Six Lectures delivered in* 1868 *before the Society of Apothecaries of London.* By Henry E. Roscoe. London. C. Macmillan & Co.—We are indebted to the kindness of the author for a copy of this work, which presents the most complete account of the history and development of spectral analysis to be found in any language, if we except perhaps the German treatise of Schellen and the Swedish work of Thalen.* The six lectures form rather less than half the work. They are simply and agreeably written, not too popular, nor yet too abstruse for a popular work in the best sense of the term. Their value is greatly increased however, for the scientific reader, by the addition of numerous and elaborate appendices including in many cases entire memoirs. Besides the diagrams and figures which illustrate the lectures, the work is enriched by an excellent lithographic reproduction of Kirchhoff's chart from A to G and of Angström's and Thalen's chart from G to H, and with chromo-lithographs of the spectra of several fixed stars and nebulæ. Kirchhoff's and Huggins' tables are given in full. The results of the analysis of the light of the sun's atmosphere up to the date of the publication of the lectures are also given.

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* Om Spektral analys. Upsala Universitets Arsckrift, 1866.

Finally there is an excellent bibliography of works and memoirs on the spectroscope and spectral analysis. That the work should not in every respect deserve unqualified praise will seem natural enough, yet we are not disposed to find fault with so rich a store of information offered to us in so acceptable a form. W. G.

II. MINERALOGY AND GEOLOGY.

1. *On the Megadactylus polyzelus of Hitchcock;* by E. D. Cope.—This genus was named by Hitchcock in his Supplement to the Ichnology of New England, p. 39, 1865; the bones have been briefly described in his Ichnology on page 186. The remains were found in a more or less fragmentary condition in the red sandstone rocks of the Valley of the Connecticut, from the neighborhood of Springfield, Massachusetts. They were found by William Smith, while engaged in superintending some excavations made at the armory, which required blasting.

The remains consist of four caudal and one dorsal vertebræ, the greater part of the left fore foot, with distal portions of ulna and radius; the greater part of the left femur, proximal end of left tibia, greater part of left fibula, tarsus and hind foot, including a tarsal bone, perfect metatarsus, proximal end of a second metatarsus, parts of the distal end of a third, and parts and impressions of four phalanges.

These fragments demonstrate the former existence in the region in question of a typical form of the suborder or order Symphypoda (*Compsognatha* Huxley), and one nearer the birds than any other hitherto found in America. Its pertinence to this order is shown by the absence of the first series of tarsal bones, apparently, as Gegenbaur has suggested, and as the structure of Lælaps proves, in consequence of their confluence with the distal extremities of the tibia and fibula. This important character is apparently assumed early in life in the present genus, and in Compsognathus, and probably quite late, in Ornithotarsus. In Compsognathus the additional peculiarity of the persistence of but two carpal bones is presented, which, according to Gegenbaur, should correspond with those of the first row of ordinary Reptilia, while those of the second have disappeared. In Megadactylus those of the first series are present, viz: the radiale and probably ulnare, and one of the second row, very much reduced, opposite to the second metacarpus. There is space for a second one of the second series, but it does not appear in the matrix, while the ulnare is probably lost.

The bird-like tendencies of the Symphypoda have been indicated above, and the very ornithic character of the bones of the present form is also very marked. The walls of the long bones are very thin, in some places near their extremities almost as much so as writing paper. The vertebræ and ischia present the same thin walls. The structure of these walls is exceedingly dense.

Prof. Cope next gives the special characters of the bones, which are here omitted. He adds:

That animals of this genus made some of the tracks similar to those of birds in the red sandstones of the Valley of the Connecticut there can be no doubt. It furthermore explains some problematical impressions which are occasionally found with them. Tracks of an animal resting in a plantigrade position, as indicated by the moulds of two long parallel metatarsi, each terminated by three toes, are accompanied by a peculiar, bilobate, transversely oval mark on the middle line, some distance behind the heels.

Prof. Hitchcock states that it appears to be the impression of a short stiff tail. The present specimen shows clearly that it was made by the obtuse extremities of the ischia. The saurian squatted down, resting on its styloid ischia as the third leg of a tripod of which the anterior pair was represented by the hinder legs. Prof. O. C. Marsh informs me that in the museum of Yale College, a slab exhibiting impressions similar to the above shows the impressions of the anterior feet also, which were put to the ground in the act of rising or sitting, or perhaps reached to it while the animal was squatting, as do those of carnivorous Mammalia.

The tracks of many of the animals discovered by Hitchcock are plantigrade. That they could not have walked like the plantigrade mammal, is sufficiently evident from the length of the metatarsal elements, which would necessitate a constant contraction of the tibialis anticus muscle, or peculiar arrangement of the tarsal bones, for its support. The latter does not appear to have existed, and the former is so very improbable, that, in connection with the pneumatic structure of the bones, there is abundant reason to suppose that they progressed by leaps, and assumed the plantigrade position when at rest.

No portion of the cranium or dentition of this genus has been preserved. The large stout hooked claws of the fore foot would indicate a more or less carnivorous diet.

The existence of Symphypoda in the strata here indicated, with the occurrence of a Pterosaurian in a similar situation in Pennsylvania, points to the existence of the transition from Keuper to Lias, that is, from Triassic to Jurassic beds, in the red sandstones of eastern United States. They have been heretofore regarded as Triassic,* which the lower portions of them undoubtedly are, and similar to the German Keuper in the presence of Labyrinthodonts, Thecodonts and Dinosauria in both Pennsylvania and N. Carolina.

The remains here described were alluded to by Prof. R. Owen, as those of a Saurian pointing to the Pterodactyles or Birds, providing the cavities of the bones were filled by marrow, and not by cartilage. Prof. Wyman regarded them as those of a reptile, though the long bones might have been referred to a bird if considered alone. "While the bones from Springfield are as hollow as those of the Pterodactyle, I do not find that they are those of this animal; there is no positive proof of the long fingers nor of the broad sternum which these reptiles possessed. The existence of the large toe in company with the small one is in favor of a jumping animal."—*From the Memoir of Prof. Cope on Extinct Reptilia and Aves, Amer. Phil. Soc., unpublished volume.*

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* Hitchcock, in his Ichnology (1858), holds that the beds containing the tracks are lower Jurassic, either Oolitic or Lias; and Dana, in his Geology, (pp. 414. 443), says that the so-called Triassic is probably in part Jurassic.—Eds. Am. J. Sci.

2. *On the Elasmosaurus platyurus of Cope;* by Dr. J. LEIDY. (Communicated by the Author).—At a meeting of the Academy of Natural Sciences of Philadelphia, March 8th, Prof. Leidy stated that after an examination of the remains of the great marine saurian, from the Cretaceous formation of Kansas, presented to the Academy by Dr. T. H. Turner, U. S. A., and described by Prof. Cope under the name of *Elasmosaurus platyurus*, he had arrived at the conclusion that the animal belonged to the Enaliosaurians. It was closely allied to *Plesiosaurus;* the peculiar characteristics of the different regions of the vertebral column, together with those of the shoulder and pelvic girdles, and the fragments of the skull and teeth, are decidedly Plesiosaurian.

Prof. Cope has fallen into the error of describing the skeleton in a reversed position to the true one, and in that view has represented it in a restored condition in his recent "Synopsis of the Extinct Batrachia, Reptilia, and Aves," published in the Transactions of the American Philosophical Society. To explain the apparently anomalous and reversed condition of the articular processes, (zygapophyses) of the vertebræ, he considers that those ordinarily existing in animals are substituted by the second set (zygosphene and zygantrum) of serpents and iguanians.

The discovery of a portion of the skull, as reported by Dr. Turner, in the vicinity of what Prof. Cope regards as the anterior extremity of the skeleton, and which he considers as confirmatory of the view he has taken of the latter, Prof. Leidy remarked, independently of the many anatomical characteristics, is more than compensated by the opposite end of the vertebral column terminating in a coossified axis and atlas, this latter still retaining in its cup the occipital condyle.

A comparison of caudal vertebræ of the Kansas saurian with isolated specimens from the Cretaceous formations of Alabama, Mississippi, and New Jersey, referred by Prof. Leidy to a Plesiosaurian, under the name of *Discosaurus*, leads him to view *Elasmosaurus* as identical with it. Such also appears originally to have been the view of Prof. Cope, in relation to a part of the same skeleton which he referred to a species with the name of *Discosaurus carinatus.*

The restored *Discosaurus* or *Elasmosaurus*, would repeat the form usually given of *Plesiosaurus*, but the neck was of more remarkable length than in the latter. It comprised the almost incredible number of *seventy-two cervicals*, and measured almost twenty-two feet in length, independent of the head. The imperfection of the rest of the vertebral column does not permit anything like a positive estimate to be made of the comparative extent of the trunk and tail.

In the true view of *Discosaurus* or *Elasmosaurus*, Prof. Cope's order of Streptosauridæ fails to maintain its ground.

3. *Ornithopsis, a gigantic animal of the Pterodactyle kind from the Wealden;* by H. G. Seeley (Ann. Mag. N. H., IV, v, 279).—Under the above title Mr. Seeley describes two vertebræ in the British Museum, one from Tilgate and the other from the Isle of Wight, which "are of size and structure and texture such that both might well have belonged to the same kind of organism," and probably the same animal. One vertebra is from the lower part of the neck, and the other from the back; and when perfect the former, from the back to the front of the centrum, could "scarcely have measured less than ten inches." "Seven such vertebræ would have made the neck 4 to 5 feet long, and the animal 10 to 12 feet," while, according to the author, it may have been two or three times as high. The vertebræ are constructed after "the lightest and airiest plan" peculiar to Pterodactyls and birds; they have pneumatic foramina as in these species, and these are very large, like those of the former species. The animal must therefore have been decidedly ornithoid; and the gigantic ornithic foot-prints of the Wealden, described by Mr. Beccles and Mr. Tyler, may have been its tracks. The author closes his paper with naming the species *Ornithopsis Hulkei*, after Dr. Hulke.

4. *Volcanic action on Hawaii;* copy of a letter from Rev. Titus Coan to Prof. Chester S. Lyman, dated Jan. 24, 1870.—"Our volcanic craters have not made great demonstrations of late, and yet are not quiet. Slight shocks of earthquakes often occur, sometimes one, two or three in a day. During the first two weeks of the present month a good deal of steam and smoke arose from *Mokuaweoweo*, the summit crater of Mauna Loa. In Kilauea the action is fitful. Occasionally the fires rage with much violence, and again they are sluggish. When I was there in August, the old south lake, Halemaumau, was a hundred feet deep, and four-fifths of a mile in diameter on the bottom. On this floor there were eight fiery ovens and orifices open. Since then there have been several vivid overflowings. These, with the slowly acting uplifting forces, have raised the bottom of the crater some 75 feet, so that now the latest facts are that the bottom is within 25 feet of the upper rim, and it is supposed that the pit has been enlarged to more than a mile in diameter. Lord Charles Hervey and Dr. Hans Berag, a Prussian savan, have made two visits there within the past month. They also rode on mules, in company with Judge Hitchcock of Hilo, to the terminal crater of Mauna Loa, and looked into Mokuaweoweo. There was no fire seen, but much steam. These gentlemen took a newly discovered route, which proved much easier than any before known. A cattle ranch has been established at Kapapala, and a milk and butter station is situated a mile higher up the ridge of the mountain. From this upper station the cattle have found their way nearly to the summit, and the herdsmen in search of them have found that mules could reach Wilkes's camp without difficulty. Starting from Kapapala as a 'base of supplies,' you can go nearly to the summit the first day. On the second day you

can ascend to the top, spend several hours, and return to camp to sleep. On the third day you can reach Kapapala ranch before night. It is also now probable that the same could be done from Kilauea as a base."

5. *Geological Map of Canada and the Northern United States;* by Sir W. E. Logan.—We have at length the pleasure of announcing the appearance of this important and long looked-for contribution to American geology. Although it bears the date of 1866, its publication has, for some reason, been delayed until 1869. A first copy was shown by Dr. Hunt at the meeting of the American Association at Salem, last summer, and it is only within a few weeks, as we are informed, that the Geological Survey of Canada has been able to procure a small number of colored copies for distribution, one of which is now before us. The legend of the map informs us that the geological details for Canada, comprising the former provinces of Upper and Lower Canada (now Ontario and Quebec) are furnished by the Geological Survey under the direction of Sir William E. Logan. He has himself compiled the geology of the various states of the Union under the supervision of Prof. Hall from various sources which are mentioned in detail in the preface to the Atlas of the Geology of Canada; (published in 1865) where he tells us that this portion of the work was done "with the approval of Prof. James Hall, who has freely placed all his materials at the disposal of the compiler, and aided by his intimate personal knowledge of the geology of a greater part of the region represented." For the geology of the province of New Brunswick, Nova Scotia and Newfoundland, also, the most authentic printed and manuscript maps were consulted, as described in the Atlas just referred to.

An indispensable preliminary to a work of this kind was a correct topographical map, and such a one for Canada had to be slowly and laboriously constructed. The sources of information for this purpose are given at length in the preface already quoted. A series of longitude determinations was made, by electric telegraph, of various points from Chicago to Quebec and Halifax, and both of the latter stations were directly compared with Cambridge. In the absence of a regular trigonometrical survey of the provinces, the U. S. boundary surveys, the lake surveys of the U. S. Topographical Engineers, and the hydrographical surveys of the British Admiralty were available for the course of the St. Lawrence and the British shores, while those of the U. S. Coast Survey were followed for the United States. For the interior of the provinces, in addition to the surveys already existing, great numbers of topographical surveys have been made by the officers of the Geological Survey during the past twenty-five years. From all this material, described in the Atlas, pp. 8–15, it has been possible to make a far more accurate delineation of the geography of the British provinces than has hitherto been attained, and the same remark will apply to the coastal region of the United States, since we believe that this is the first time the beautiful and accurate work of our own Coast Survey has ever been reproduced in a complete form for our shores from the St. Croix to the Chesapeake. The construction of the map was intrusted to Mr. Robert Barlow, formerly of the British Ordnance Survey, and now chief topographer to the Survey of Canada. It has been engraved on steel by Ramboz and Jacobs of Paris, under the superintendence of Mr. Gustave Bossange, and is remarkable for the beauty of its execution. The geological lines having been traced upon the plate, they were placed in the hands of Mr. Stanford, the well known map publisher of London, under whose direction the printing and hand coloring of the map have been executed. The Atlas already referred to contains a small colored geological map, on the scale of 125 miles to the inch, which is a reduction of that now before us, and bears the date of 1864. Changes and additions to the geology have, however, been made in the large map, on which it has been possible also to give the subdivisions of the Quebec group in Eastern Canada, which the small scale of the first map did not allow.

The present map is on the scale of twenty-five miles to the inch, and measures eight feet from east to west by three and a half feet from north to south; extending southward to latitude 37°, and westward to longitude 100°. Its northern limits include the lakes Manitoba and Winnipeg, James's Bay, Newfoundland, and the adjacent Labrador coast, while to the south it takes in Kanzas and northern Virginia.

The geological subdivisions adopted on this map are—1. Laurentian, 2. Labradorian (or Upper Laurentian), 3. Huronian; while for the paleozoic series the names and divisions of the New York survey are essentially adopted, as follows: 4. Potsdam formation, 5. Calciferous, 7. Chazy, 8. Birdseye and Black River, 9. Trenton, 10. Utica, 11. Hudson River, 12. Medina and Oneida, 13. Clinton, 14. Niagara, 15. Guelph, 16. Onondaga or Salina, 17. Lower Helderberg, 18. Corniferous and Oriskany, 19. Hamilton, 20. Chemung and Portage, 21. Old Red sandstone, 22. Lower Carboniferous limestone, 23. Bonaventure (conglomerate), 24. Coal measures, 25. Upper Carboniferous limestone, 26. Permian, 27. Trias, 28. Cretaceous, 29. Tertiary. In the eastern basin, as is well known, the geological survey of Canada admits the existence of a Quebec group, which is regarded as equivalent of the Calciferous and Chazy formations and is divided into three parts in ascending order, viz: the Levis, Lauzon and Sillery formations. These are represented on the map before us; the Levis being colored like the Calciferous, and the Sillery like the Chazy, while the Lauzon (6), is distinguished by a separate color. Add to this two colors for intrusive rocks, one for granites and the other for diorites and dolerites, and we have not less than thirty-one geological divisions, indicated on the map by as many colors. The system of coloring adopted by Sir William Logan is essentially that of the Survey of Great Britain, with such modifications as were required to adapt it to our American geology, and has the merit of bringing into distinct view the great geological divisions without offending the eye by crude and harsh contrasts.

With the exception of the reduction of this in the Atlas, it is believed that no geological map has appeared which presents to the student a connected view of so great an area of the continent. It extends from the Cretaceous and Tertiary rocks of New Jersey, to those of Nebraska and Dakota, and shows at a glance by far the greater part of the wide paleozoic basin of North America. It may at first seem strange that a map designed primarily to display the geology of Canada should be made so comprehensive; but it will be seen that the proposed limits of the New Dominion extend even farther westward than this map, while the southern point of the province of Ontario stretches as far as northern Pennsylvania, or below the 42nd parallel. A clear understanding of the geology of the upper St. Lawrence basin, was, moreover, not possible without a delineation of the great coal fields adjacent, whose relation to Canada, it should be added, is not less important commercially than geologically. These coal fields now furnish large supplies of fuel to the middle and western portions of the Dominion, which, in return, is sending to the coal regions rich ores from its inexhaustible mines of iron,—the commencement of a commerce which must grow in importance, and bind more closely these provinces to our great republic.

On the other hand, we cannot fail to be struck with the extent of the Acadian coal basin, including large portions of Nova Scotia, and New Brunswick, and part of Newfoundland. Out of it, in fact, the Gulf of St. Lawrence has been excavated; and this wide maritime area, with its thick seams of superior bituminous coal, contiguous to safe harbors, and not far removed from the great manufacturing districts of New England, must every year increase in importance alike to the provinces and to the Union. We are tempted to dwell still farther on the great commercial questions raised by the inspection of this geological map, which the geographer, the merchant, and the statesman may consult with equal advantage; but we must confine ourselves to its geological aspects.

For that part of the United States which lies between the Mississippi and the longitude of the Hudson, the geological lines are now so well defined that, except for some parts of the Appalachian belt, no subsequent researches will probably necessitate any considerable change in the map. The geological structure of the provinces of Ontario and Quebec, south of the great Laurentian region which stretches along the north side of the St. Lawrence basin, has been wrought out with an accuracy rarely surpassed, and requires a map on a much larger scale to exhibit it in detail. We have lately seen such a map, about to be published by the Geological Survey of Canada, which, extending from a little west of Montreal to a little east of Quebec, includes the region between that portion of the St. Lawrence and the frontier of the United States. It is engraved on a scale of four miles to the inch, and geologically colored to exhibit in detail the complicated structure of the Canadian extension of the Appalachians, the so-called Notre Dame range. Its publication is delayed by the want of topographical details for some portions, but the map will soon appear, and it is proposed to follow it by the publication of maps of other sections, on the same scale. (See the Atlas, page 23.)

During the three years which have elapsed since the engraving of the present map, considerable progress has been made in investigating the geology of the maritime British provinces. The published reports of Mr. Murray upon Newfoundland show that besides the Laurentian, Potsdam and Quebec rocks of the northwestern portions, extensive developments of Laurentian, Huronian and Primordial Silurian occur towards the southeast, higher rocks, including an area of coal measures, occupying the intermediate portion. This work of Mr. Murray, if continued, will ere long give us a correct notion of the geology of the whole island.

Coming now to the Acadian provinces (Nova Scotia and New Brunswick) much new work has been done, though very much remains to be accomplished there. The map accompanying Dr. Dawson's Acadian Geology (1869) adds something to the present one, and gives a part of the results from the labors of Matthews, Bailey and Hartt. We now know of the existence in New Brunswick, to the south of the coal fields, of considerable developments of Primordial and Upper Silurian rocks, each with a well characterized fauna, and also of a remarkable Devonian flora. A belt near the southern coast of this province, once regarded as of igneous rocks, has been found to belong to an ancient stratified series, probably Huronian, interposed between the Primordial strata and a band of still more ancient rocks, once designated granites, and now recognized as highly crystalline gneisses of Laurentian age. It is probable that the more considerable granite areas now indicated in other parts of New Brunswick will be found, on further examination, to be ancient indigenous rocks as already suggested by Hind and by Dawson, and in part, at least, of Laurentian age. The same remark will apply to Maine and Nova Scotia.

The existence of Laurentian and Huronian rocks to the southeast of the Carboniferous area in Newfoundland and in New Brunswick, and the recent detection by Dr. Hunt of a belt of Laurentian in eastern Massachusetts, leads us to hope that we are approaching to a comprehension of the geological structure of New England. We look for much in this connection from the new geological survey of New Hampshire, under Professor C. H. Hitchcock, and confidently expect that in a second edition of the map before us, which will soon be required, the geology of the New England states will no longer be a partial blank. To the geological student who is familiar with the region, this state of things conveys no reproach. The wide differences in original condition and volume between the sediments of the contiguous western and eastern basins, the comparative rarity of calcareous deposits throughout the paleozoic series in the latter, and its highly altered and crystalline condition, have hitherto presented insuperable difficulties in the way of unravelling the geological structure of this eastern region.

It is to be hoped that the government of the New Dominion will make liberal provision for the distribution of this valuable map. Meanwhile, a geological map, on the same scale and plan, of the southeastern United States is greatly needed to supplement this admirable publication of the Geological Survey of Canada, and we trust that a hand to prepare and means to publish it will not be wanting.

5. *Labradorite Rocks at Marblehead;* by T. STERRY HUNT.—The following note, appended to my paper on Norite Rocks in the last number of this Journal, was accidentally omitted. In speaking of the boulders of labradorite rocks at Marblehead Neck, I said "specimens of this rock, correctly determined and labelled, are found in the collection of the Essex Institute at Salem. To these my attention was called at the time of the meeting in August last, by Prof. C. H. Hitchcock, after which, in company with Dr. G. B. Loring, Prof. Packard and Prof. Kerr, I visited the locality at Marblehead Neck and collected further specimens of the characteristic labradorite rock."

Montreal, March 18, 1870.

6. *Explorations in the Rocky Mountains by J. D. Whitney.*—Professor J. D. WHITNEY has given the California Academy of Sciences some of the results of explorations under his direction in the Rocky Mountains during the summer vacation of 1869.

The party, which was well provided with instruments for topographical, astronomical, and barometrical work, consisted chiefly of professors and students from the Mining School of Harvard University, and was also accompanied by Professor Brewer, of the Yale Scientific School, and Mr. C. F. Hoffmann, of San Francisco. A careful triangulation was made of the dominating range of the Rocky Mountains between Gray's Peak and the south edge of the South Park, and a map drawn by Mr. Hoffmann, on a scale of two miles to an inch, embracing an area of about 3,500 square miles. This map includes the whole of the South Park and its vicinity; but not the whole of the main divide of the Rocky Mountains, that portion which lies to the northwest of the head of the Arkansas river being necessarily left to be completed at a future time. It is hoped that it will be possible to extend the topographical work to the north and west, so that a detailed map may be prepared of the whole of the highest portion of the Rocky Mountains.

Among the results obtained by this exploration was the determination of the elevation of some high points not previously measured. The highest peak ascended lies to the west of the Arkansas, and it surpasses in elevation any yet observed in the Rocky Mountains. It was named Mount Harvard, and found to be 14,270 feet in height. The next highest points are: Gray's Peak, which was ascertained to be 14,145 feet in elevation; Mount Lincoln, 14,123 feet; and Mount Yale, 14,078 feet. Many other points were measured, but these were the only ones that were found to be over 14,000 feet high.

Dr. Parry is the only other explorer who has published any measurements of the peaks of this region. Having, however, no station barometers nearer than St. Louis, his results are liable to considerable uncertainty, as is shown by the fact that his elevation of Denver was found by the spirit-level surveys of the Central Pacific and Denver roads to be 282 feet too great, a result closely corroborated by the Kansas Pacific Railroad surveys. Dr. Parry also obtained for the height of Gray's Peak a result one hundred feet greater than ours. In every other instance where observations were taken by the Harvard Mining School party at stations previously occupied by Dr. Parry, the results of the latter are found somewhat too high, the discrepancy varying from 50 to 450 feet. This would indicate that the elevation of Pike's Peak, given by Dr. Parry at 14,216, may also be a little too high. But, to obtain the necessary data for working up to the last degree of accuracy the barometrical observations taken in this region, it will be necessary that stations be made on the plains at the base of the mountains—say at Denver—and at some point as high up as possible—as, for instance, Georgetown or Montgomery—and the observations continued for at least one year synchronously at the two stations.

In the meantime, it will be convenient to have the approximate heights of all the points in the Rocky Mountains yet measured, and which exceed 14,000 feet in elevation. They are as follows:

	Feet.	
Mount Harvard	14,270	
Gray's Peak	14,245	(Parry)
Pike's Peak	14,216	(Parry)
Mount Lincoln	14,123	
Mount Yale	14,078	
Long's Peak	14,050	(?)

The result here given for Gray's Peak is 100 feet greater than that obtained by the Harvard party. That for Long's Peak is an estimate based on a barometrical observation by Messrs. Powell and Byers, without any corresponding base observation; the barometer stood at 18·100 inches.

From the above it will be seen that no point has yet been found in the Rocky Mountains as high as several in the Sierra Nevada. It will also be noticed as a remarkable coincidence how little the highest points differ from each other in elevation.

It is thought by some that there are still higher peaks than any yet measured to the southwest of Mount Harvard and Mount Yale, in the yet unexplored regions lying between the Arkansas and the Grand. This party was unable to carry its work so far in that direction as would have been necessary in order to decide that point.

The other results of this expedition will be worked out and published in due time.

7. *California Geological Survey.*—All geologists, and all interested in the development of the resources of our country, will rejoice to learn that the resumption of the Geological Survey of California, under Prof. J. D. Whitney, has been ordered by the Legislature of the State. An appropriation of 2,000 dollars per month for two years has been made, "to complete the field work, and publications." Besides this a deficiency appropriation of 25,000 dollars has been passed. The act with reference to this continuance of the survey is entirely satisfactory in all respects. It was chiefly due to the influence and energy of Hon. E. Tompkins, of the Senate, that the Survey bill was carried through both branches of the Legislature by a large majority.

8. *The Geological Survey of Ohio; its Progress in* 1869. Report of an Address delivered to the Legislature of Ohio, Feb. 7th, 1870, by J. S. NEWBERRY, Chief Geologist. 60 pp. 8vo.—Dr. Newberry, the able head of the Geological Survey of Ohio, presents in this report a general review of the organization of the corps for the survey under his charge, his plan and aims, and a brief statement of the geological structure of the State, and the important objects to be secured by the investigations in progress. Some of the results arrived at during the past year have already been mentioned in the last volume of this Journal, at page 417.

9. *Sketches of Creation;* a popular view of some of the grand conclusions of the sciences in reference to the history of matter and of life; together with a statement of the intimations of science respecting the primordial condition and the ultimate destiny of the earth and the solar system; by ALEXANDER WINCHELL, LL.D., Prof. of Geology, Zoology, and Botany, in the University of Michigan, and Director of the State Geological Survey. 460 pp. 12mo, with many illustrations. New York, 1870. (Harper & Brothers).—The scope of Professor Winchell's work is well made known in its title. It is written for the popular reader; and while its sentences may be regarded as too rhetorical by the scientific student, they may attract others to the subject, and aid many in appreciating the grandeur and bearing of the truths which have been brought to light by geological investigation.

10. *Isomorphism of Gadolinite, Datolite and Euclase.*—RAMMELSBERG has pointed out in the *Zeitschrift* of the German Geological Society (vol. xxi, p. 807, 1869) the isomorphism of Gadolinite with datolite. He cites the fact that DesCloizeaux has ascertained through optical examination the crystallization of Gadolinite to be monoclinic (Ann. Ch. Ph., IV, xviii); and shows by chemical analyses that the ratio of the oxygen of the silica to that of the bases is 2 : 3 as in the other species above named. The obliquity (or angle C)=89° 28′ and I : I=116°.

Rammelsberg also shows, by a comparison of angles, the isomorphism of Datolite and Euclase. As he does not mention the fact, we add that this isomorphism was announced and proved sixteen years since in this Journal, (II, xvii, 215, 1854), by J. D. Dana, and also in Dana's Mineralogy, 4th ed., 1854, p. 204, where the Euclase group of isomorphs is made to include *Datholite*, *Euclase*, *Sphene* and *Zoisite*, to which he soon after (ib., II, xix, 363) added *Keilhauite*. The isomorphism is mentioned also in the last edition of Dana's Mineralogy (1868); and moreover the similarity in chemical composition (which Rammelsberg here exhibits) is there brought out (on pp. 362, 363), both under the new and old system of chemical formulas.

10. *Mineralogical contributions of* G. VOM RATH, of Bonn.—In Poggendorf's Annalen, vol. cxxxvi, p. 405, vom Rath has published one of his valuable mineralogical papers, containing the following articles: on the crystallization of Vivianite; the chemical formula of Eulytite (silicate of bismuth), with observations on its crystallization and other characters; on the crystalline form of Atelestite, which he makes monoclinic, with c (clinodiagonal) : b : a (vert.)$=0·869:1:1·822$; and a on $c=110° 30'$; on the Labradorite of Närödal on the Sognefiord in Norway, with analyses; on the Boulangerite of Silbersand near Mayen, with analyses. For the Eulytite of Schneeberg he found $\ddot{\mathrm{Si}}$ 16·52, $\overline{\mathrm{Bi}}$ 82·23, $\dddot{\overline{\mathrm{P}}}$, $\overline{\mathrm{Fe}}$ 1·15=99·90; again, $\ddot{\mathrm{Si}}$ 15·93, $\overline{\mathrm{Bi}}$ 80·61, $\dddot{\overline{\mathrm{P}}}$ 0·28, $\overline{\mathrm{Fe}}$ 0·52=97·34; whence the formula $\overline{\mathrm{Bi}}^2\ddot{\mathrm{Si}}^3$. This places the species among the Unisilicates.

This paper contains also a notice of a crystalline compound of Zinc and Calcium, corresponding to the formula $\mathrm{Zn}^{12}\mathrm{Ca}$=Zinc 95·13, Calcium 4·87=100. He found 95·11 and 4·90. Its crystals are small square octahedrons, having the angle of the terminal edges 134½°, and of the basal 66° 18′, whence the vertical axis=0·4619.

11. *Reale Comitato Geologico d'Italia.*—The Italian Government has appointed a Geological corps, for the publication of a large geological chart of Italy, with descriptions of the geological structure of the country. The corps includes Prof. J. Cocchi, of Florence, President, Bartolommeo Gastaldi, Professor of Mineralogy at Turin, F. Giordano, Inspector of the Royal Corps of Mining Engineers, G. Meneghini, Prof. of Geology at Pisa, and Ludovico Pasini, Senator of the Kingdom of Italy; with the assistants Pietro Zezi, Mining Engineer, Giuseppe Grattarola and Angelo Alessandri, Geological Assistants, and Felice Momo, "Aspirante." The 1st and 2d numbers of the Bulletin (Bolletino) were issued in January and February of the present year; and the 1st volume of the Memoirs, to contain papers on Italian Geology, will soon be published. In the February number of the Bulletin, Prof. Cocchi has an abstract of a paper on Elba, and E. Bechi an article on the composition of a dark-colored volcanic rock of Monte Catini containing *mica*, called *Selagite*, (named by Santi *Lava limacciosa micacea*), and also of a prehnitoid rock from the same locality, and of the prehnite of Impruneta. The mica of the selagite was found to consist of

$\ddot{\mathrm{Si}}$ 40·8, $\dddot{\overline{\mathrm{Al}}}$ 22·1, $\overline{\mathrm{Fe}}$ 21·0, $\dot{\mathrm{Mg}}$ 0·5, $\dot{\mathrm{Ca}}$ 5·6, $\dot{\mathrm{K}}$ 5·9, Fl 0·8, $\dot{\mathrm{H}}$ 3·5=100·2.

It has G.=3·15, and H.=2·5. The paste or rock material containing the mica was found to consist of

$\ddot{\mathrm{Si}}$ 44·5, $\dddot{\overline{\mathrm{Al}}}$ 1·7, $\dot{\mathrm{Fe}}$ 37·3, $\dot{\mathrm{Mg}}$ 1·4, $\dot{\mathrm{Ca}}$ 14·0, $\dot{\mathrm{Na}}$, $\dot{\mathrm{K}}$ (*tr*). $\dddot{\overline{\mathrm{P}}}$ 1·1=100. H=4·5, G=2·789.

12. *Gibbsite and Wavellite.*—HERMANN (Bull. Soc. Impér. Nat. Moscow, No. 4, 1868, 496), has analyzed a grayish pearly mineral, occurring on limonite, in Chester Co., Pa., in thin crusts, delicately concretionary under a lens. It has a hardness of 3·0, G.=2·35, and is subtranslucent. He obtained

$\dddot{\overline{\mathrm{Al}}}$ 63·84, $\dot{\overline{\mathrm{H}}}$ 33·45, $\ddot{\mathrm{Si}}$ 1·50, $\overline{\mathrm{P}}$ 0·91, $\dot{\mathrm{Mg}}$, $\dddot{\overline{\mathrm{Fe}}}$ *trace*=99·70.

Hermann has obtained for the composition of the Wavellite of the same region (ib.):

$\overline{\mathrm{P}}$ 32·70, $\dddot{\overline{\mathrm{Al}}}$ 35·83, $\dot{\overline{\mathrm{H}}}$ 28·39, $\dddot{\overline{\mathrm{Fe}}}$ 3·08, Fl *tr.*=100,

agreeing nearly with the analysis by Genth. He found G.=2·30 and H.=3·5.

The Gibbsite (hydrargillite) of Villa Rica afforded Hermann (ib.): $\dddot{\overline{\mathrm{Al}}}$ 63·60, $\dddot{\overline{\mathrm{Fe}}}$ 2·00, $\dot{\overline{\mathrm{H}}}$ 34·40=100.

13. *Hermann on Samarskite and the compounds of the Columbium metals* (ib., p. 463).—Hermann here reviews the composition of different minerals containing the metals of the columbium group. He deduces for Columbite the formula $\dot{\mathrm{R}}\,\overline{\mathrm{R}}$, the second metal including the Columbium and related metals; for Samarskite and Æschynite, $\dot{\mathrm{R}}\,\overline{\mathrm{R}}$; for Tantalite $\dot{\mathrm{R}}^2\,\overline{\mathrm{R}}^5$. He makes the oxyds of the Columbium metals, $\overline{\mathrm{R}}$, $\ddot{\mathrm{R}}$, $\overline{\mathrm{R}}$, $\dot{\mathrm{R}}$.

14. *Minerals of Elba.*—Mr. ANTONIO D'ACHIARDI of Pisa, has described the crystals of several of the minerals of Elba, in an article published in the Nuovo Cimento, II, iii, Feb., 1870. The species included are Quartz, Wollastonite, Beryl, Epidote, Lepidolite, Tourmaline, Ilvaite, and Orthoclase. He also publishes the analysis of a Halloysite—which afforded

$\ddot{\mathrm{Si}}$ 55·15, $\dddot{\overline{\mathrm{Al}}}$ 27·72, $\dot{\mathrm{Mg}}$ and $\dot{\mathrm{Ca}}$ 5·10, $\dot{\mathrm{K}}$ 1·15, $\dot{\overline{\mathrm{H}}}$ 10·20=99·32.

It is soapy in aspect and feel; milk-white, with small black and pale flesh-red spots; opaque, but becoming translucent in water; and adheres to the tongue.

15. *Mineralogische Notizen, of* F. HESSENBERG.—The ninth number of Hessenberg's admirable crystallographic memoirs on minerals has recently been published by the Senckenberg Nat. Gesellschaft in Frankfort on the Maine (vol. vii, p. 257, 1870). It contains figures and measurements, with various observations, on the species Calcite from Lake Superior, Reissite of v. Fritsch, a rhombic Zeolite (perhaps a new mineral) from Santorin, Wollastonite, Periclin, Strontianite, Sphene, Caledonite, Hematite, Pyrite.

16. *Pencatite and Predazzite.*—The mineral carbonates Pencatite and Predazzite, which have been suspected to be only mixtures (see Dana's Min., 5th ed., 1868, p. 708) are proved to be such by G. Hauenschild, through a microscopic examination of specimens. He found them to be calcite, containing crystals of brucite.—*Sitzber. Ak. Wien.*, vol. lx, 1869.

17. *Rammelsberg on some native Tantalum and Columbium Compounds.*—In the *Zeitschrift* of the German Geological Society for 1869, at p. 586, Rammelsberg has a paper discussing the minerals *Tantalite*, *Yttrotantalite*, *Euxenite*, *Æschynite* and *Pyrochlore*.

In a paper commencing at page 106 of the same volume, Rammelsberg treats of the composition of *Silicates.*

18. *An Elementary Treatise on Quartz and Opal, including their varieties, with a notice of the principal foreign and British localities in which they occur;* by GEORGE WILLIAM TRAILL. New edition, greatly enlarged. 74 pp. 12mo. Edinburgh, 1870.—This beautifully printed little mineralogical volume contains descriptions of the many varieties of quartz and opal, with observations on their uses, localities, and other points of popular interest connected with them. The value of the work would be increased in another edition, by adding some account of the optical peculiarities of the species, about which there are many strange facts that might easily be made intelligible to minds little familiar with science.

III. BOTANY AND ZOOLOGY.

1. *How Crops Feed; a Treatise on the Atmosphere and the Soil as related to the nutrition of Agricultural Plants, with illustrations;* by SAMUEL W. JOHNSON, M.A., Prof. Analytical and Agricultural Chemistry in the Sheffield Scientific School of Yale College, &c. pp. 375, 18mo. New York: (Orange Judd & Co.) 1870. —This is the sequel to the treatise entitled "How Crops Grow," which was issued two years ago, and which has taken its place as an essential text-book. It was most promptly reproduced in England, very handsomely as respects the typography, &c., under the editorship of two Professors at the Royal Agricultural College at Cirencester,—by whom "numerous additions have been made to it, and in some parts it has been entirely rewritten." No means are furnished (at least we can find none) by which the reader can ascertain what the alterations and additions to this English edition are,—which seems unjust.

In the present volume, in considering "how crops feed," Prof. Johnson is in fullest force, upon ground that he has complete mastery of; and consequently he has produced a compendium of what is known of the chemistry and physics of vegetation which is eminently clear and satisfactory, and which supplies a long felt want. The first division of the book is devoted to the atmosphere, the second to the soil as related to vegetation; and the action of the various elements of each upon plants, especially the staple plants of agriculture, and of plants upon them, is in turn considered, with unusual perspicuity, and with about the fullness requisite for a textbook. From the biological side we might have wished to present some things somewhat differently,—to draw, for instance, a more marked line of distinction between vegetation as made up of organisms which, no less than animals, do work at the expense of organic materials and therefore produce or compose carbonic acid, and, on the other hand, as fulfilling the peculiar and essential work of vegetation, viz., the production of organic matter and the consequent decomposition of carbonic acid; and to distinguish also more definitely between this great operation which produces the materials for growth, and growth itself, which is the conversion of these into tissue or structure, and which of course goes on indifferently to light, which is all-essential to the former. The scientific reader will notice one of the author's strong and peculiar points, which is presented by way of conclusion in the final paragraph, viz., how several causes conspire to render the soil in a certain degree the conservator of its own fertility, by protecting its own resources from waste and too rapid use. A. G.

2. *Martius, Flora Brasiliensis*, fasc. 48: *Convolvulaceæ* expos. C. F. Meissner. Aug. 1869.—Prof. Meissner, who was obliged for a time to lay aside serious botanical work, here signalizes his return to it by a discriminating elaboration of the Brazilian *Convolvulaceæ*. The great genus *Ipomœa* is preserved in the extended sense, including *Calonyction*, *Quamoclit*, *Exogonium*, *Pharbitis*, &c., but with *Operculina* of Manso maintained as a genus. The work is extensively illustrated by 53 folio plates. A. G.

3. *Development of the Flower of Pinguicula vulgaris, &c.;* by Prof. A. Dickson of the University of Glasgow. A quarto memoir, from the Trans. Roy. Soc. Edinb. 1869.—The letter-press and three excellent plates illustrate the organogeny of the flower of *Pinguicula*, and make out that the ovary is probably composed of five carpels,—certainly not of two, notwithstanding the bilabiate stigma, —and the placenta from the first free-central, so that the order should be regarded as Primulaceous in type, and not particularly related to *Scrophularineæ;* and then the embryos of this genus and of *Utricularia* are investigated and figured; the fact of the single cotyledon in *P. vulgaris* is confirmed, as well as of the two in other species, and the undivided or rather undeveloped and so-called acotyledonous embryo in *Utricularia vulgaris.* A. G.

4. *A Geographical Handbook of all the known Ferns, with Tables to show their Distribution;* by K. M. Lyell. London, Murray, 1870, pp. 225, 8vo.—Fern-amateurs are not very uncommon in this country; in England they abound; and to know ferns, if not to collect or cultivate them, is among educated people, especially ladies, rather the rule than the exception; and of popular Fern-books there is no small choice. Among them all we shall hardly meet with so solid and faithful a volume as this by Mrs. Col. Lyell. It is not a descriptive work, but, as its title denotes, is a full catalogue of the Ferns known in every particular country, with the ranges in each; followed by a series of compact and admirable tables, in which the genera and species are systematically arranged, and the distribution through all the continents or regions, and their main divisions, clearly indicated. Of course the main sources of information are the works of Sir William Hooker, and particularly the *Synopsis Filicum*, brought out by Mr. Baker, to which may be added the latter's elaborate memoir on the distribution of Ferns, in the Transactions of the Linnæan Society. Yet this volume must have cost long labor, and the result is well worth it. A. G.

5. *Bulletin of the Torrey Botanical Club.*—These 8vo sheets (of which we have Nos. 1 and 2, for January and February) are issued by the Club, which holds its regular meetings in the Tor-

reyan Herbarium at Columbia College, and has special oversight of the botany of the region immediately around New York. Some of its brief articles are of more than local interest. For instance, Dr. Allen, in the first number, shows that the dwarf *Œnothera* of Montauk Point, Long Island, is not *Œ. linearis*, as has been thought, but a depressed form of *Œ. fruticosa*, dwarfed, no doubt by the bleak exposure; and Mr. Leggett, the editor, announces the discovery, by Mr. Brown, of flowers fertilized in the closed bud in *Aristolochia serpentaria*. In the second number, Mr. Leggett explains how it came to pass that *Lepidium Virginicum*, unlike all its congeners, has accumbent cotyledons, and proves that it is by no means so different as has always been supposed. Also that the embryo in this species and its near allies partakes of the character which has been deemed peculiar to *Subularia*, *Senebiera*, &c., viz., in the fold being above the base of the cotyledons, not at their junction with the radicle. Mr. Leggett writes that, "In the species which I have examined, viz., the present, *L. ruderale*, *L. campestre*, and, perhaps, *L. intermedium*, the cotyledons are continued, in the form of petioles, about half way down the radicle; the cotyledons, in fact, being transversely folded upon themselves, as stated and illustrated in Gray's Genera in the case of *Subularia* and *Senebiera*, the genera immediately preceding *Lepidium*. In the other species of *Lepidium* the plane of division between these petioles, or 'radicular' portions of the cotyledons, is parallel to the cotyledons proper, and consequently to the seed partition. In *L. Virginicum* this split is likewise parallel to the partition, and thus the 'radicular' portions of the cotyledons, is incumbent, and so far the species is in accordance with its congeners. Where the cotyledons expand into a blade, they are turned sharply at right angles to the partition and become accumbent. If the embryo be held with the edge of the cotyledons toward the eye, it is the left blade which comes from the back of the radicle, and thus has the longer turn to make. *Cakile Americana*, Nutt., resembles *Lepidium Virginicum* in these particulars, except that the 'radicular' portion of the cotyledons is relatively much shorter, and in one instance I found the blades of the cotyledons almost spirally bent over the radicle, so as to pass, as it were, through an incumbent stage. I have also observed this narrowing of the cotyledons into a petiolar portion, greater or less, in *Nasturtium*, *Cardamine*, *Arabis*, *Barbarea*, *Erysimum*, and *Raphanus*, so far as represented in our local Flora; but in all those genera, the 'radicular' split has conformed nearly or quite to the cotyledons, as they are incumbent or accumbent: in *Cardamine* it is long and somewhat inclined to one side." We have verified this statement as respects *Lepidium Virginicum*, and only wonder how this conformation has been overlooked. No. 3, for March, has also been received. A. G.

6. *Notes relating to Vegetable Physiology, &c.*—A part of the following memoranda are drawn from the last numbers of the *Bulletin* of the Botanical Society of France, especially its *Revue Biographique*, which is very faithfully edited.

The effect of Barberry-bushes in rusting Wheat, after having long been accounted a groundless popular superstition, is at length understood and admitted by the Cryptogamists. The botanists used to rebut the charges of the farmers by the statement that the rust in the grain fields and the prevalent fungus of the Barberry belonged to very different genera, and that therefore the one could not give origin to the other. But DeBary in Germany and Œrsted in Denmark, following up similar enquiries by Tulasne in France, have concluded that *Uredo*, *Puccinia*, and *Œcidium* are to be regarded, not as so many genera, but as three successive forms of fructification of the same fungus, or in some sort an alternation of generations. DeBary ascertained that the spores of the *Puccinia graminis* do not germinate when sprinkled on the leaves and stalks of the cereal grains, which this rust infests, while they will germinate on the leaves of the Barberry, and there give rise to the *Æcidium Berberidis:* and the spores of this are equally inert upon the Barberry, but will grow in their turn upon Wheat, and there reproduce first the *Uredo* or yellow rust and later the *Puccinia graminis* or dark rust. Another species of *Puccinia* equally produces its corresponding *Æcidium* upon Buckthorn; another alternates between the cereal grains and certain Boragineous weeds. These results have been practically tested, in the large way, last summer, in France. Long hedges of Barberry, planted along the Paris and Lyons railway in a commune in the Côte d'Or, were complained of by the adjacent cultivators, and were cut away at certain places by way of experiment; and an investigation by the railroad company, whose interests were adverse to such a decision, left no doubt of the injurious effects of the Barberry on the contiguous wheat fields.

The turning green of etiolated plants, or in other words the production of chlorophyll upon exposure to light, was found by Guillemin to take place much more promptly under diffuse light than in direct sunshine. Sachs, finding that the blanched plantlets of Maize, which he used, turned green more rapidly when shielded by a paper cover than when exposed to the sun directly, attributed the result to the greater elevation of the temperature of the confined air. But Famintzin showed that the shaded plantlets turned green first at a lower temperature than those unshaded. Prillieux, from whose paper, read last autumn before the Botanical Society of France, these facts are taken, confirmed this result in two other ways,—first, by submitting the etiolated plantlets to different intensities of the same light from which the calorific rays had been mainly screened out, and secondly by placing them in a cone of light at different distances from the focus of the condensing lens; those in the brightest light remained apparently unchanged in color during an exposure which had sufficed to develop a decided green hue in those of feebler light. Prillieux thinks the difference is wholly physiological; that chlorophyll is formed promptly under light of a certain proper intensity, and not beyond; that it is slightly if at all produced under that stronger or direct light which acting upon this same chlorophyll is so efficient in the decomposition of carbonic acid and the consequent evolution of oxygen gas.

Boussingault, in one of the latest of his admirable papers on the functions of leaves, published in the Ann. Sci. Nat., tom. 10 of the current series, comes to the conclusion that, though leaves destitute of chlorophyll do not decompose any carbonic acid, yet that they begin to do so as soon as any chlorophyll is produced.

Is light absolutely requisite to the decomposition of carbonic acid in green foliage, and how much is necessary?—These questions are satisfactorily answered by Boussingault, through a series of experiments in which green leaves were introduced into a mixture of carbonic acid and hydrogen, over mercury, and the evolution of oxygen tested by the introduction of phosphorus, and its phosphorescence in the dark, or by the white cloud in light. He thus found, 1, that leaves do not decompose any carbonic acid in the dark, nor in twilight after sunset; but they do so very well under the diffuse light of a northern exposure, and under ordinary shade; as is shown indeed by the vegetation which thrives in the shade of a forest and the like; 2, that this evolution of oxygen begins as soon as the leaves are exposed to the light, and stops instantly when taken out of the light. Van Tieghem, experimenting upon aquatic plants, at first supposed he had proved the contrary, but in further experiments came to the same result.

To what portion of the solar radiation is this decomposition owing?—Prillieux, using light of different colors but of equal intensity, through colored solutions, thought he had ascertained that the decomposition was directly due to the luminous intensity, irrespective of the nature of the rays; that the orange and yellow rays and those of that part of the spectrum are most efficient merely because they are most luminous. But Dehérain, repeating these experiments, and measuring carefully the quantities of gas emitted, concludes that the blue and violet rays are not so effective as the yellow and red, when both are brought to the same intensity. Also that the same holds true of the evaporation of water from foliage. According to Dehérain, this is a transpiration rather than a simple evaporation, and is governed by the amount of light, and even goes on in a saturated atmosphere. He confirms an old observation of Guettard, that the water is more largely exhaled from the firmer and denser upper surface of the leaf,—which would not have been expected.

The movement of Protoplasm and of Chlorophyll with it, in the cells of leaves, on the other hand, is effected only by the rays of the other end of the solar spectrum, viz., the blue and violet. This is shown by Borodine (Acad. St. Petersb.), who has seen remarkable movements of the chlorophyll-grains in terrestrial plants, especially in the leaves of *Mnium*, a Moss, confirming an earlier discovery of Famintzin. In diffused light the grains of chlorophyll are applied to the surfaces of the cells parallel to the surface of the leaf or other organ: under direct light they promptly move away to the side walls, to return again when the light is diminished. This should be considered in connection with the observations, recorded above, that blanched plants do not turn green so readily under direct as under diffused light. Rose, more recently, concludes, as did Mohl long ago, that these movements do not originate in the grains of chlorophyll, but belong to the protoplasm in which they are imbedded.

Leaves illuminated by artificial light also decompose carbonic acid, as Prillieux shows, experimenting by electrical, the Drummond, and even common gas light.

Vegetation may thrive in an atmosphere rich in carbonic acid. This has been proved long ago as to an air containing ten per cent of this gas. Herié-Mangon, as stated by Dumas, caused a plant of *Thuja nana* to grow and prosper in an atmosphere half of which was carbonic acid.—That foliage takes in carbonic acid freely from the surrounding atmosphere, and also from water in the case of submerged plants, is well established.

Is carbonic acid taken in by the roots also? It was commonly thought that the greater part of this element of the plant's food was appropriated by the roots, either as dissolved in water or directly in the æriform state; and it is difficult to think otherwise when we consider the great store contained in every fertile or artificially manured soil, within reach of the roots of plants, and that the air in the pores of such a soil is vastly richer in carbonic acid than the atmosphere above. Nevertheless Corewinder of Lille thinks he has made out that it is not absorbed by the roots, at least in any considerably quantity, but only becomes available as it rises into the air and reaches the foliage. So unlikely a conclusion needs more decisive proof; although the favorable action of carbonic acid in respect to the solution of the mineral matters needed by the plant and absorbed by it, may be in some sort independent of its own absorption.

Oxalic acid, and especially Oxalate of Lime in plants, according to Arno Aē (in Ratisbon Flora) ought not to be regarded as in any proper sense products of excretion. They must play some essential part; for the leaves of perennials which contain oxalate of lime give it up in autumn before they perish to the parts which live over the winter, and it passes again in spring into the new vernal foliage.

Nature and use of the Latex. Schultz-Schultzenstein, with whom our principal knowledge of the vessels of the latex began, regarded the liquid they contain (the milky juice, &c., of certain plants) as truly representing the blood of animals, and therefore as nourishing fluid. Treviranus led off in the opposite opinion, which, drawing its strongest arguments from the fact that colored juices are found only in a comparatively small number of plants, and that they largely contain gum-resins and the like, pronounces this liquid to be a secretion and not a nourishing sap: this view was vigorously maintained by Von Mohl. Trécul, who has in these last dozen years done so much excellent work in vegetable anatomy, and especially upon the latex-vessels,—and who discovered their inosculation with or termination in ducts of all sorts and other elongated cells, and the penetration of the colored juices into these,—regards these juices as a kind of analogue of venous blood, which, having done its work in the cells, is conveyed into the ducts and thence to the leaves for a new elaboration. The recent German physiologists, Hanstein and Sachs, considering that latex is rich in assimilated matters, especially proteine, fit for nourishment, conclude that a part of it at least is employed in growth, and is therefore elaborated sap, although the caoutchouc, &c., in it may be excrementitious. In France, Faivre, who has formerly investigated the latex of *Ficus elastica*, has lately and for five years studied that of the White Mulberry. He confirms the last mentioned view; and shows that the latex of this tree contains a large proportion of assimilable nourishing matter, both ternary and quaternary, and that this matter is actually used by the plant in growth, in the way of an elaborated sap.

Insect-aid to Fertilization in plants, and the arrangements therefor, have been much studied of late by Hildebrand in Germany and Delpino in Italy. The former a few years ago wrote a systematic treatise on the subject which should be translated into English. In a later paper he has been noticing the successive disappearance from the tropics northward of certain tribes or groups of plants with the disappearance of the tribes of insects, or of humming birds, &c., which effect their fertilization: Roses, Peonies, &c., disappearing with the larger Coleoptera, many Silenes and Lychnises with nocturnal Lepidoptera, until, in the arctic zone there are only such flowering plants as are fertilized by the aid of Hymenoptera, Diptera, and the wind. Delpino, in analyzing the Phænogamous flora of Nova Zembla, concludes that of its 124 flowering plants, 16 are dichogamous by the aid of Hymenoptera, 84, dichogamous and monogamous by aid of these and Diptera, and 24 are fertilized by the wind. So of the 91 species in Spitzbergen, 2 are fertilized by Hymenoptera, 63 by Hymenoptera and Diptera, and 26 by the wind.

Fertilization in Acrogenous Cryptogamous plants. A valuable contribution has been made, by Strasburger, Professor in the University of Warsaw. His papers on the fertilization of Ferns, &c., are published in the Memoirs of the Imperial Academy of St. Petersburgh and the Bot. Zeitung for 1868, and are reproduced in French in the Ann. Sci. Naturelles. The interesting point is, that a mucilage, secreted in the canal of the archegonium or pistillidium and discharged from its orifice, serves to catch and entangle the moving spermatozoids and to direct their course down to the cell to be fertilized,—into which one or more of the spermatozoids work their way by their pointed end, while the other and enlarged globular extremity often breaks off and is left behind.

The influence of stock upon scion and the converse, which has of late been much under consideration, appears to be well made out in one kind of case, viz., in the propagation from the one to the other of variegation. The older facts of the sort are confirmed by some recent cases which Dr. Masters has exhibited to the Scientific Committee of the Royal Horticultural Society;—in which the foliage of the grafts of one species *Abutilon* took on variegation from the variegated stock of a different species; and *vice versa*, a variegated graft, inserted upon a green stock, and after a time pinched back, caused buds of the stock to develop with variegated foliage. Moreover, in a case recorded by Prof. Morren, the variegation ceased after the accidental destruction of the variegated graft; and it is said that "the mere insertion of a detached variegated leaf into a slit in the back of a green *Abutilon* was sufficient to inoculate the latter, even although the inserted leaf speedily perished." *Gard. Chronicle*, March 5.—Cases of this kind, which may best be regarded as the propagation of disease, bear however only indirectly upon the question of the sharing of special qualities between stock and scion. A. G.

7. Prof. FRANCIS UNGER, of Vienna, distinguished in Fossil Botany, &c., in former years the associate of Endlicher, died, in his native town of Gratz, February 12th, in the 69th year of his age. It was at first reported that he had died suddenly; then, that he was found "murdered in his bed." A. G.

8. *On Deep Sea Dredging in the neighborhood of the British Isles in* 1869:—*The Temperature, Currents, Life, Waters, and Gases present in the waters, in the depths of the Oceans;* by Dr. W. B. CARPENTER.—The following are extracts from a Lecture by Dr. Carpenter before the Royal Institution of Great Britain on the 11th of February last. We omit the most of what relates to the life of the sea bottom, as this part of the subject was presented by Prof. Verrill in our January number, at page 129. The discourse commences by giving the results of the three cruises of the Porcupine—undertaken to complete and extend the investigations made by the "Lightning" Expedition of 1868. The *first* cruise commenced from Galway, and was directed first to the southwest, then to the west, and finally to the northwest as far as the Rockall Bank; the greatest depth of dredging done by it was 1476 fathoms; the *second*, to the northern extremity of the Bay of Biscay, where a depth 2500 fathoms was known to exist, and dredging was carried to a depth of 2345 fathoms; the *third*, over the area between the north of Scotland and the Faroe Islands.

* * * The following summary of the results [with regard to *temperature*] brings into marked contrast the conditions of the warm and cold areas, which occupy respectively the W. S. W. and E. N. E. portions of the channel between the north of Scotland and the Faroe Islands, and lie side by side in its midst.

The *surface*-temperature may be said to be everywhere nearly the same, viz., 52°; the variations above or below this being attributable either to atmospheric differences (as wind, sunshine, &c.) or to difference of latitude. Alike in the warm and the cold areas there was a fall of from 3° to 4° in the first 50 fathoms, bringing down the temperature at that depth to 48°. A slow descent took place nearly at the same rate in both areas through the next 150 fathoms; the temperature in the warm area at the depth of 200 fathoms being 47°, whilst in the cold it was 45·7°. It is below this depth that the marked difference shows itself. For whilst in the warm area there is a slow and pretty uniform descent in the next 400 fathoms, amounting to less than *four degrees* in the whole, there is in the cold area a descent of *fifteen degrees* in the next 100 fathoms, bringing down the temperature at 300 fathoms to 30·8°. Even this is not the lowest; for the serial soundings taken at depths intermediate between 300 and 640 fathoms (the latter being the greatest depth met with in the cold area, midway between the Faroe and the Shetland Islands) showed a further progressive descent; the lowest bottom-temperature met with being 29·6°. Thus, while the temperature of the superficial stratum of the water occupying the cold area clearly indicates its derivation from the same source as the general body of water occupying the warm area, the temperature of the deeper stratum, which may have a thickness of more than *two thousand* feet, ranges from the freezing-point of fresh water to 2½° below it. Between the two is a *stratum of intermixture* of about 100 fathoms thickness, which marks the transition between the warm superficial layer and the body of frigid water which occupies the deeper part of the channel.

The shortest distance, within which these two contrasted submarine climates were observed at corresponding depths, was about 20 miles; but a much smaller distance was sufficient to produce it when the depth rapidly changed. Thus near the southern border of the deep channel, at a depth of 190 fathoms, the bottom-temperature was 48·7°; while *only six miles* off, where the depth had increased to 445 fathoms, the bottom-temperature was 30°·1. In the first case, the bottom evidently lay in the warm superficial stratum; whilst in the second it was overflowed by the deeper frigid stream.

It seems impossible to account for these phenomena on any other hypothesis than that of the *direct derivation of this frigid water from the Arctic basin.* And this agrees very well with other facts observed in the course of the exploration. Thus:—(1) The rapid descent of temperature marking the "stratum of intermixture" began about 50 fathoms nearer the surface in the most northerly portion of the cold area examined, than it did in the most southerly, as might be expected from the nearer proximity of the cold stream to its source. (2) The sand covering the bottom contains particles of volcanic minerals, probably brought down from Jan Mayen or Spitzbergen. (3) The fauna of the cold area has a decidedly Boreal type; many of the animals which abound in it having been hitherto found only on the shores of Greenland, Iceland, or Spitzbergen.

Although the temperatures obtained in the warm area do not afford the same striking evidence of the derivation of its whole body of water from a southern source, yet a careful examination of its condition seems fully to justify such an inference. For the water at 400 fathoms in lat. 59½° was only 2·4° colder than water at the same depth at the northern border of the Bay of Biscay, in a latitude more than 10° to the south, where the surface-temperature was 62·7°; and the approximation of the two temperatures is yet nearer at still greater depths, the bottom-temperature at 767 fathoms at the former station being 41·4°, whilst the temperature at 750 fathoms at the latter point was 42·5°. Now, as it may be certainly affirmed that the lowest temperature observed in the warm area is considerably above the isotherm of its latitude, and that this elevation could not be maintained against the cooling influence of the Arctic stream but for a continual supply of heat from a warmer region, the inference seems inevitable that the bulk of the water in the warm area must have come thither from the southwest. The influence of the Gulf Stream proper (meaning by this the body of superheated water which issues through the 'Narrows' from the Gulf of Mexico), if it reaches this locality at all—which is very doubtful—could only affect the *most superficial* stratum; and the same may be said of the surface-drift caused by the prevalence of southwesterly winds, to which some have attributed the phenomena usually accounted for by the extension of the Gulf Stream to these regions. And the presence of the body of water which lies between 100 and 600 fathoms' depth, and the range of whose temperature is from 48° to 42°, can scarcely be accounted for on any other hypothesis than that of a *great general movement of Equatorial water toward the Polar area*; of which movement the Gulf Stream constitutes a peculiar case modified by local conditions. In like manner, the Arctic Stream, which underlies the warm superficial stratum in our cold area, constitutes a peculiar case, modified by the local conditions to be presently explained, of *a great general movement of Polar water toward the Equatorial area*, which depresses the temperature of the deepest parts of the great Oceanic basins nearly to the freezing-point.

During the *first* and *second* cruises of the 'Porcupine,' the temperature of the eastern border of the great North Atlantic basin was examined at various depths between from 54 to 2435 fathoms, and in widely different localities, ranging from lat. 47° to lat. 55°. The *bottom*-temperature was ascertained at thirty stations, and *serial* soundings were taken at seven stations; making the total number of observations eighty-four. Amongst all these the coincidence of temperature at corresponding depths is extraordinarily close; the chief differences showing themselves in the temperature of the *surface* and of the stratum immediately beneath it. A decided *super-heating* is observable in this superficial stratum, not extending to a depth of much more than 70 or 80 fathoms, and more considerable at the southern than at the northern stations. Whether this 'super-heating' is entirely due to the direct influence of solar heat, or depends in any degree on an extension of the Gulf Stream as far as the southern part of the area examined, is a question which can only be resolved by the determination of its relative amount at different seasons. Between 100 and 500 fathoms, the rate of decrement is very slow, averaging only about 3° in the whole, or three-fourths of a degree for every 100 fathoms; and this body of water has a temperature so much above the isotherm of the northern stations, at which the observations were made, as decidedly to indicate that it must have found its way thither from a southern source. Between 500 and 750 fathoms, however, the rate of decrease becomes much more rapid, the reduction being 5°·4, or above 2° per 100 fathoms; while between 750 and 1000 fathoms it amounts to 3°·1, bringing down the temperature at the latter depth to an average of 38°·6. Beneath this there is still a slow progressive reduction with increase of depth, the temperature falling a little more than 2° between 1000 and 2435 fathoms; so that at the last-named depth, the greatest at which it was ascertained, it was 36°·5.—Thus it is obvious either that the vast body of water occupying the deeper half of the Atlantic basin has been itself derived from a colder region, or that its temperature has been reduced by the diffusion through it of frigid water from a Polar source. The latter supposition best accords with the *gradual* depression of temperature exhibited between 500 and 1000 fathoms which corresponds with the "stratum of intermixture" of the cold area.

The temperature-soundings recently taken by Commander Chimmo, R. N., and Lieutenant Johnson, R. N., at various points in the North Atlantic basin, when the requisite corrections are applied for the influence of pressure on the bulbs of the unprotected thermometers employed by them, give results which are remarkably accordant with our own; so that it may be stated with confidence that the temperature of the deeper parts of the North Atlantic sea-bed is but a very few degrees above the freezing-point.

Now a glance at the North Polar region, as laid down either on a globe, or any projection of which the Pole is the center, shows that the Polar basin is so much shut in by the northern shores of the European, Asiatic, and American continents, that its only communication with the North Atlantic basin—besides the circuitous passages leading into Hudson's and Baffin's Bays—is the space which intervenes between the eastern coast of Greenland and the northwestern portion of the Scandinavian peninsula. If, therefore, there be any such general interchange of polar and equatorial water as that for which we have argued, the Arctic current must flow through the deeper portions of this interspace, at the north of which lies Spitzbergen, whilst Iceland and the Faroes lie in the middle of its southerly expanse. Now in the channel that lies between Greenland and Iceland, the depth is such as to give a free passage to such a frigid stream; but between Iceland and the Faroe Islands there is no depth so great as 300 fathoms at any part, except in a narrow channel at the southeast corner of Iceland; so that an effectual barrier is thus interposed to any movement of frigid water at a depth exceeding this. A similar barrier is presented, not merely by the plateau on which the British Islands rest, but also by the bed of the North sea; the shallowness of which must give to such a movement a not less effectual check than would be afforded by an actual coast-line uniting the Shetland Islands and Norway. Consequently, it is obvious that a flow of ice-cold water at a depth exceeding 300 fathoms from the surface, down the northeastern portion of this interspace, *can* only find its way southward through the deeper portion of the channel between the Faroe and Shetland Islands; which will turn it into a W. S. W. direction between the Faroe Islands and the north of Scotland, and finally discharge such part of it as has not been neutralized by the opposing stream coming up from the southwest, into the great North Atlantic basin, where it will meet the Icelandic and Greenland currents, and unite with them in diffusing frigid waters through its deeper portion. In thus spreading itself, however, the frigid water will necessarily mingle with the mass of warmer water with which it meets, and will thus have its own temperature raised, whilst lowering the general temperature of that mass; and hence it is that we do not find the temperature of even the greatest depths of the Atlantic basin nearly so low as that of the comparatively shallow channel which feeds it with Arctic water.

It may be questioned, however, whether the whole body of Arctic water that finds its way through the channels just indicated, could alone maintain so considerable a reduction in the temperature of the enormous mass which lies below 1000 fathoms in the Atlantic basin; subject as this must be to continual elevation by the surface-action of the sun on its southern portion. And as the few reliable observations on Deep-sea Temperatures under the equator indicate that even there a temperature not much above 32° prevails, it seems probable that part of the cooling effect is due to the extension of a flow of frigid water from the Antarctic Pole, even north of the Tropic of Cancer. Of such an extension there is evidence in the temperature-soundings recently taken in H.M.S. 'Hydra' between Aden and Bombay, where the cooling influence could scarcely have been derived from any other source than the Antarctic area.*

The unrestricted communication which exists between the Antarctic area and the great Southern Ocean-basins would involve, if the doctrine of a general Oceanic circulation be admitted, a much more considerable interchange of waters between the Antarctic and Equatorial areas, than is possible in the Northern hemisphere. And of such a free interchange there seems adequate evidence; for it is well known to navigators that there is a perceptible 'set' of warm surface-water in all the southern oceans toward the Antarctic Pole; this 'set' being so decided in one part of the Southern Indian Ocean, as to be compared by Capt. Maury to the Gulf Stream of the North Atlantic.† Conversely, it would appear from the application of the necessary pressure-correction to the temperatures taken in Sir James Ross's Antarctic expedition, the voyage of the 'Venus,' &c., at depths greater than 1000 fathoms, that the bottom-temperature of the deepest parts of the Southern Oceanic basin really approaches the freezing-point, or is even below it. And if the temperature of the deeper portion of the *North* Pacific Ocean should be found to exhibit a depression at all corresponding to that of the North Atlantic, it must be attributed entirely to the extension of this Antarctic flow; since the depth of Behring's Strait, as well as its breadth, is so small as to permit no body of Arctic water to issue through that channel.† * * * *

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* The lowest temperature *actually observed* in these soundings was 36½°. The temperature of 33½° given in the previous discourse, as existing below 1800 fathoms, proves to have been only an *estimate* formed by Captain Shortland under the idea that the rate of reduction observed at smaller depths would continue uniform to the bottom, which the serial soundings of the 'Porcupine' prove to be by no means the case.

† 'Physical Geography of the Sea,' §§ 748–750.

The *Foraminifera* collected in the 'Porcupine' expedition present features of no less interest, though their scale is so much smaller. The enormous mass of Globigerina-mud (sometimes almost pure, sometimes mixed with sand) that everywhere covers the deep-sea bottom in the region explored, save where its temperature is reduced nearly to the freezing-point, may be judged of from the fact that in one instance the dredge brought up *half a ton* of it from a depth of 767 fathoms. The resemblance of this deposit to chalk is greatly strengthened by the recognition of several characteristically Cretaceous types among the Foraminifera scattered through the mass of *Globigerinæ* of which it is principally composed; as also that of *Xanthidia*, frequently preserved in flints. Not many absolute novelties presented themselves among the foraminifera that form true calcareous *shells;* the chief point of interest being the occurrence of certain types of high organization at great depths, and their attainment of a size that is only paralleled in much warmer latitudes, or in the Tertiary or yet older formations. This is especially the case with the *Cristellarian* group, which has a long geological range; and also with the *Miliolinæ*, of which specimens of unprecedented size presented themselves. The most interesting novelty was a beautiful *Orbitolite*, which, when complete, must have had the diameter of a sixpence, but which, from its extreme tenuity, always broke in the process of collection.—Of arenaceous Foraminifera, however, which construct *tests* by cementing together sand-grains, instead of producing shells, the number of new types is such as to seriously tax our power of inventing appropriate generic names. Many of these types have a remarkable resemblance to forms previously known in the Chalk, the nature of which had not been recognized. Some of them throw an important light on the structure of two gigantic arenaceous types from the Upper green-sand, recently described by the speaker and Mr. H. B. Brady, an account of which will appear in the forthcoming part of the 'Philosophical Transactions;'

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† This statement is not in accordance with the observations hitherto made on the temperatures of the ocean along the Asiatic coast of the Pacific and the distribution of oceanic life; for these indicate a north Polar current as decided as that of the Atlantic. See the isothermal chart of the oceans in Dana's Report on Crustacea, and in this Journal, II, vol. xvi, 1853, and also his Manual of Geology. The observations on the animal life of the coast, made by W. Stimpson, in the Ringgold Expedition, are confirmatory evidence on this point.—J. D. D.

and there is one which can certainly be identified with a form lately discovered by Mr. H. B. Brady in a clay-bed of the Carboniferous limestone.

The question now arises, whether—as there must have been deep seas in all geological periods, and as the changes which modified the climate and depth of the sea-bottom were for the most part very gradual—we may not carry back the continuity of the accumulation of *Globigerina*-mud on some part or other of the ocean-bed into geological epochs still more remote; and whether it has not had the same large share in the production of the earlier calcareous deposits that it has undoubtedly had in that of the later. The Foraminiferal origin of certain beds of the Carboniferous limestone, for example, appears to be indicated by the presence of *Globigerinæ*, long since observed by Professor Phillips in sections of them, as well as by the fact just stated. The sub-crystalline character of these rocks cannot be regarded as in any way antagonistic to such an idea of their origin, since it is perfectly well known that all traces of the organic origin of calcareous rocks may be completely removed by subsequent metamorphism,—as in the Chalk of the Antrim coast.

What is the *source of nutriment* for the vast mass of animal life covering the abyssal sea-bed is a question of the greatest biological interest. That animals have no power in themselves of generating the organic compounds which serve as the materials of their bodies—and that the production of these materials from the carbonic acid, water, and ammonia of the Inorganic world, under the influence of light, is the special attribute of vegetation—is a doctrine so generally accepted, that to call it in question would be esteemed a physiological heresy. There is no difficulty in accounting for the alimentation of the higher animal types, with such an unlimited supply of food as is afforded by the *Globigerinæ* and the *Sponges* in the midst of which they live, and on which many of them are known to feed. Given the *Protozoa*, everything else is explicable. But the question returns,—On what do these *Protozoa* live?

The hypothesis has been advanced that the food of the abyssal *Protozoa* is derived from *Diatoms* and other forms of minute plants, which, ordinarily living at or near the surface, may, by subsiding to the depths, carry down to the animals of the sea-bed the supplies they require. Our examination of the surface-waters, however, has afforded no evidence of the existence of such microphytic vegetation in quantity at all sufficient to supply the vast demand; and the most careful search in the Globigerina-mud has failed to bring to light more than a very small number of specimens of these siliceous envelopes of Diatoms, which would most assuredly have revealed themselves in abundance, had these protophytes served as a principal component of the food of the *Protozoa* that have their dwelling-place on the sea-bed.—Another hypothesis has been suggested, that these *Protozoa*, which are so near the borders of the vegetable kingdom, may be able, like plants, to generate organic compounds for themselves—manufacturing their own food, so to speak, from inorganic materials. But it is scarcely conceivable that they should do this without the agency of light; and, as it is obviously the want of that agency which excludes the possibility of vegetation in the abysses of the ocean, the same deficiency would prevent animals from carrying on the like process.

A possible solution of this difficulty, offered by Prof. Wyville Thomson in a lecture delivered last spring, has received so remarkable a confirmation from the researches made in the 'Porcupine' expedition, that it may now be put forth with considerable confidence. It is, he remarked, the distinctive character of the *Protozoa*, that "they have no special organs of nutrition, but that they absorb water through the whole surface of their jelly-like bodies. Most of these animals secrete exquisitely-formed skeletons, sometimes of lime, sometimes of silica. There is no doubt that they extract both of these substances from the sea-water, although silica often exists there in quantity so small as to elude detection by chemical tests. All sea-water contains a certain amount of organic matter in solution. Its sources are obvious. All rivers contain a large quantity; every shore is surrounded by a fringe, which averages about a mile in width, of olive and red sea-weeds; in the middle of the Atlantic there is a marine meadow, the Sargasso Sea, extending over 3,000,000 of square miles; the sea is full of animals which are constantly dying and decaying; and the water of the Gulf Stream, especially, courses around coasts where the supply of organic matter is enormous. It is, therefore, quite intelligible that a world of animals should live in these dark abysses: but it is a necessary condition that they should chiefly belong to a class capable of being supported by absorption through the surface of matter in solution; developing but little heat, and incurring a very small amount of waste by any manifestation of vital activity. According to this view, it seems highly probable that at all periods of the earth's history some form of the Protozoa—Rhizopods, Sponges, or both—predominated over all other forms of animal life in the depths of the sea; whether spreading, compact, and reef-like, as in the Laurentian and Palæozoic *Eozoön;* or in the forms of myriads of separate organisms, as in the *Globigerinæ* and *Ventriculites* of the Chalk."*

During each cruise of the 'Porcupine,' samples of sea-water obtained from various depths, as well as from the surface, at stations far removed from land, were submitted to the permanganate test, after the method of Prof. W. A. Miller, with an addition suggested by Dr. Angus Smith for the purpose of distinguishing the organic matter *in a state of decomposition* from that which is only *decomposable;* with the result of showing the uniform presence of an appreciable quantity of matter of the latter kind, which, not having passed into a state of decomposition, may be *assimilable* as food by animals,—being, in fact, protoplasm in a state of extreme dilution. And the careful analyses of larger quantities collected during the third cruise, which have been since made by Dr. Frankland, have fully confirmed these results, by demonstrating the *highly azotized* character of this organic matter, which presents itself in samples of sea-water taken up at from 500 to 750 fathoms depth, in such a proportion that its universal diffusion through the oceanic waters may be safely predicated.

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* "The Depths of the Sea," a Lecture delivered in the theatre of the Royal Dublin Society, April 10, 1869.

Until, therefore, any other more probable hypothesis shall have been proposed, the sustenance of animal life on the ocean-bottom at any depth may be fairly accounted for on the supposition of Prof. Wyville Thomson, that the Protozoic portion of that fauna is nourished by the direct absorption from the dilute protoplasm diffused through the whole mass of oceanic waters, just as it draws from the same mass the mineral ingredients of the skeletons it forms. This diffused protoplasm, however, must be continually undergoing decomposition, and must be as continually renewed; and the source of that renewal must lie on the *surface-life* of plants and animals, by which (as pointed out by Prof. Wyville Thomson) fresh supplies of organic matter must be continually imparted to the oceanic waters, being carried down even to their greatest depths by that *liquid diffusion* which was so admirably investigated by the late Professor Graham.

Not only, however, has the nutrition of the abyssal fauna to be explained; its *respiration* also has to be accounted for; and on this process also the results of the analyses of the gases of the sea-water made during the 'Porcupine' expedition throw very important light. Samples were collected not only at the surface, under a great variety of circumstances, but also from great depths; and the gases expelled by boiling were subjected to analysis according to the method of Prof. W. A. Miller—the adaptation of his apparatus to the exigencies of ship-board having been successfully accomplished during the first cruise by Mr. W. L. Carpenter. The general average of thirty analyses of surface-water gives the following as the percentage proportions:—25·1 oxygen, 54·2 nitrogen, 20·7 carbonic acid. This proportion, however, was subject to great variations, as will presently be shown. As a general rule, the proportion of oxygen was found to diminish, and that of carbonic acid to increase, with the depth: the results of analyses of *intermediate* waters giving a percentage of 22·0 oxygen, 52·8 nitrogen, and 26·2 carbonic acid; while the results of analyses of *bottom*-waters gave 19·5 oxygen, 52·6 nitrogen, and 27·9 carbonic acid. But *bottom*-water at a comparatively small depth often contained as much carbonic acid and as little oxygen as *intermediate* water at much greater depths; and the proportion of carbonic acid to oxygen in *bottom*-water was found to bear a much closer relation to the abundance of animal life (especially of the more elevated types), as shown by the dredge, than to its depth. This was very strikingly shown in an instance in which analyses were made of the gases contained in samples of water collected at every 50 fathoms, from 400 fathoms to the bottom at 862 fathoms, the percentage results being as follows:

	750 fath.	800 fath.	Bottom, 862 fath.
Oxygen - -	18·8	17·8	17·2
Nitrogen - - -	49·3	48·5	34.5
Carbonic acid -	31·9	33·7	48·3

The extraordinarily augmented percentage of carbonic acid in the stratum of water here immediately overlying the sea-bed was accompanied by a great abundance of animal life. On the other hand, the lowest percentage of carbonic acid found in bottom-water —viz: 7·9—was accompanied by a "very bad haul." In several cases in which the depths were nearly the same, the analyst ventured a prediction as to the abundance, or otherwise, of animal life, from the proportion of carbonic acid in the bottom-water; and his prediction proved in every instance correct.

It would appear, therefore, that the increase in the proportion of carbonic acid, and the diminution in that of the oxygen, in the abyssal waters of the ocean, is due to the respiratory process, which is no less a necessary condition of the existence of animal life on the sea-bed, than is the presence of food-material for its sustenance. And it is further obvious that the continued consumption of oxygen and liberation of carbonic acid would soon render the stratum of water immediately above the bottom completely irrespirable— in the absence of any antagonistic process of vegetation—were it not for the upward diffusion of the carbonic acid through the intermediate waters *to the surface*, and the *downward* diffusion of oxygen *from* the surface to the depths below. A continual interchange will take place *at* the surface between the gases of the sea-water and those of the atmosphere; and thus the respiration of the abyssal fauna is provided for by a process of diffusion, which may have to operate through *three miles* or more of intervening water.

The varying proportions of carbonic acid and oxygen in the *surface*-waters are doubtless to be accounted for in part by the differences in the amount and character of the animal life existing beneath; but a comparison of the results of the analyses made during the agitation of the surface by wind, with those made in calm weather, showed so decided a reduction in the proportion of carbonic acid, with an increase in that of oxygen, under the former condition, as almost unequivocally to indicate that superficial disturbance of the sea by atmospheric movement is absolutely necessary for its purification from the noxious effects of animal decomposition. Of this view a most unexpected and remarkable confirmation has been afforded by the following circumstance:—In one of the analyses of surface-water made during the second cruise, the percentage of carbonic acid fell as low as 3·3, while that of oxygen rose as high as 37·1; and in a like analysis made during the third cruise, the percentage of carbonic acid was 5·6; while that of oxygen was 45·3. As the results of every other analysis of surface-water were in marked contrast to these, it became a question whether they should not be thrown out as erroneous;

until it was recollected that, while the samples of surface water had been generally taken up from the *bow* of the vessel, they had been drawn in these two instances from *abaft the paddles*, and had thus been subjected to such a violent agitation in contact with the atmosphere, as would preëminently favor their thorough aëration.

Hence, then, it may be affirmed that every disturbance of the ocean-surface by atmospheric movement, from the gentlest ripple to the most tremendous storm-wave, contributes, in proportion to its amount, to the maintenance of animal life in its abyssal depths—doing, in fact, for the aëration of the fluids of their inhabitants, just what is done by the heaving and falling of our own chests for the aëration of the blood which courses through our lungs. A perpetual calm would be as fatal to their continued existence, as the forcible stoppage of all respiratory movement would be to our own. And thus universal stagnation would become universal death. * * *

In conclusion, he referred to the systematic and energetic prosecution of deep-sea explorations by the United States Coast Survey and by the Swedish Government—the results of which appear to be singularly accordant with those now briefly expounded,—as showing that other maritime powers are strongly interested in the subject; and expressed the earnest hope that the liberal assistance of H. M. Government, which has already enabled British Naturalists to obtain the lead in this inquiry, would be so continued as to enable them to keep it in the future. In particular, he called attention to the suggestion lately thrown out by M. Alex. Agassiz, that an arrangement might be made by our own Admiralty with the Naval authorities of the United States; by which a thorough survey, physical and biological, of the North Atlantic should be divided between the two countries; so that British and American explorers, prosecuting in a spirit of generous rivalry labors most important to the science of the future, might meet and shake hands on the mid-ocean.

9. *Secretion of Sulphuric acid by certain Gasteropods.*—During the autumn of 1853, Professor TROSCHEL succeeded in obtaining in Messina two specimens of the gigantic gasteropod *Dolium galea* Lk., intending to remove the animal from its shell for preservation. One of the mollusks threw out its enormous proboscis to defend itself, and M. Troschel seized it for examination; when immediately there was projected from this proboscis a drop of a limpid liquid which, falling upon the marble pavement of the room, produced, much to his astonishment, an active effervescence. He succeeded in collecting a portion of this liquid, and in proving it to be a secretion of the salivary gland. A few months later, this saliva was analyzed by Dr. Bœdeker of Bonn, who found in it free sulphuric acid.

Shortly after, MM. Quoy and Gaimard, having discovered in the genus *Cassis*, a gland analogous to that of the *Dolium*, suggested that possibly these gasteropods also secreted a similar fluid, using it perhaps as a means of defense, but also to aid them in perforating bivalve shells. The *Dolium* saliva was subsequently analyzed by W. Preyer, and Keferstein published a description of the gland.

This was the state of the question when Professor Panceri—whose paper has just been published in the Memoirs of the Royal Academy of Naples—undertook its investigation. He associated Professor de Luca with him in the work, and to him we are indebted for new analyses of this remarkable secretion. These analyses were comparative, made upon the saliva of two similar individuals. The liquid was colorless, and slightly opaline from the presence of a sulpho-nitrogenous organic substance precipitable by alcohol. Its density was 1·025 in (I) and 1·030 in (II). The analysis gave:

	I.	II.
Free sulphuric acid, ...	3·42	3·30
Combined sulphuric acid, ...	0·20	0·15
Combined chlorhydric acid, ...	0·58	0·60
Potassa, soda, magnesia, oxyd of iron, phosphates, organic matter and loss, . .	1·80	2·35
Water, ...	94·00	93·60
	100·00	100·00

The mollusks, their shells, and their glands were separately weighed, with results as follows:—

	I.		II.	
Mollusks, ...	1305	grams.	520	grams.
Shells, ...	550	"	255	"
Glands, ...	150	"	80	"
	2005	"	855	"

Hence the glands constitute from 7 to 9 per cent of the total weight of the animal.

Moreover, M. Panceri found that on laying open the secretory gland of a *Dolium*, there was evolved, within a few moments, a considerable quantity of gas, which, on examination, proved to be pure carbonic acid. One gland, weighing approximately 45 grams, yielded 206 cubic centimeters of this gas. As to the secretion of so acid a fluid from the alkaline blood of the mollusk, this cannot surprise us, since it is entirely analogous to the secretion of an acid gastric juice from a similarly alkaline blood in the higher animals.

The following list comprises those species of mollusks which, like the *Dolium galea*, secrete an acid saliva:—

PROSOBRANCHS.

Cassis sulcosa Lk.
Tritonium nodiferum Lk.
" *hirsutum* Fab. Col.
" *cutaceum* (*cretaceum?*) Lk.
" *corrugatum* Lk.
Cassidaria echinophora Lk.

OPISTHOBRANCHS.

Pleurobranchidium Meckelii Leuc.
Pleurobranchus tuberculatus Meck.
" *testitudinarius* Cautr.
" *brevifrons* Phil.
Doris ————

No trace of such a secretion has been detected in the genera Carinaria, Firola, Phylliroë or in any of the numerous perforating Acephala, such as the Pholas.

After describing minutely the anatomy of the secreting organ, in both the above divisions of gasteropods, M. Panceri closes his paper with some remarks upon the origin and function of this unique secretion. Has the sulphuric acid its origin in the oxydation of the sulphur of the albuminoid tissues, or does it result from a decomposition of the sulphates found in sea-water? M. Panceri is inclined to the latter hypothesis, since, as is well known, the circulatory system of the gasteropods is so arranged at the extremity of the body as to permit the water in which they live to enter the blood in small quantity. With regard to the *role* played by the acid in this secretion, M. Panceri considers it useful solely for defense; the habits of the gasteropods studied by him forbidding the hypothesis that it is used to perforate hard bodies. Two views may be taken of the decomposition which produced this acid: 1st, it may take place solely for the purpose of obtaining the acid, in which case the secretion and emission will be but secondary and intermittent; or 2d, its production may be regular and continuous, the sulphuric acid like urea for example, being an effete product resulting from the normal chemical changes taking place in the body.—*Les Mondes*, II, xxii, 451, March 10, 1870.

10. *On the discovery of the sensitiveness to light possessed by Unios;* by WILLIAM SHARSWOOD.—In the March number of this Journal for 1869, p. 280, Prof. C. A. White has published an article under the caption "Are Unios sensitive to light?" in which he has recited some experiments by which he had been led to justly believe "that no doubt is entertained that the posterior portion of this mollusk is keenly sensitive to light, but exactly what organs are thus sensitive has not been ascertained." In the May number of this Journal for 1869, another article on this subject is presented over the caption "*Are Unios sensitive to light?* by Isaac Lea," in which are brought out Dr. Lea's prior observations establishing the same fact; referring to the Proceedings of the Academy of Natural Sciences of Philadelphia, for 1857, to substantiate his claim, and to a subsequent publication in the Introduction to vol. vi of his "Observations on the Genus Unio." Now the fact is that Mr. Lea was anticipated, by fourteen or fifteen years, in a few lines, by Prof. S. S. Haldeman, in his "Fresh-water Univalve Mollusca," Physadæ, January, 1843, p. 8, where he says, "In the other fresh-water families described in this work, the power of vision, or sensitiveness to the action of light, is rendered evident by intercepting it with an opaque object, when they instantly retract; and I have even observed the protruded branchial canal of *Unio radiatus* (Gmelin) to be suddenly withdrawn, when subjected to the same experiment." The following is another statement on the subject by the same author, published in the Iconographic Encyclopedia, Zoology, p. 69. (New York, 1850). "The extremity [of

the siphons in Unio] scarcely extends beyond the shell; it is papillate, and provided with eyes which have the power of distinguishing light from darkness, as the siphons are suddenly withdrawn, when a shadow is cast upon them."

Philadelphia, 24th and Sharswood Sts., March 9, 1870.

11. *Report on the Invertebrata of Massachusetts*, published agreeably to an order of the Legislature. Second edition, comprising the Mollusca; by AUGUSTUS A. GOULD, M.D., edited by W. G. BINNEY. Boston, 1870.—Like most revisions of antiquated works, this is a somewhat unsatisfactory book. The difficulties have even been much greater than usual, in this instance, on account of the death of the author at a time when the revision of the original work was in a very unsatisfactory and unfinished condition, which necessarily made the task of the editor laborious and the resulting work quite different, probably, from what either the author or editor would have personally wished.

The typography and illustrations of the book are excellent and do credit both to the State and the editor, as well as to the artists. The wood-cuts, of which there are 405, have nearly all been drawn from nature by Mr. E. S. Morse, whose rare artistic talent and thorough knowledge of the subject have enabled him to produce figures that are unequaled in accuracy. The drawings have been most faithfully reproduced by the engraver, Mr. Henry Marsh, whose skill contributed so largely to the value of Harris' Report on Insects. The accuracy and beauty of the cuts makes us regret that a portion of the labor had not been expended upon the hinges and interior parts of the bivalves, lingual dentition and opercula of the Gasteropods, and other parts, which are of far greater importance than mere external views, no matter how accurate.

The twelve plates are, with one exception, printed in colors and illustrate well the Nudibranchs, Ascidians, Cephalopods and some of the Pteropods. The Bryozoa and all the Radiates and Articulates are omitted from this edition, which was undoubtedly the wisest course, for these groups, which were extremely imperfectly represented in the first edition, have now been so numerously collected and become so well known that at least another volume, the size of the present, would be required to do them any justice. The greatest improvement upon the text of the first edition is the part relating to the Nudibranchs, which was elaborated by Dr. Gould some time before his death. Several species not before described or noticed are introduced, and all the species are well described and illustrated with colored figures. The Ascidians, which were very briefly and imperfectly treated in the first edition, now occupy 27 pages, and include 29 nominal species. Dr. Gould had prepared nothing on this class and the editor has unfortunately been obliged to compile the descriptions, many of them very brief and almost worthless, in fact hardly meriting the name of descriptions, which have been published in various other works. The figures are, however, mostly very good. Some, which were drawn from life by Mr. Burkhardt, have been contributed by Prof. Agassiz, but others have been drawn from alcoholic specimens and give but a poor idea of the form and appearance of the animals when living. In consequence of compiling both bad descriptions and good ones, several species of Ascidians appear under two different names. Thus *Cynthia placenta* Packard is the same as *Ascidia carnea* Ag., and is a true *Cynthia*; *C. gutta* Stimpson is, perhaps, the young of the same species, which is abundant and of larger size in the Bay of Fundy. *Molgula arenata* Stimpson is the same, in all probability, as *Ascidia psammophora* Agassiz, but the figure of the latter is very poor and does not represent the normal state of expansion. This species is abundant on certain sandy bottoms near New Haven, and is a true *Molgula*. *Ascidia ocellata* Agassiz appears to be the same as *A. tenella* Stimpson. The figure of the latter agrees exactly with numerous specimens, which we obtained living abundantly from low-water to 50 fathoms at Eastport, Me., which are no doubt Stimpson's species. *Boltenia microcosmus* Agassiz is probably only a reddish variety of *B. clavata*; we have often observed such variations among the many hundreds of specimens of *B. clavata* collected at Eastport, Me., where it is very abundant from low-water to 50 fathoms. *Glandula mollis* Binney is not the species described by Stimpson under that name, even if it belongs to the genus *Glandula*, which is very doubtful; figure 328, at least, looks much more like a *Cynthia* or an *Ascidia*. Perhaps two species are confounded together, but neither can be *G. mollis*, which is an abundant species at Eastport and very constant in its characters, as described by Dr. Stimpson. Although the names given by Agassiz to some of the species above mentioned are earlier than Stimpson's, no descriptions were given by which the species could possibly be recognized, except perhaps in the case of *A. ocellata*, in which the ocellæ are so prominent a feature. Therefore the names given by Stimpson, and accompanied by good descriptions, should be retained. But there are quite a number of Ascidians, living on the New England coast, which have been entirely omitted. The part relating to Conchifera has been much improved and enlarged, and many additional species introduced. All the species are beautifully illustrated by external views, but no figures of the hinge or internal markings are given, except in very few instances. The parts relating to Pteropods and Cephalopods are mostly compiled from various sources by the editor. It is to be regretted that more attention was not bestowed upon the distribution of the species in depth and geographically, for a very large amount of information of this kind has accumulated since the first edition, but often only the old localities are mentioned, though Dr. Stimpson had given a far greater amount of information of this kind, as long ago as 1851, in his "Shells of New England," which has not even been incorporated into the present work.

Most conchologists and naturalists generally will be surprised at the extent to which obsolete ideas of classification are retained. Thus we find the Brachiopods introduced between the Conchifers and Gasteropods! The *Scalaridæ* between *Littorinidæ* and *Vermetidæ!* In fact the arrangement of the families is very often unnatural. We find, also, many singular inconsistencies in the genera adopted, for in some families the modern views and modern names are introduced to a considerable extent, while in others, where such changes were even more desirable, the old ideas and the old genera are retained from the first edition, almost without change. But by way of apology we find the following in the editor's preface: "Should any disappointment be felt that Dr. Gould has not adopted in his work all the improvements in classification, &c., which more recent investigations have suggested, it must be remembered that this is not a new work. It is rather a reprint of an old one, with such additions and improvements as Dr. Gould considered absolutely necessary to its present usefulness." Also, "upon assuming the charge of the publication and receiving the manuscripts, drawings, notes, &c. of Dr. Gould, I endeavored to learn thoroughly what plan he had made for revising the first edition, as I was directed to complete the work *as nearly as possible in accordance with the views and wishes of the author.* I believe I have been able to arrive at a clear idea of his intentions, which, according to my orders, I have most scrupulously endeavored to carry out, irrespective of my own opinions. It is only in treating the *Pulmonifera* that I have exercised my own judgment, and here only to the extent that I believe Dr. Gould would have approved." We do not exactly understand how it is that the editor did not also *exercise his own judgment* in treating the Ascidians, Pteropods, and Cephalopods, since he informs us that nothing had been prepared by Dr. Gould relating to these classes. But we fear the editor has in some cases done Dr. Gould injustice by not introducing important and essential changes, which he himself would undoubtedly have adopted, had he lived to completely revise the work. Thus there seems to be no reason why many genera, now well established, should not have been adopted. For example, *Eurosalpinx* established for "*Buccinum cinereum*," which has nothing to do with *Buccinum; Phychatractes* Stimpson for "*Fasciolaria ligata*," etc. Even among the *Pulmonifera*, where Mr. Binney is on his own familiar ground, there are many inconsistencies which we cannot suppose that Dr. Gould would have approved of, and which can scarcely be in accordance with the editor's own judgment. Thus we find the "*Helix? harpa*" Say still retained under Say's name, although Mr. Morse has well elucidated its anatomical and physiological peculiarities, showing it to be viviparous and to have many other characters entitling it *at least* to distinct *generic* rank, and has established the genus *Zoögenites* for it, which certainly has stronger claims than some other genera of the same family, which are adopted in this work; neither do we see sufficient reasons for retaining *Helix striatella*, *H. labyrinthica*, *H. asteriscus*, *H. pulchella*, etc. in the genus *Helix*, when *H. arborea*, *H. electrina*, *H. chersina*, *H. lineata*, etc. are separated as *Hyalina*. It would have appeared to us better to have retained *all* the species in the old "genus" *Helix*, than to have produced such an unphilosophical mixture of old and new ideas as we find in this case and others.

Finally we may remark that although this work will prove of but little or no use for conveying correct ideas of classification, or even of the character of genera, it will nevertheless be indispensable and of the utmost value in the determination of the species.

V.

12. *Notes on American Crustacea, No.* 1, *Ocypodoidea;* by SIDNEY I. SMITH. 8vo, 64 pages, with 4 lithographic plates. From the Transactions of the Connecticut Academy, Vol. II, April, 1870.—This memoir includes a nearly complete monograph of the American species of *Gelasimus* or "fiddler-crabs," of which 21 species are now known from both coasts of America. Of these 14 are described at length in this paper, including 9 that are new, and most of them are illustrated. In addition to these a number of new genera and species are described, and of others, previously known, more accurate and complete descriptions are given. One new family, *Dissodactylidæ*, allied somewhat to the *Pinnotheridæ*, but differing from that and all other families of Ocypodoidea in having a palate or endostome which is not divided by a median ridge, separating the efferent passages. The plates are equal to, if they do not surpass, any that have ever been published for this class. Nearly all the figures are copied from photographs made by the author. The following are new species:

Gelasimus heterophthalmus, *G. heteropleurus*, *G. princeps*, *G. armatus*, *G. ornatus*, *G. pugnax*, *G. rapax*, *G. mordax*, *G. gibbosus*, *Cardiosoma crassum*, *Pseudothelphusa plana*, *Opisthocera*, gen. nov., *O. Gilmanii*, *Epilobocera armata*, *Glyptograpsus*, gen. nov., *G. impressus*, *Sesarma sulcata*, *S. occidentalis*, *S. angusta*, *Prionoplax ciliatus*, *Euryplax politus*, *Glyptoplax*, gen. nov., *G. pugnax*, *Pinnotheres Lithodomi*, *Ostracotheres politus*, *Dissodactylus*, fam. et gen. nov., *D. nitidus*.

Those that are redescribed are as follows:

Gelasimus minax LeConte, *G. brevifrons* Stimp., *G. pugilator* Latr., *G. subcylindricus* Stimp., *G. Panamensis* Stimp., *Cardiosoma guanhumi* Latr., *C. quadratum* Sauss., *Epilobocera Cubensis* Stimp., *Dilocarcinus pictus* Edw., *Sesarma reticulata* Say, *Euryplax nitidus* Stimp., *Pinnotheres margarita* Smith, *Pinnaxodes Chilensis* Smith. V.

13. *Monografia della Famiglia dei Pennatularii ; per il* DOTT. SEBASTIANO RICHIARDI. From Archivio per la Zoologia l'Anatomia e la Fisiologia, Ser. II, vol. i, Turin, 1869, 8vo, with 14 folded plates.—In this memoir the author has given descriptions of all the genera and species of Pennatulacea hitherto discovered. When the specimens have not been seen by the author he has quoted the original descriptions, but in other cases the descriptions are quite detailed. The plates are very good and illustrate a large number of species, including a considerable number of new ones. One new genus, *Sceptonidium*, is described, which is allied to *Virgularia*. It has short fleshy pinnæ, with a wide naked space behind; the axis is angular. The only species is *S. mosam-*

bicanum. He reunites the genus, *Leioptilum*, to *Pennatula*, but recognizes most of the other generic divisions that have been proposed. Of *Pennatula* he describes 7 species, including one that is new, *P. Targionii*, locality unknown; of *Pteroides*, 27 species, including the following new ones: *P. Grayi* (=*P. grisea* Esper), locality unknown, *P. Vogtii*, *P. Cornaliæ*, *P. Clausii*, Mediterranean, *P. Pancerii*, locality unknown; of *Sarcoptilus* one, *S. grandis* Gray; of *Ptilosarcus* two, *P. Gurneyi* and *P. sinuosus* Gray; of *Halisceptrum* one; of *Scytalium* one; of *Stylatula* 5 species; *S. finnmarchica* (Sars sp.), *S. gracilis* V., *S. elongata* V., *S. multiflora* (Kner, sp.), *S. elegans* (Danielsen sp.); of *Virgularia* 8 species, including two new ones, *V. Leuckartii*, North Sea, and *V. Köllikerii*, Mozambique; of *Lygus* one species; of *Crinillum* one; of *Funiculina* 3; of *Umbellularia* one; of *Kophobelemnon* 3; of *Lituaria* 2; of *Cavernularia* 5, including *C. Haimeii* and *C. Defilippii*, two new species from unknown localities; of *Veretillum* 6; of *Renilla* 3, *R. reniformis*, *R. violacea* Q. and G., and *R. sinuata* Gray, the last from the Philippines.

In the genus *Renilla* the author is certainly at fault, owing no doubt to lack of specimens of some of our American species. To *R. reniformis* he unites *R. peltata* V., *R. Danæ* V., and *R. amethystina* V., the last from the Panamian fauna, the others Atlantic. Neither of these approach the true *reniformis* of the Carolina coasts, of which I have examined hundreds of specimens in all states of preservation. The latter never grows so large as either of the three, but has larger polyps and many other differences. The three species referred to are all more nearly allied to *R. violacea* than to *R. reniformis.* There can be no doubt but that these three species are all distinct, one from another; it is possible that *R. Danæ* may be identical with *R. violacea*, though it does not seem probable. The others are beyond doubt good species. To *R. violacea* he unites *R. patula* Verrill. Between the two forms there are certain resemblances and they may possibly prove to be the same, but I have seen no Brazilian specimens that appear to be identical with *R. patula.* In the Museum of Yale College and the Museum of Comparative Zoölogy I have studied five distinct American species, and admitting that one may be identical with *R. violacea*, no further reduction seems admissible. It seems quite probable, however, that the number of species of *Pteroides* might be considerably reduced by a careful study of all the original specimens. A. E. V.

14. *The Butterflies of North America*, with colored drawings and descriptions; by WM. H. EDWARDS. Part 5. Philadelphia, Decem., 1869.—Number five of this beautiful work has been considerably delayed, on account of the plates, and was not actually published until April. The plates are excellent, and well sustain the character of the work. The following species are illustrated: *Argynnis Edwardsii; Colias Eurydice; Limenitis Lorquini; Grapta Faunus; Lycæna pseudargiolus; L. neglecta.* The Synopsis of North American Butterflies includes the species of *Nathalis*, *Anthocaris*, *Callidryas*, *Gonepteryx.* V.

IV. ASTRONOMY.

1. *Elements of Felicitas* (109), *from observations of the first opposition;* by WM. A. ROGERS. (Communicated for this Journal, and dated Alfred Observatory, Alfred Center, N. Y., April 12, 1870).—From my last Elements, published in the *Astronomische Nachrichten*, the following normal places were obtained, the deviations from the Ephemeris being found in the columns marked $\Delta\alpha$ (C—O), $\Delta\delta$ (C—O).

The observations of Hamilton College, Washington, and Chicago, were kindly communicated in advance of publication.

Date. W. M. T.	α . . . . δ	Δα (C–O)	Δδ (C–O)	Observations.
	° ′ ″	″	″	
Oct. 9·0	14 9 6·0 + 9 36 4·52	—1·9	+0·2	Hamilton Col. (9). Alfred (3). Chicago (2).
Oct. 29·0	9 35 51·6 + 9 52 17·8	—1·7	—2·8	H. C. (4). Washington (3). Alfred (7).
Nov. 10·0	8 0 26·2 +10 9 51·3	+0·9	—2·8	Wash. (5). Hamburg (1). Bilk. (2). Madrid (6).
Nov. 28·0	8 06 08·3 +11 08 14·9	+2·6	—3·0	Lund (2). H. C. (2). Madrid (5). Alfred (8). Wash. (5). Hamburg (3).
Dec. 28	14 27 49·1 +14 21 37·8	+4·0	—3·6	Alfred (5). H. C. (1). Wash. (4).
1870.				
Jan. 22	24 06 03·4 +18 06 49·6	+6·0	—5·0	Alfred (15). H. C. (2). Wash. (4).
Feb. 22	39 36 26·9 +23 01 39·1	+5·0	—5·4	H. C. (1). Wash. (1). Alfred (5).

From the first, fourth and sixth places, the following elements were computed:—

Oct. 9·0 1869, W. M. T.

		°	′	″
	M	339	0·5	45·21
Mean Eq. 1869·0.	π	55	56	03·25
	☊	4	56	04·35
	i	8	02	56·10
	φ	17	27	62·67
log.	*a*	·4304068		
	μ	802″·4102		

These elements represent the normals thus:—

		Δα (C–O)	Δδ (C–O)
1869.	Oct. 9.	—0·1″	+0·6″
	Oct. 29.	—0·4	—0·6
	Nov. 10.	+1·0	+0·2
	Nov. 28.	+0·4	+1·1
	Dec. 28.	—1·0	+1·2
1870.	Jan. 22.	+0·1	+0·9
	Feb. 22.	+0·0	+1·6

2. *On the Periods of certain Meteoric Rings;* by DANIEL KIRKWOOD. (Proc. Amer. Phil. Soc., March, 1870.)—(1.) *The Meteors of April 20th.*—In the Astronomische Nachrichten, No. 1632, Dr. Weiss called attention to the fact that the orbit of the first comet of 1861 very nearly intersects that of the earth, in longitude 210°; the point passed by the latter at the epoch of the April meteoric shower. A relation between the meteors and the comet, similar to that recently detected between the November meteors and the comet of 1866, was thus suggested as probable. Is this hypothesis in harmony with facts? and if not, are our present data sufficient for determining with any reasonable probability, the true period of the April meteors?

Dates of the April Shower.—Professor Newton selects the following from Quetelet's Catalogue as belonging to this period:[*]

1.	B. C. 687,	4.	A. D. 1093, '4 '5, and '6
2.	" 15,	5.	" 1122, '3
3.	A. D. 582,	6.	" 1803.

Period of the First Comet of 1861.—The elements of this body were computed by Oppolzer, who assigned it a period of 415 y. 4. Now while it is true that the interval from B. C. 687 to A. D. 1803, is very nearly equal to 6 periods of 415 years, the slightest examination will show that this period does not harmonize with *any of the intermediate dates.* This fact, then, without further discussion, seems fatal to the hypothesis that the period of the meteors is nearly equal to that of the comet.

What is the probable period of the ring?—The showers of 1093—6 and 1122—3 at once suggest a period of from 26 to 30 years. The nodal passage of the densest portion of the ring at the former epoch may be placed any where between 1093 and 1096, and that of the latter, in either 1122 or 1123. The entire interval from B. C. 687 to A. D. 1803 is 2490 years, or 88 periods of 28·295 y. each; and the known dates are all satisfied by the following scheme:

B. C.	687 to	B. C. 15	672·000	years	= 24	periods of	28·000	y. each.
"	15 to	A. D. 582	597·000	"	= 21	"	28·429	"
A. D.	582 to	" 1093·714	511·714	"	= 18	"	28·429	"
"	1093·714 to	" 1122·143	28·429	"	= 1	"	28·429	"
"	1122·143 to	" 1803	680·857	"	= 24	"	28·369	"

These coincidences indicate a period of about 28$\frac{3}{7}$[†] years, corresponding to an ellipse whose major axis is 18·59. Hence the distance of the aphelion is very nearly equal to the mean distance of Uranus. It will also be observed that the time of revolution, which seems to have been somewhat lengthened about the Christian era, was previously one-third of the period of Uranus.

(2.) *The Meteors of December* 11*th*–13*th.*—In the catalogue of Quetelet we find the four following extraordinary displays which belong undoubtedly to this period. Observations made in England. 1862, indicate also a more than ordinary number of meteors at the December epoch in that year.

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[*] This Journal, July, 1863.

[†] Herrick assigned a value of 27 years. See this Journal, April, 1841, p. 365.

1. A. D. 901. "The whole hemisphere was filled with those meteors called falling stars, the ninth of Dhu'lhajja, (288th year of the Hegira) from midnight till morning, to the great surprise of the beholders, in Egypt."—*Modern part of the Universal History*, 8vo, vol. ii, p. 281. Lond. 1780. The date of this phenomenon corresponds to the December epoch, A. D. 901.

2. 930. "Averse remarquable d'ètoiles filantes en Chine."

3. 1571. "On vit à Zurich 'du feu tomber du ciel'".

4. 1830, 1833, and 1836. The maximum seems to have occurred in 1833, when as many as ten meteors were seen simultaneously. "Dans la nuit du 11 au 12 décembre, on vit à Parme une grande quantité d'étoiles filantes de différentes grandeurs, qui se dirigeaient presque toutes avec une grande vitesse vers le SSE. A 10 heures et ½, entre les seules constellations du Bélier et du Taureau, on en compta environ une dizaine."

5. (Doubtful.) 1861, 1862, and 1863. Maximum probably in 1862. The meteors at this return were far from being comparable in numbers with the ancient displays. The shower, however, was distinctly observed. R. P. Greg, Esq., of Manchester, England, says the period for December 10th–12th was, in 1862, "exceedingly well defined."*

These dates indicate a period of about 29⅛ years. Thus:

901 to 930.... 1 period of 29·000 years.
930 to 1571....22 periods of 29·136 years.
1571 to 1833.... 9 periods of 29·111 years.
1833 to 1862.... 1 period of 29·000 years.

(3). *The Meteors of October 15th–21st.* The showers of the following years (see Quetelet's Catalogue) belong to this epoch:

1. 288. "Apparition en Chine."

2. 1436 and 1439. In each year a remarkable apparition was observed in China.

3. 1743. (Quoted from Herrick, in this Journal for April, 1841). "A clear night, great shooting of stars between 9 and 10 o'clock, all shot from S.W. to N.E. [Qu. N.E. to S.W. ?] One like a comet in the meridian very large, and like fire, with a long broad train after it, which lasted several minutes; after that was a train like a row of thick small stars for twenty minutes together, which dipped N."

4. 1798. "Brandès marque, à Goettingue, un grand nombre d'étoiles filantes dans les observations simultanèes qu' il fait avec Benzenberg."

These dates indicate a period of about 27½ years:

288 to 1439....42 periods of 27·405 years each.
1439 to 1743....11 " 27·636 "
1743 to 1798.... 2 " 27·500 "

If these periods are correct, it is a remarkable coincidence that the aphelion distances of the meteoric rings of April 18th—20th, October 15th—21st, November 14th, and December 11th—13th, as well as those of the comets 1866 (i), and 1867 (i), are all nearly equal to the mean distance of Uranus.

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* This Journal, May, 1863, p. 461.

3. ***Abstracts from the Report of the Council of the Royal Astronomical Society***, at the fifteenth Annual meeting, Feb. 1870.

(1.) ***Work done with the Photoheliograph at the Kew Observatory.***—The first instalment of the measurements and reductions of the Kew Sun-pictures taken during the two years 1862 and 1863, containing also the areas of the observed groups and an explanation of the methods followed in the working out of the observations, have been published in the last volume of the Transactions of the Royal Society. Nearly 150 separate copies of the paper, printed partly at the private expense of Mr. Warren De La Rue, were distributed chiefly to foreign observatories, scientific institutions, and distinguished astronomers and physicists.

The second instalment, containing the heliographic positions of the Sun-spots observed from the beginning of 1864 to the end of 1866, is nearly ready, and will be presented to the Royal Society at an early date during the present session.

Some investigations were also made last year on the influence which a refracting medium of considerable density would have on the apparent size and figure of the Sun, and the time of rotation, as calculated from spots at different latitudes. The preliminary discussion has led to the conviction that a comparison of the times of rotation, as derived from spots while they are near the limb, with those deduced from the same spots when near the center, will throw much light on several important questions connected with solar physics. The matter will be exhaustively investigated in the general discussion of the Kew results.

During the present year it is intended to bring, if possible, the work of the measurements up to date. The scarcity of spots during 1867 and 1868 was such, that the pictures of these years may be measured in a comparatively short time; and it is hoped that by completing, by the end of this year, the observations made up to at least the end of 1869, that the greater part of the succeeding years may be devoted to as careful a discussion of the whole work as is required by the importance of the astronomical and physical problems involved in it.

Messrs. De La Rue, Stewart, and Loewy, state that the reductions of Hofrath Schwabe's observations are now finished. By comparing his observations with those taken by Carrington, and also with the Sun-pictures taken at Kew, they arrived at very favorable conclusions regarding the accuracy of the delineations of the distinguished German observer. Beginning with the year 1832, they have measured the spotted area of all his pictures up to the time when Carrington's series commenced. From the results obtained they have, first of all, deduced fortnightly views, and in the next place, in order to get rid of the more transitory fluctuations, they have taken a series of those monthly views corresponding to the middle and end of each month, from the beginning of 1832 till the end of May 1868. Putting these results into a graphical form, they have obtained a curve exhibiting only the irregularities of comparatively long periods, and from the curve by the ordinary method of equalization, they have deduced an equalized curve exhibiting the decimal period of solar disturbances.

They find therefrom the following epochs of maximum and minimum spotted area:—

Minimum	Nov. 28	1833	Minimum	April 21	1856
Maximum	Dec. 21	1836	Maximum	Oct. 7	1859
Minimum	Sept. 21	1843	Minimum	Feb. 14	1867
Maximum	Nov. 14	1847			

From these dates it will be perceived that (as has been already observed) the time between the minimum and maximum is always less than that between the maximum and next minimum.

It will also be noticed that the whole period is not always of uniform length; nevertheless, judging from what has gone before, they believe that they are perhaps entitled to conclude that the approaching minimum will not be delayed much beyond the end of this year. It ought also to be remarked that, in all the three series, the progression from maximum to minimum is not a simple progression, but exhibits in each case traces of a secondary maximum.

Finally, they have examined these results for traces of the action of the planets upon Sun-spots, and pursuing the method indicated in their preliminary researches, they derived the following table, as exhibiting the evidence deduced from all the observations between 1832 and 1868:—

Relative Planetary Separation.	Excess or Deficiency of Spotted Area. Jupiter and Venus—	Mars and Mercury.
Between 0° and 30°	+ 881	+1675
30 " 60	— 60	— 139
60 " 90	— 452	—1665
90 " 120	— 579	—2355
120 " 150	— 705	—2318
150 " 280	— 759	—1604
180 " 210	— 893	— 481
210 " 240	— 752	+ 547
240 " 270	— 263	+ 431
270 " 300	+ 70	+ 228
300 " 330	+ 480	+1318
330 " 0	+1134	+2283

From this table there appears to be an excess of solar activity when either *Jupiter* and *Venus* or *Mars* and *Mercury* are together, and a deficiency when they are 180° apart. We see also that the progression of the numbers is regular in each case and very similar in the one case to what it is in the other.

(2.) *Spectrum Analysis.*—Our Fellow, Mr. Lockyer, has pursued with great diligence and success his spectroscopic researches on the Sun. Since the last Report he has found that several other substances besides hydrogen are occasionally to be detected above the photosphere, namely, sodium, barium, magnesium, and iron. The presence of the bright lines of these substances rising above the photosphere, and associated with the bright lines of hydrogen, Mr. Lockyer regards as indicating a state of disturbance of the solar matter, greater than was present at the time of his earlier observations. From a slight alteration in refrangibility of the bright lines of hydrogen, as shown by their want of perfect coincidence with the corresponding lines of absorption, Mr. Lockyer believes that he has evidence of rapid currents in the solar matter, sometimes attaining a velocity of 40 miles per second in a vertical direction, and a velocity of 120 miles per second in a horizontal or cyclonic direction. He considers that his observations support the conclusion that "the chromosphere and the photosphere form the true atmosphere of the Sun, and that under ordinary circumstances, the absorption is continuous from the top of the chromosphere to the bottom of the photosphere." In these observations Mr. Lockyer is accumulating a store of facts, upon which we may hope to base a more complete theory of the constitution of the Sun than we now possess.

(3.) *Lunar Radiation.*—After stating the results of Lord Rosse's observations upon Lunar Radiation which are given in this Journal, (vol. xlviii, p. 436), the council go on to say:

Some later observations have been made upon the same subject in Paris, respectively by M. Baille, at the Ecole Polytechnique, and M. Marié-Davy at the Paris Observatory. The former employed a concave mirror of 39 centimeters aperture to condense the Moon's rays upon his pile, and also made use of a Thomson's galvanometer. The one conclusion at which he arrived was, that the full Moon, at Paris and in the summer months, gave as much heat to his pile as a radiating surface 6·5 centimeters square, maintained at boiling-water temperature and placed at a distance of 35 meters. M. Marié-Davy has published results of two series of measures secured during the lunations of October and November last. The first were made with a pile attached to a 9-inch equatorial refractor, the second with an 8-inch mirror; the object-glass in the former case having been found to intercept a large proportion of the heat-rays. M. Marié-Davy's measures confirm those of Lord Rosse. They show that the heating effect of the Moon increases with the illumination of the visible disk. Between October 9, when the Moon was four days old, and October 20, when it was full, the measured heat of the condensed beam of moonlight increased from 0°·00017 (centigrade) to 0°·00287. If this last number be divided by the ratio of the area of the concentrated image to the area of the object-glass, we have twelve-millionths of a centigrade degree as the direct heating power of the full Moon at the Earth's surface. This is the result given by the object-glass, that afforded by the mirror is about six times as great. It will be seen that M. Marié-Davy has converted his galvanometer indications into centigrade equivalents: how this conversion was effected, and how the constancy of the scale indications is secured, if it is secured, we are not informed. He confirms Lord Rosse's inference that the proportion of solar to lunar radiation is about as 80000 to 1, and likewise concludes that the Moon imparts to us no heat from an internal or cosmical source. Further, he infers that the diffusive power of the lunar surface is considerable, at least equal to that of the least colored of terrestrial rocks; and he finds that the lunar heat by reason of its large percentage of obscure rays is far more impressionable by atmospheric humidity than that from the Sun.

It will be remembered that Professor Smyth, in his Teneriffe experiments, determined the heating power of the full Moon to be equal to one-third of that of a Price's candle at a distance of 14 feet 9 inches. M. Marié-Davy finds that such a candle at such a distance affects his pile to the extent of 0°·00075 centigrade, which he conceives to be the heating power of the Moon upon the summit of Teneriffe, upon the supposition that the heat emitted by the respective candle-flames was sensibly the same.

(4.) *Heating Power of the Stars.*—The experiments upon this subject, commenced by Mr. Stone with the great Equatorial of the Greenwich Observatory, in the year 1868, and mentioned in the last Annual Report, were continued as weather permitted during 1869. In pursuing them, Mr. Stone was led to the construction of a thermopile which there is good reason to believe will prove of value in thermometric researches other than those of the class for which it was specially prepared. His early trials convinced him that it was almost impossible to distinguish the feeble currents generated by stellar heat from the grosser effects produced upon the pile by exposure of one face within the telescope tube and the protection of the other face outside the telescope tube. It was evident that to maintain the pile in thermal equilibrium its two faces must be exposed to precisely similar atmospheric influences. Mr. Stone therefore resorted to what in effect may be described as a horse-shoe pile, the two faces of which being similarly presented to the object-glass of the telescope were affected alike by disturbing causes, whether these took the form of draughts of air or cooling of the metals of the pile by radiation. By this arrangement the whole heat of a star's image cast upon either face of the pile manifested itself *per se*. Decided indications of heat from *Arcturus* and α *Lyræ* were thus obtained on several nights. The amounts were measured by a reflecting galvanometer; and by a somewhat tedious process the galvanometer indications were converted into Fahrenheit-scale equivalents. It was found that the heating effect of *Arcturus*, after allowing for absorption by the object-glass, was 0·00000137 of a Fahrenheit degree; that of α *Lyræ* being about two-thirds of this amount. Otherwise expressed, the heat from *Arcturus*, at an altitude of 25° at Greenwich, is about equal to that from a three-inch cube of boiling water at a distance of 400 yards, while from α *Lyræ* it is equal to that from the same cube at 600 yards. Mr. Stone conceives that the difference of heating power may be connected in cause with difference of color. He finds that the manifested heat diminishes rapidly as the amount of moisture in the air increases, and that all sensible effect is cut off by the slightest cloud or haze. The details of the investigation are published in the *Proceedings of the Royal Society* for January, 1870.

(5.) *Transit of Venus*, 1874.—The preparatory arrangements, for the observations of the transit of *Venus*, 1874, are sufficiently advanced to encourage us to expect most important additions to the data already collected for the determination of our fundamental astronomical unit of length. The British Government has already placed at the disposal of the Astronomer Royal sufficient funds for the equipment of five stations, each with an Altazimuth and Transit, for the determination of longitude and local time, and with two telescopes, one of which is to be of six inches aperture, and to be provided with driving power. Four of the larger instruments have already been obtained, and the Transits and Altazimuths are in actual progress in the manufactory of Messrs. Troughton and Simms. The second telescope at each station will be of four inches aperture.

The present intention appears to be to place the British observers at Kerguelen's Island, Oahu, Auckland in New Zealand, Alexandria, and Rodriguez.

A Commission, consisting of Admiral Paris, MM. Faye, Laugier, Villarceau, and Puiseux, has reported to the Bureau des Longitudes that it would be particularly desirable for the French astronomers to occupy the Islands of St. Paul and Amsterdam, Yokohama, Tahiti, Nouméa, Mascate and Suez.

The North German astronomers have referred the consideration of the action which they should urge upon their Government, to a Committee, of whom the illustrious astronomer of Gotha was elected chairman.

This Committee appear to lay great stress upon the employment of heliometers for fixing the relative position of *Venus* on the Sun at the different stations.

Equatorials with driving power for the eye-observers were to be of 6 feet focal length and 52 lines aperture. The employment of photography and spectroscopic observations was discussed. The chief objection to the employment of photography appeared to be in the question of expense, although Professor Argelander was not satisfied with the degree of accuracy which might be expected from it. The further consideration of this question was, however, deferred to a sub-committee. Spectroscopic observations were proposed only to indicate the approach of the planet for the observation of external contacts. The Committee recommended that the Government be urged to fit out four expeditions,—two to the North and two to the South. The stations specially referred to as favorable were, Nertschinsk, Hakodadi, Kerguelen, Edwards', Crozet's and Auckland Islands, and in certain respects Mauritius.

The Russian territories offer most valuable positions for the location of observers. These stations are certain to be well occupied by the Russian astronomers. The Director of the Imperial Observatory at Pulkova has already secured a Committee for the consideration of a proposition to establish a chain of observers across the country from Kamtschatka to the Black Sea, at intervals of about 100 miles. This appears desirable on account of uncertainties connected with the atmospheric conditions in the month of December.

It is to be hoped that, for the same reason, there will not be a too great crowding of the observers toward one or two points, in other regions, to the exclusion of others of nearly equal importance; and that, in this matter at least, after the best consideration has been given the subject, we may all adopt instruments of not very unequal power, and attempt to make the same class of observations in the same way. Uniformity in these observations means success; want of uniformity, comparative failure.

3. *Star-Drift;* by R. A. PROCTOR.—With referrence to the accompanying account of my paper on this subject, recently communicated to the Royal Society, it is to be remarked that the interest, if any, attaching to my results must be founded on the way in which they bear on received theories respecting the distribution of the fixed stars. It is quite evident that according to the views usually accepted, the stars which appear in any part of the heavens must be regarded as situated at very different distances from the eye; the faintest nine or ten times farther from us, at the very least, than the brightest, and the different stars altogether too far apart to exert any influence on each other. Indeed, whatever theory we may hold respecting stellar distribution, regarded generally, we must be prepared to recognize in the stars seen toward any part of the sky, objects which lie at very different distances. And regarding these objects as severally in motion, we must be prepared to find in general the utmost diversity, not only as respects the direction of the apparent motions of the stars, but also as respects the magnitude of these motions. It is only when one has adopted the theory that the stars are grouped according to special laws of aggregation, that one would be led to anticipate that here and there, almost as by accident, so to speak, some indications of their grouping might be discoverable in the characteristics of the stellar proper motions. Although I had become firmly convinced that the stars are not distributed throughout space with any approach to that general uniformity insisted on by many astronomers, I had very little hope that a suggestion I threw out a year ago in the pages of the *Student*, that the stellar proper motions if examined carefully might afford evidence in favor of my views, would be confirmed in any very distinct manner if the method I had pointed out should ever be applied. I knew that a certain community of motion in the constellation Taurus had led Mädler to important, but as I judged incorrect conclusions as to the nature of the stellar motions; but I also knew that that community of motion was one which could only be appreciated by the few who had convinced themselves of what was to be *expected* if the stars were uniformly distributed. I had an impression at that time that Mädler had examined the stellar proper motions over

the whole of the northern hemisphere, and that it was the exceptional community of proper motion in Taurus which had led him to form his well-known theory respecting a central sun. It was only when I was reminded that he had in fact examined the stellar proper motions in the neighborhood of Taurus alone, having been led by independent considerations to regard that neighborhood as that within which a central sun was to be looked for, that I was encouraged to map down all the recognized proper motions. To my surprise I found that in Gemini, Cancer, and Leo, a community of motion far more striking than that noticed by Mädler in Taurus was to be recognized; and further, that though in other directions, as I had expected, stellar motions belonging to different depths in space were intermixed, it was yet possible to trace out laws of association indicating the existence of drifting star-groups in these directions also.

I lay very little stress on the indications which have led me to name the great double cluster in Perseus as more likely to be an important center of motion than the Pleiades. But it is worthy of mention that Mädler required a star on the Milky Way as the center of the galaxy, and Alcyone does *not* lie on the Milky Way; he required his center to lie ninety degrees from the apex of the solar motion, and Alcyone does *not* lie ninety degrees from the mean of the last determinations of that point. The great cluster in Perseus fulfills both conditions in the most perfect manner.

A careful examination of the proper motions of all the fixed stars in the catalogues published by Messrs. Main and Stone (Memoirs of the Royal Astronomical Society, vols. xxviii and and xxxiii) has led Mr. Proctor to the conclusion that in parts of the heavens the stars exhibit a well-marked tendency to drift in a definite direction. "In the catalogues of proper motions, owing to the way in which the stars are arranged, this tendency is masked; but when the proper motions are indicated in maps, by affixing to each star a small arrow whose length and direction indicate the magnitude and direction of the star's proper motion, the star-drift (as the phenomenon may be termed) becomes very evident. It is worthy of notice that Mädler, having been led by certain considerations to examine the neighborhood of the Pleiades for traces of a community of proper motion, founded on the drift he actually found in Taurus his well known theory that Alcyone (the *lucida* of the Pleiades) is the common center around which the sidereal system is moving. But in reality the community of motion in Taurus is only a single instance, and not the most striking that might be pointed out, of a characteristic which may be recognized in many regions of the heavens. In Gemini and Cancer there is a much more striking drift towards the southeast, the drift in Taurus being towards the southwest. In the constellation Leo, there is also a well-marked drift, in this case toward Cancer.

"These particular instances of star-drift are not the less remarkable, that the stars are drifting almost exactly in the direction due to the proper motion which has been assigned to the sun, because the recent researches of the Astronomer Royal have abundantly proved that the apparent proper motions of the stars are not to be recognized as principally due to the sun's motion. Mr. Stone has shown even that we must assign to the stars a larger proper motion, on the average, than that which the sun possesses. Looking, therefore, on the stars as severally in motion, with velocities exceeding the sun's on the average, it cannot but be looked upon as highly significant that in any large region of the heavens there should be a community of motion such as I have described. We seem compelled to look upon the the stars which exhibit such community of motion as forming a distinct system, the members of which are associated indeed with the galactic system, but are much more intimately related to each other. In other parts of the heavens, however, there are instances of a star-drift opposed to the direction due to the solar motion. A remarkable instance may be recognized among the seven bright stars of Ursa Major. Of these, the stars β, γ, δ, ϵ, and ζ are all drifting in the same direction, and almost exactly at the same rate towards the 'apex of the solar motion,' that is, the point *from* which all the motions due to the sun's translation in space should be directed. If these five stars, indeed, form a system (and I can see no other reasonable explanation of so singular a community of motion), the mind is lost in contemplating the immensity of the periods which the revolutions of the components of the system must occupy. Mädler had already assigned to the revolution of Alcor around Mizar (ζ Ursæ) a period of more than 7000 years. But if these stars, which appear so close to the naked eye, have a period of such length, what must be the cyclic periods of the stars which cover a range of several degrees upon the heavens? In like manner the stars α, β, and γ Arietis appear to form a single system, though the motion of α is not absolutely coincident either in magnitude or direction with that of β and γ, which are moving on absolutely parallel lines with equal velocity. There are many other interesting cases of the same kind." The author hopes soon to be able to lay before the Royal Society a pair of maps in which all the well-recognized proper motions in both hemispheres are exhibited on the stereographic projection. In the same maps also the effects due to the solar motion are exhibited by means of great circles through the apex of the solar motion, and small circles or parallels having that apex for a pole. The star-drift described by Mr. Proctor serves to explain several phenomena which had hitherto been thought very perplexing. In the first place, it accounts for the small effect which the correction due to the solar motion has been found to have in diminishing the sums of the squares of the stellar proper motions. Again, it explains the fact that many double stars which have a common proper motion, appear to have no motion of revolution around each other; for clearly two members of a drifting system might appear to form a close double, and yet be in reality far apart and travelling, not around each other, but around the center of gravity of the much larger system they form part of

While mapping the proper motions of the stars, Mr. Proctor has been led to notice that the rich cluster around χ Persei falls almost exactly on the intersection of the Milky Way with the great circle which may be termed the equator of the solar motion; that is, the great circle having the apex of the sun's motion as a pole. This circumstance points to that remarkable cluster, rather than to the Pleiades, as the center of the sidereal system, if indeed that system has a center cognizable by us. When we remember that for every fixed star in the Pleiades there are hundreds in the great cluster in Perseus, the latter will seem the worthier region to be the center of motion. The author is disposed, however, to regard the cluster in Perseus as the center of a portion of the sidereal system, rather than as the common center of the Galaxy. —*Nature*, No. 8, March 3.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *National Academy of Sciences: List of Papers read at the Meeting in April*, 1870, *at Washington, D. C.*—

On the measurement of wave-lengths by means of indices of refraction; by Dr. Wolcott Gibbs.

On the coming Transits of Venus, and the mode of observing them; by Prof. Simon Newcomb.

Meridional arcs measured in connection with the U. S. Coast Survey; by Prof. J. E. Hilgard.

The Relations of the four Archetypes of structure of the Animal Kingdom, as parts of one Life System; by Prof. A. Guyot.

Observations on the Measurement and Iconography of Crania; by Dr. Geo. A. Otis, U. S. A.

The northmen in Greenland; by Dr. I. I. Hayes.

Considerations on the apparent inequalities of long period in the moon's mean motion, and on the possible variability of the sidereal day; by Prof. Simon Newcomb.

On the deviation of Compasses in iron-clad ships; by Prof. Wm. Harkness, U. S. N.

On Artificial deformation of Skulls; by Dr. Geo. A. Otis, U. S. A.

On the proposed Astronomical observatory in the Argentine Republic; by Dr. B. A. Gould.

Scientific operations now in progress by the Smithsonian Institution; by Prof. Joseph Henry.

On the comparison of Barometers; by Dr. B. F. Craig.

On the influence of the interior structure of the earth, on precession and nutation; by Gen. J. G. Barnard, U. S. A.

Reduction of photographic observations of Præsepe; by Dr. B. A. Gould.

On the Lignites of Western America; by Dr. J. S. Newberry.

On the use of certain Artificial lights in photographing objects as seen with the microscope; by Dr. J. J. Woodward, U. S. A.

On the classification of Clouds; by Prof. Poey.

New breeds of Hardy Silk Worms, which feed on the "Ailanthus and Oaks," and the importance of their introduction into the country as a future industry; by J. Q. A. Warren.

On some of the phenomena attending the great tornado thunder-storm of Iowa and Illinois, of June 3rd, 1860; by Wm. L. Nicholson.

Astronomical Photography; by Lewis M. Rutherfurd.

Classification of Mammals; by Theodore Gill.

Redemption periods of life-annuities and reversions; by E. B. Elliot.

Description of a new binocular for the Microscope to be used with high powers; by F. A. P. Barnard.

Report on Metric Standards; by J. E. Hilgard.

The Basalts of Oregon, Washington, and Idaho; by R. W. Raymond.

On the polarization of the atmosphere; by Prof. Poey.

A new form of Quarternions; by Benjamin Peirce.

2. *The Mean Pressure of the Barometer and the Prevailing Winds over the Globe for the Months and for the Year.* Part II; by ALEXANDER BUCHAN, M.A., F.R.S.E., Secretary of the Scottish Meteorological Society, &c. 54 pp. 4to; (from the transactions of the Roy. Soc. of Edinburgh, vol. xxv.)—This memoir presents the results of a most important research with regard to the lines of equal barometric pressure, *or isobaric lines*, over the globe. The results as regards the months of January and July are given by the author on maps in his Handy Book on Meteorology published two years since. The paper here issued contains the maps for each of the twelve months, and also another for the means of the year. We cite a few paragraphs giving some of the conclusions.

Distribution of Atmospheric Pressure, in December, January, and February.—In these months, the highest pressures are grouped over the land portions of the northern hemisphere, and the larger the extent of the land the greater is the pressure. The area of high barometer (30 inches and upward) embraces nearly all Asia; all Europe, south of the North and Baltic Seas; the North Atlantic, between 15° and 45° lat.; the West Indies; North America, except the north and northwest; and the North Pacific, between 8° and 24° lat. There are also two regions of high pressure of comparatively small extent—the one in the South Atlantic, and the other in the South Pacific.

The regions of low pressure are the northern portions of the North Atlantic and of the North Pacific, including portions of the continents adjoining; the belt of low pressure in the equatorial regions, towards which the trade winds blow; and the remarkable depression in the Antarctic regions, which probably is subject to little variation throughout the year.

In March, pressure diminishes over Asia, the middle and south of Europe, and the United States of America. Everywhere else, except in the tropics, it is rising. This rise of pressure is most apparent in the temperate regions of the southern hemisphere. In the north of the Atlantic it is rapidly rising, the average pressure in Iceland now being 29·609 inches, thus showing an increase of 0·34 inch as compared with January.

In April, the heavy lines showing a pressure above the average have now all but left Asia, Europe, and the United States, and the isobars of 30 inches bound a belt of high pressure which completely encircles the globe in the south temperate zone. Pressure continues to rise in the north of the Atlantic, and to the north of North America, and it is probable that a space of high pressure (at least 30 inches) surrounds the North Pole. In this month pressure is more equally distributed over the globe than in any other month; for, excepting the Antarctic Ocean, it scarcely rises anywhere above 30·1 inches, or falls below 29·8 inches.

In May, in the north of Europe, in Greenland, and in the north of America, atmospheric pressure attains the maximum of the year. Pressure continues to increase over the south temperate zone, and the *isobar* of 30·1 inches now nearly extends round the globe. At this time the highest pressure in the southern hemisphere occurs in the southeast of Australia, where, at Deniliquin, it is 30·185 inches. Pressure is rapidly falling over Asia and the United States.

In June, July and August, pressure falls in the central regions of Asia to about 29·5 inches. In this season this great diminution of pressure, which may be regarded as absolutely determining the summer climates of Asia, reaches its lowest point. Pressure falls also in the interior of North America, where at Utah, Great Salt Lake, it is only about 29·7 inches. The annual maximum of the south temperate zone is attained in these months. The isobar of 30·1 inches goes completely round the globe, and a still higher pressure prevails over the south of Africa, and over those parts of the ocean immediately to the west and east of it. In these months the arrangement of the isobars may be regarded as being, generally speaking, reversed from that of December, January, and February, and on this account a comparison of these two groups of months is very instructive.

From this period, pressures increase over the continents of the northern hemisphere, and diminish over the south temperate zone, till the distribution of pressure is regained, which has been already shown to prevail during the winter months. *In September and October*, an interesting feature of these lines is a very rapid diminution of the pressure indicated as taking place in the north of the Atlantic and adjoining regions. This is the season of the year when the first great decrease of temperature takes place, which is accompanied by heavy rains and furious storms. The increase of pressure in Sweden in October, taken in connection with the simultaneous decrease in Greenland, Iceland, north of Norway, and the British Islands, is interesting, as bearing on the transference of masses of the atmosphere from one region to another.

In November, pressure rises considerably over the continents of the northern hemisphere, and falls in the south temperate zone; and the belt of low pressure in the equatorial regions may be regarded as now passing completely round the globe. This belt, towards which the trades on each side of the equator blow, does not occur in the summer months in the Indian Ocean; but, on the contrary, there is a continuous diminution of pressure northward, from Australia and Mauritius to the interior of Asia. It will be seen that in November, as compared with October, the isobars have advanced a little northward from the British Isles to Iceland, and eastward from Baffin's Bay to Iceland, thus indicating a general increase of pressure over the north of the Atlantic and regions adjoining. Coincident with this increase of pressure, there occurs a diminution of pressure to the southeast of it, including Austria, Italy, and countries adjoining the Mediterranean; and in the Atlantic to the south of it, from about latitude 45° to 15°N. Probably these extensive oscillations of the pressure are parts of one general movement of the atmosphere, which in one of its manifestations has been long known to meteorologists under the name of the great November wave, but of which no very satisfactory account has yet been given.

In addition to these changes in the monthly distribution of the pressure, it is probable that a system of low pressures traverses the continent of Africa, following the sun's course; but since the grounds of this supposition have been recently laid before the Society, in a paper on "The Determination of Heights, chiefly in the the Interior of Continents, by Observations of Atmospheric Pressure,"* it is not necessary to reproduce them here. The probable pressure for the months is shown on the separate charts.

3. *Royal Society of London.*—Fifty-three candidates have offered themselves for the fellowship of the Royal Society during the present session, and in June next fifteen out of the number will be elected.—*Athenæum*, March 12.

4. *Prizes for Comets.*—The Academy of Sciences at Vienna have offered eight gold medals for the discovery of as many comets during the coming three years.—*Athen.*, *ibid.*

OBITUARY.—MAGNUS, of Berlin, the physicist, died in that city, on the 4th of April.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Hand Book of the Sulphur-Cure as applicable to the Vine disease in America, and diseases of Apple and other Fruit Trees;* by WILLIAM J. FLAGG, author of "Three Seasons in European Vineyards." 100 pp. 12mo. New York, 1870. (Harper & Brother).—Mr. Flagg is the proprietor of extensive vineyards in Ohio, and has devoted much study to the management of vines, in health and in disease. Those who have read his lively "Three Seasons" need not be told that he handles his theme with point and vivacity, as well as with a discriminating judgment. Mr. Flagg makes no claim to a scientific knowledge of microderms, but he clearly shows, 1st, that mildew in the grape is always due to a fungus growth, and 2d, that this fungus, whether *Oidium*, *Erysephe*, or another genus, is certainly destroyed by the early and thorough application of flour sulphur. Every cultivator who reads the 'Hand Book,' must at least accept the author's closing words, and "try it." It is probably a misprint which states the "gramme"

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* Proceedings of the Roy. Soc. Edin., vol. vi, p. 465.

on p. 50 to be about 23 grains, in place of 15·43 grains. As it clearly appears that sulphur, in a very fine state of subdivision, is much preferred to even finely pulverized roll sulphur, ground in a mill, would it not be well to try the precipitated sulphur of the Pharmacopeia (*sulphur precipitatum*) "lac sulphur" or "milk of sulphur," which is an impalpable non-crystalline powder, and entirely free of the acid which contaminates flour sulphur, and which is often hurtful to the delicate leaves of the growing grape. Every farmer could prepare his own product.

2. *The Chemical Forces, Heat, Light, Electricity, with their applications to the expansion, liquefaction, and vaporization of Solids: the Steam Engine: Photography: Spectrum analysis: the Galvanic battery: Electro-plating: the Electrical illumination of Light-Houses: the Fire Alarm of Cities: the Atlantic Telegraph:* an Introduction to Chemical Physics, designed for the use of Academies, Colleges, and Medical Schools. Illustrated with numerous engravings, and containing copious lists of experiments, with directions for preparing them. By Thomas Ruggles Pynchon, M.A., Scovill Professor of Chemistry and the Natural Sciences, Trinity College. 534 pp. 12mo. Hartford, 1870. (O. D. Case & Co.)—Prof. Pynchon's book is designed chiefly for a class of readers who would be unable to follow him with the aid of mathematics. "All matters of which a knowledge could equally well be obtained from any good treatise on Natural Philosophy, have been omitted," the author tells us in his Preface:—a statement which appears hardly sustained by the rather copious list of "subjects which have been most carefully elaborated," commencing with 'heat,' and embracing pretty much the usual range of physical topics. The work is very neatly printed, and while it is not well adapted to the accurate drill of the recitation room, it is a good *vade mecum* for a course of lectures on chemical physics, and for the use of the general reader, containing a large amount of useful and interesting information on various cognate physical subjects.

3. *The Life of John James Audubon, the Naturalist.* Edited by his Widow, with an introduction by James Grant Wilson. 443 pp. 12mo. New York, 1870. (G. P. Putnam & Son.)—This charming biographical sketch of Audubon is an abridgment of a much more extended memoir prepared by Mrs. Audubon, and sent in 1867 to a London publishing house, who employed Mr. Robert Buchanan to prepare a single volume containing about one-fifth of the original manuscript. The American edition contains some additions, and suppresses several objectionable passages inserted by the London editor. Audubon was a wonderful combination of artist, naturalist, and enthusiast, fused into an intense individuality by a strong will, and gilded by heroism and poesy.

American ornithology has always been fortunate in its historians and devotees, among whom the names Wilson, Nuttall and Audubon must always stand preëminent; and this loving tribute of a devoted wife, who was always the sympathizing companion of her husband, will revive in the present generation something of the admiration for the genius of Audubon, which his gentle voice and child-like simplicity ever kindled in his contemporaries.

Audubon kept a copious journal of his daily life, from which much of the present volume is drawn, and its perusal excites the hope that in due time we may see the entire work, of which this sketch may be considered as only the precursor. Inman's spirited portrait of Audubon, engraved by Hall, prefaces the volume, which is in all respects worthy the good taste of the publishers. S.

4. *A Physician's Problems;* by Charles Elam, M.D. pp. 400, 12mo. 1869. Boston, (Fields, Osgood & Co.)—This is a carefully written and philosophical discussion of some of the most important 'Problems' which can engage the attention of the medical man. The list of topics discussed is sufficiently suggestive. I. Natural Heritage; II. On Degeneration in Man; III. On Moral and Criminal Epidemics; IV. Body; V. Mind; VI. Illusions and Hallucinations; the demon of Socrates; the amulet of Pascal; VII. On Somnambulism; VIII. Revery and Abstraction. This book addresses a much larger class of readers than that to which it is specially addressed, and while the author's views may not be universally accepted, his discussion of them is scholarly and always interesting. S.

Palæontology of the Geological Survey of New York, by James Hall. Vol. IV, Part 1. 428 pp. 4to, with 69 plates. Albany, N. Y. 1870.

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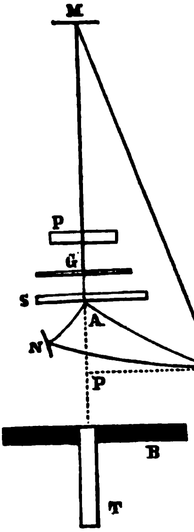
[SECOND SERIES.]

ART. I.—*Photometric Experiments.* PART SECOND.—*On the amount of light transmitted by plates of polished crown glass at a perpendicular incidence;* by OGDEN N. ROOD, Professor of Physics in Columbia College.

IT would seem that in the direct photometric experiments on this matter, the instrumental means employed have been more or less defective, in consequence of which the results gained, although having a certain amount of general correctness, still leave much to be desired. The main sources of error have been, first, the use of a defective mode of making the compensations, not allowing the experimenter to take full advantage of the sensitiveness of the eye, and secondly the employment of *two sources* of light, which always brings with it another set of unavoidable errors. Apart from this, in cases when the amount of reflected or transmitted light has been thus approximately obtained, the indices of refraction of the substances used do not seem to have been determined, so that for *perpendicular incidence*, slender data exist for comparing the results of theory and experiment.

It will be seen in what follows, that, to avoid the variability caused by two sources of light, I have devised a method in which only a single gas-flame is employed, the light being divided in such a way that a certain portion of it always illuminates the posterior side of the screen, while the other portion is reflected from a movable mirror, and pursues its way unobstructed to the screen, or is transmitted to it by the plate under experiment. The compensation is made by moving the mirror, and when once effected of course remains undisturbed by fluctuations of the flame. In addition to this it will also be observed that a more delicate screen, constructed, however, on the general principles indicated in the first part of this paper, has been employed, and these modifications, taken in connection with certain precautions described below, have given the results an accuracy superior to that attained in the experiments on silver mirrors.

Arrangement of the photometer with a single gas flame for experiments on the amount of light transmitted by plates of various substances.



The source of light is a small gas flame at L, about an inch in height, the gas flowing as before from a plain circular opening. A portion of its light, L A, directly illuminates the screen S, while another portion N A is reflected on the screen by a Liebig's mirror, one protected by a coating of copper being selected: this equalizes the light on both sides of the bare "spot" in S. A third portion of the light from L falls on M, a mirror like that just mentioned, and there is reflected perpendicularly through P, the plate under examination, on G the ground glass. It will be observed that the path pursued by the light in this latter case is the distance L M + M G: by the graduation on the instrument M G is actually measured, G P is known; also P L; hence we have the means of calculating the distance M L, which is to be added to M G, giving the distance required. As these calculations involve some labor, I constructed for my instrument a table by which the *measured* distance G M, is readily converted into the *total* distance.

The photometer was the same as that described in the first part of this paper, the following changes having been introduced: the "spot" on the screen was made much smaller, being only about $\frac{1}{30}$ of an inch in horizontal diameter, with a length about three times as great; it was observed by a small telescope T, magnifying six diameters. This reduction in the size of the "spot" rendered it possible to illuminate the ground surrounding it in a manner beyond reproach.

Mode of adjusting the apparatus for experiment.

1st. The center of the flame at L, and the center of the mirror M, must be at the same height above the common base.

2nd. The screen and plates to be examined are to be made perpendicular to the axis of the instrument, by the aid of the small gas flame mentioned in the first part of this paper: the telescope T is also most readily collimated by the use of the same flame, the screen, &c., having been temporarily removed.

3rd. The mirror M evidently must be brought into such a position that it shall send the ray MP perpendicularly to P, or along the axis of the instrument when its distance is such as to effect compensation; for if the reflected ray were sent obliquely through the ground glass plate G, noticeable errors would be introduced. This adjustment, which is important if good results are expected, is best effected by making one or two compensations so as to determine approximately the correct distance of the mirror, which is then rotated on its vertical axis so that the reflected image of the flame is made to fall in the center of the field of the telescope, the screen having been previously removed. By a repetition of this operation finally the mirror is brought into its proper position, when a *single* series of measurements can be made, the minute differences in the individual compensations introducing no appreciable errors. When, however, the compensation point itself has been shifted by the introduction or removal of a plate of glass, the mirror will correspondingly be moved away or toward S, and of course the ray MP will be sent a little to the right or left, and it becomes necessary to devise some simple way in which this difficulty can be obviated, without in each instance removing the screen and altering the focus of the telescope.

The mirror then being actually in adjustment, this, when lost, can be recovered as follows: in the screen B about four inches above the telescope is a circular aperture $\frac{1}{16}$ of an inch in width; the reflection of the flame in the mirror is observed through it with the naked eye, and the image of the flame is seen higher up on the mirror, and is made to coincide with a short black line drawn there previously. The ray MP can always thus be made to coincide with the axis of the instrument, it not even being necessary for the observer to leave his seat or diminish the sensitiveness of the eye by exposure. I am particular in describing this precaution, having in my own case rejected the results of 1300 compensations which were made with a comparative lack of attention to this single point.

Mode of measuring the amount of light transmitted by a plate of glass, &c.

1st. The plate is placed at P and collimated by the use of the small gas flame.

2nd. A compensation is effected by moving the mirror.

3rd. The mirror is adjusted by the use of the aperture in B, and another compensation made, followed by a second adjustment of the mirror, if necessary.

4th. A series of careful compensations are now made, *alternately by the approach and recession of the mirror;* these are registered on the fillet of paper.

5th. The plate is removed without the experimenter changing position or exposing the eye to bright light, and a compensation is made which necessitates of course a new adjustment of the mirror by the aid of the aperture in the screen B. Finally, a series of compensations are effected with the free flame and registered.

At this point it may be well to notice an objection which might be urged to this mode of experimenting, viz: it is evident that the angles at which the light is reflected from the mirror will not be identical in the presence and absence of the plate of glass, and as the amount of light reflected by glass and silver varies with the angle of reflection, this might become a source of error and necessitate a correction. With the plates of colorless glass employed by me, however, the angle in one case was 5° 30′ and in the other 5° 18′, the difference of 12′ being much too small to produce an appreciable effect, as we know from the results of older experiments.

For the purpose of showing what can be effected by this method, I can perhaps do no better than give the results of five sets of experiments; each of them consisted of four sets of eight double, or sixteen single compensations, and during the progress of each trial the conditions remained the same. The experiments were made as follows: eight double compensations were effected with the photometer and registered; then everything being arranged as before, a new set of compensations were made, &c., all of which was repeated four times. The arithmetical mean of the distances of the source of light from the screen was then obtained separately for each of the four experiments, and from these four mean values of the distance, a final mean value was deduced. This last quantity was then squared and compared separately with the squares of each of the first four mean distances, which proceeding tells at once the difference of the four results from the final mean result, in per cents or fractions of a per cent, and is adapted to give an idea of the degree of accuracy to be expected in this kind of observation.

Differences from mean in per cents:

No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
+·012	−·275	−·252	+·573	−·102
+·347	+·126	−·137	−·208	+·057
+·158	−·124	+·385	−·060	+·041
−·512	+·275	+·007	−·308	+·005

It will be observed that in the twenty experiments, only two cases occur in which the difference rises slightly above one half per cent, or above $\frac{1}{200}$ of the whole quantity of light, while in nearly all the other cases the difference falls considerably below this quantity, and the average of their differences is less than 0·2 per cent, or less than $\frac{1}{500}$ of the whole amount.

As the reliability of measurements with a photometer is evidently connected with the power of the eye in distinguishing different degrees of brightness in adjacent surfaces, it is well to review here briefly the results that have been obtained by different observers, as to the sensitiveness of the eye under favorable conditions.

The least difference which Boguer with his method, (two shadows,) was able to distinguish was $\frac{1}{64}$ of the whole; Fechner's friends using the same method were able to reduce this quantity to $\frac{1}{100}$. Arago remarked that where one of the illuminated surfaces was in motion smaller differences could be perceived, and was able under favorable circumstances to distinguish $\frac{1}{131}$, while Masson, who used revolving discs, along with his friends, was able to distinguish between $\frac{1}{50}$ to $\frac{1}{120}$ of the whole quantity of light. Near a window Helmholtz was able to distinguish with certainty a difference of $\frac{1}{133}$, and occasionally as small a quantity as $\frac{1}{167}$, while in the middle of a room he was able to appreciate certainly only $\frac{1}{117}$, seldom and uncertainly $\frac{1}{133}$.*

In all the better of the above mentioned experiments, photometers in any proper sense of the term were not employed, the best of them being made by the use of revolving discs of white paper furnished with narrow black sectors, which are calculated to discover the absolute sensitiveness of the eye under the most favorable conditions. It must not however be imagined that because a good eye can distinguish a difference of $\frac{1}{120}$ on a revolving disc, that the same eye would be able to obtain a similar result with ordinary forms of the photometer, where the conditions are far less favorable, and accordingly it seemed of interest to me to determine by a series of experiments the average sensitiveness of the eye while using the photometric method described in this paper, particularly as the plan of using a single flame and mirror offered great facilities for making these observations in a reliable manner. The following mode was employed: the light from the mirror was allowed to fall on the unobstructed screen, and the mirror was *drawn up* till the spot had disappeared, when the observation was registered; the next compensation was made by *pushing* the mirror *away*, and so on alternately, *the compensations by pull and push being registered on separate fillets of paper by two pens.* In the first experiment fourteen compensations were made by the advancing, and an equal number by the retreating mirror, and the mean of these twenty eight quantities was taken as the true distance of the mirror from the screen; then on comparing the mean of either set of compensations with this true distance, it was easy to ascertain how great a difference from the truth had been tolerated by the eye under the given circumstances. In the first experiment this quantity was found to be $\frac{1}{114}$, in a second similar trial $\frac{1}{125}$ of the total amount of light. The "screen" in these experiments was quite *new*, and an inferior result was obtained by using a "screen," which had for six weeks been exposed quite unprotected to the action of the air; here the usual darkening of the edges had begun and progressed so far as to be faintly visible during compensation.

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* Physiologische Optik, von H. Helmholtz, page 311.

In the experiments just detailed the highest *average* sensitiveness of the eye is only $\frac{1}{125}$, while as before shown in practical use the average difference from the mean is less than $\frac{1}{500}$ of the whole. This higher degree of accuracy results naturally from the mode in which the compensations are made, (alternately by approach and recession,) and the fact that the compensations are thus interwoven, in such a manner as to eliminate errors introduced by the slightly varying sensibility of the eye.

Experiments on the amount of light transmitted by plates of glass at a perpendicular incidence.

A knowledge of the amount of light transmitted and reflected by colorless transparent substances has a certain degree of interest from a technical point of view, enabling opticians to calculate the loss necessarily experienced in various instruments from this source, as well as the intensity where the light is reflected from a single or from two parallel surfaces of glass or other material. Of far more importance, however, is the interest attaching to this subject from a theoretical point of view, and especially in connection with the Undulatory Theory of Light. Formulas for the intensity of the reflected and transmitted beams at all angles have been deduced by eminent supporters of this theory, which in some cases have occasioned considerable discussion. The following very simple formula for the intensity of common light reflected at a perpendicular incidence was first given by Thomas Young; $I = \frac{(n-1)^2}{(n+1)^2}$; the intensity of the incident beam being equal to unity, and n being the index of refraction of the reflecting substance. The same formula was afterwards reached by Poisson in a rigid and learned analysis of the subject, and it was again deduced by Fresnel.* As is well known Fresnel's formulas were subsequently modified by the celebrated Cauchy, but in Cauchy's formula for reflection as soon as the incidence of the light differs considerably from the polarizing angle, the small quantities which depend on e, the coefficient of ellipticity, become so much reduced that they can be omitted from the numerator and denominator, and Cauchy's formula becomes identical with Fresnel's.† It hence appears that the formula above quoted is as well theoretically established, as any which has been deduced under the guidance of the Undulatory Theory, and one would naturally suppose that it had been often tested carefully by experiment. This does not seem to have been the case, and I do not know that it has ever been rigidly tried by a delicate photometric method. On this account I have made a series of observations on plates of colorless glass which are detailed below.

Mode of experimenting.

When a beam of light falls on a plate of glass or other transparent colorless substance, a certain portion will be reflected, another portion transmitted, a third absorbed. If the plate of glass be colorless and thin, the portion absorbed will be smaller than the necessary errors of observation, so that it can safely be neglected. For example, Bunsen found that in using a plate of crown glass 4·7 millimeters thick, that it absorbed only $\frac{4}{10}$ of one per cent of the *chemical* rays that fell on it at a perpendicular incidence.‡ The thickest plate of glass employed by me was 1·67 millimeters from surface to surface, i. e. about one-third of that used by Bunsen, and as we know that the chemical rays are extinguished by glass in a far larger proportion than those which are luminous, it follows that in the plates mentioned below we can safely neglect the internal extinction. This point being settled, the mode of proceeding becomes quite simple; it is only necessary to measure the amount of light transmitted, and the difference between the incident and transmitted light gives the amount of that reflected, and after making a correction for internal reflection, we shall have the means of comparing the results of theory and experiment.

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* Pogg. Annalen, Bd. xxii, p. 98.

† Jamin in Pogg. Erg., Bd. iii, p. 256.

‡ Pogg. Annalen, Bd. ci, p. 241.

Mode of determining the indices of refraction.

In my experiments on transparent substances, always where it was possible prisms of the substance were ground and the index of refraction of the sodium line determined as usual with a graduated circle, collimating and observing telescope. In the particular experiments detailed at the termination of this article, the plates of glass used were so thin that it was not practicable to grind from them prisms, and for all such cases I contrived, tested and used two somewhat new modes of procedure, as neither the method of the Duke de Chaulnes (alteration of the focus of a microscope,) nor that of Bernard (displacement of an image viewed obliquely through a plate,) were here found to give reliable results.

1. A minute angular fragment of the glass to be experimented on was placed in a cell, on a glass slide, like those used for mounting microscopic objects, and surrounded with a mixture of "body-sperm-oil" and oil of cassia, the proportions being varied till the refraction of the glass for the sodium line had been exactly compensated by the oil. *Olive oil* became turbid when mixed with the oil of cassia and hence could not be used. The mode of comparing the refractive power of the mixture of the oils and glass was as follows: at the distance of half an inch below the level of a microscope stage was a fine slit, cut in tinfoil which had been pasted on glass; the microscope was focussed on this, a sodium flame being used to illuminate it; the cell with the oil and fragment of glass was then placed on the stage of the microscope, and moved so that the light from the slit passed through the angular fragment, when it would happen that the line of light would be refracted to the right or left hand according as oil or glass predominated in refractive power, which made it instantly evident whether sperm oil or oil of cassia was needed. A number of experiments were made to test this method, which was found to answer well, the index of refraction as determined by prism corresponding with that obtained by the use of the fragment: so in the case of a sample of crown glass a triangular prism gave the index of refraction as 1·526, while by the new method it was found to be 1·529. This method, however, is capable of still more accurate results, as in the above mentioned experiments, the compensation was pushed only far enough to answer my immediate purpose; that is to say, the fragment may be considered to consist of one *large* triangular prism with a moderate angle, and a number of smaller prisms, some of which are sure to have very large angles; these latter become effective when the glass is under oil, total reflection no longer taking place, and they act powerfully on the light coming from the slit, still furnishing faint images having

a considerable deviation even after the main portion of the glass fragment has ceased to perceptibly deflect it.

In no case did I push the compensation far enough to gather in all these outstanding beams.

2. Another method, which in the case of crown glass was found to answer quite well enough for my purpose and to be very convenient, consisted in fusing to a spherical globule a fragment of the glass, placing it in the mixed oils and effecting compensation by observing when the globule ceased to act as a lens, for which purpose a small telescope or the microscope can be employed. Thus, for example, a certain kind of crown glass when ground into a prism gave as index of refraction for the sodium line 1·526, while when tested according to this second method, the results of two experiments were 1·5232 and 1·5235.

I give now the results of careful sets of experiments on the amount of light transmitted by two different samples of crown glass: in each case the results of four independent trials are given, each trial being worked out with the aid of seven double compensations. In the first case the thickness of the glass was ·15 millimeters, the index of refraction 1·5236, and allowing for the effects of internal reflection, it should, according to theory, have transmitted 91·736 per cent of the light falling on it. Experiment gave:

92·227
91·371
91·019
91·143

91·440

The difference ·296 being hardly larger than the necessary error connected with the method of making the determination.

In the second case the index of refraction was 1·5225, the thickness 1·677 millimeters, and by theory it should have transmitted 91·763 per cent of the light falling on it. Below are the actual results obtained:

90·886
90·948
90·892
91·895

91·155

The difference here of ·500 per cent, or $\frac{1}{200}$ of the whole amount, is almost equally satisfactory, and these experiments show, I think, that the reflecting power of glass with the above index of refraction, *conforms in the closest manner to the predictions of theory.* Elaborate experiments were also made with flint glass, quartz and calc-spar, but I suppress the results, as it afterwards turned out that they were contaminated with minute errors of the character described in this article under the head "mode of adjusting, &c." 3rd; the adjustment alluded to having indeed been always made, but not with a sufficient degree of care to exclude the last trace of error.

New York, March 29th, 1870.

ART. II.—*The Ethers of Arsenic Acid and of Arsenious Acid;* by J. M. CRAFTS.*

MR. FRIEDEL and I observed, while studying the ethers of silicic acid, that the silicate of ethyl is readily decomposed by heating it with anhydrous boracic acid in a sealed tube; that pure borate of ethyl is formed, and that silicic acid, or an ether containing a very large proportion of silicic acid, is deposited in the tube. It occurred to me that a similar reaction might offer a convenient method of preparing the ethers of feeble acids. The ethers of the acids of arsenic and antimony and of tungstic acid have not yet been obtained, and experiments were made with a view to their preparation, but I have only succeeded in the case of arsenious acid in obtaining a new ether by the action of the acid upon silicic ether. Another method, however, afforded the means of preparing the ethers of arsenic acid, and several different methods of preparing the ethers of arsenious acid have been discovered. The present paper is devoted to the description of the methylic, ethylic and amylic ethers of both acids of arsenic.

ARSENIATE OF ETHYL.

The first attempts were made to prepare this body by the action of arsenic acid on the silicate of ethyl. The arsenic acid was dried by heating it in a current of dry air, and it was then sealed in a glass tube with the silicate of ethyl, and heated in an air-bath. After heating 10 hours at 210° centigrade there appeared to be no reaction; on heating 3 hours longer at 230°, a considerable quantity of the arsenic acid dissolved and at a little higher temperature the tube exploded, probably in consequence of the oxydation of the ether by the arsenic acid.

In another experiment 20 grams of silicate of ethyl were heated with 8 grams of arsenic acid for 6 hours at 220°–230°. A gelatinous silicate of ethyl was deposited, and on opening the tube about $\frac{1}{2}$ litre of a gas having the properties of ethylene was evolved. A considerable quantity of common ether was formed, and the remainder of the liquid contents of the tube was partially decomposed on distillation. After several distillations a small quantity of a body was isolated, which contained arsenious, but no arsenic acid, and which proved to be an impure arsenious ether. It is evident from these results that, at the temperature required for the action of arsenic acid upon the silicate of ethyl, the greater part of the product is decomposed with formation of common ether and ethylene, while a small quantity of the arsenic acid is reduced and arsenite of ethyl is formed, and that the reaction does not furnish any arseniate of ethyl.

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* The chemical symbols used have the values which belong to them in the new system.

The arseniate of ethyl can be easily prepared by the action of the iodid of ethyl on the arseniate of silver—

$$3C_2H_5I+Ag_3AsO_4=3AgI+(C_2H_5)_3AsO_4.$$

When the two bodies, mixed with pure anhydrous ether are heated together at about 90° in a sealed tube, the reaction is completed in a few hours, and the point at which this takes place is marked by the change of color of the silver salt from red to light yellow. It is important that the arseniate of silver should be in excess and that the temperature should not exceed 100°, as the iodid of ethyl decomposes a portion of the arseniate of ethyl, and the decomposition takes place more readily, the larger the quantity of iodid and the higher the temperature.

The liquid together with the iodid of silver is taken from the tube and washed with pure ether until it is entirely freed from arseniate of ethyl. This operation can be most conveniently performed on a funnel, provided with a glass plate ground to fit it. A hole should be drilled in the plate, and a funnel tube ground into it, in order that the liquid may be poured upon the filter without removing the glass plate.

The solution in ether is distilled and finally heated in a water-bath, while a current of dry air is passed through it to carry off the last trace of ether. The arseniate of ethyl thus obtained can be most easily purified by distillation in a partial vacuum, as it is decomposed in part by distillation in the air. In operations of this kind it is preferable to distil under a diminished pressure rather than to push the exhaustion of the air as far as possible since the boiling point remains more constant, when the pressure is high. This probably is due in great part to the fact, that a slight variation of pressure does not alter the relative pressure so much, when the exhaustion is incomplete, as when it is nearly complete. For instance, the pressure may well vary 2 millimeters during the course of a distillation, and a variation from 5 to 7 millimeters affects the boiling point much more than a variation from 200 to 202 millimeters.

In the case of the arseniate of ethyl it was found that it distilled under a pressure of 60 millimeters at about 85° lower than in the air, and that this difference of temperature was sufficient to prevent decomposition. The distilling apparatus was provided with an air reservoir and with a mercury guage, and the pressure was kept nearly constant at 60 millimeters during the course of the distillation.

Arseniate of ethyl distils at 148°–150° under a pressure of 60 millimeters.

About $\frac{4}{5}$ of the theoretical quantity are obtained by the above method of preparation.

The arsenic ethers are immediately decomposed by smallest traces of water and it is essential that the common ether used in their preparation should be absolutely anhydrous. It is also important for the operation in the sealed tube, that it should contain no alcohol, since the iodid of ethyl acts upon alcohol with formation of ordinary ether and water. Ether can be most easily freed from small quantities of water and alcohol by digesting it several times with shavings of sodium, which can be cut with a knife from a large piece, allowing it to stand each time 24 hours with the sodium, and then distilling. Ether so prepared can be heated with sodium to 90° in a sealed tube without the smallest evolution of hydrogen.

The following analyses were made of arseniate of ethyl distilling at 148°–150° under a pressure of 60 millimeters.

I. *Substance* = 0·2980 *grms.*; CO_2 = 0·3470 *grms.*; H_2O = 0·1740 *grms.*

II. *Substance* = 0·9260 *grms.*; *arseniate of magnesium and ammonium* = 0·7705 *grms.*

The determination of arsenic was made by decomposing a weighed quantity of the arseniate of ethyl by water, evaporating with nitric acid, and determining the arsenic acid as the arseniate of magnesium and ammonium on a weighed filter.

	I.	II.	Calculated for $(C_2H_5)_3AsO_4$
C =	31·79	....	31·86
H =	6·49	....	6·64
As =	....	32·85	33·18

The density of liquid arseniate of ethyl at 0° compared with that of water at 4°=1·3264. Its density at 8·8°=1·3161.

The density of vapor of arseniate of ethyl can not be taken on account of its decomposition by heat. This decomposition, however, is not so great, but that its boiling point in the air can be determined with sufficient accuracy. When a portion of the arseniate of ethyl, which has been purified by distillation under a diminished pressure, is distilled in the air, the greater part of it passes at 235°–238°, but toward the last part of the distillation decomposition ensues, and a white frothy deposit of arsenic acid is left in the retort.

Arseniate of ethyl attracts moisture from the air and is immediately decomposed with formation of arsenic acid and alcohol. It dissolves immediately in water, undergoing the same decomposition. Particular attention was paid to the question of the existence of ethyl-arseniates, and experiments were made under the most favorable conditions for observing their formation, but thus far with negative results. Felix D'Arcet, (Ann. Chem. und Pharm., xix, p. 202), gives the analysis of the barium salt of the di-ethyl arsenic acid $Ba(C_2H_5)_4(AsO_4)_2$; but Hugo Schiff (Ann. Chem. und Pharm., cxi, p. 370,) failed to obtain this body on dissolving arsenic acid in alcohol, evaporating until the solution acquired the consistency of a syrup, dissolving in water and neutralizing with baric carbonate. This result proves at least that no ethyl arsenic acid can exist in aqueous solution, and my own experiments confirm those of Mr. Schiff. It appeared possible that when the arseniate of ethyl is decomposed by water, the decomposition might pass through two stages, and that ethyl arsenic acids might at least exist for a time in the aqueous solution, even though they were ultimately completely destroyed. In order to test this idea, a weighed quantity of arseniate of ethyl was dissolved in water, litmus solution was added and it was neutralized immediately with a standard solution of ammonia. If the decomposition with water had been a gradual one with formation of the acids mentioned, the neutralized solution should have developed an acid reaction after the lapse of a considerable time. This was not the case. The same amount of ammonia was required for neutralization, either immediately, or after the solution had stood 24 hours, and moreover, the reaction was exactly similar to that of a corresponding quantity of pure arsenic acid dissolved in water. It should be noticed that no sharp change of color can be obtained by neutralizing a solution of arsenic acid containing litmus with ammonia.

Another attempt was made to obtain the ethyl arsenic acid in an alcoholic solution. Pure arseniate of ethyl was mixed with alcohol, and only enough aqueous alcohol was added to furnish water for the decomposition expressed by the reaction:

$$(C_2H_5)_3\,AsO_4+H_2O = H(C_2H_5)_2\,AsO_4+C_2H_6O;$$

and the mixture was distilled in vacuo. It behaved exactly like a solution of arsenic acid in arseniate of ethyl: the latter distilling and leaving the anhydrous arsenic acid as a residue. It was thought probable that in this experiment the first product of the action of water would enter into the following reaction with some of the undecomposed arseniate of ethyl:

$$H(C_2H_5)_2\,AsO_4+(C_2H_5)_3\,AsO_4=(C_2H_5)_4As_2O_7+C_2H_6O;$$

but no condensed ether like the one whose formula is given was obtained, and the only result of the decomposition with aqueous alcohol under these circumstances is the setting free of arsenic acid and alcohol.

Arseniate of ethyl is immediately decomposed by ammonia with the formation of a crystalline body, which it is very difficult to obtain pure on account of the extreme avidity with which it absorbs moisture. The results of several analyses lead to the conclusion, that the body which is first formed has the composition: $(C_2H_5)_2NH_2AsO_3$, and that it absorbs water to form a di-ethyl arseniate of ammonium, $(C_2H_5)_2NH_4AsO_4$, but the study of this product is not yet completed.

ARSENIATE OF METHYL.

The arseniate of methyl can be prepared from the iodid of methyl and arseniate of silver in the same way that the arseniate of ethyl is prepared. The crude product is purified by distillation under a pressure of sixty millimeters. At this pressure it distils without decomposition at 128°–130°.

I. *Substance*=0·4500 *grms.*; CO_2=0·3285 *grms.*; H_2O=0·1980 *grms.*

II. *Substance*=0·8070 *grms.*; As_2O_5=0·4966 *grms.*

The determination of arsenic was made by decomposing the ether with water, evaporating with nitric acid, and heating the arsenic acid thus formed with oxyd of lead.

	I.	II.	Calculated for $(CH_3)_3AsO_4$.
C =	19·90	----	19·57
H =	4·91	----	4·89
As=		40·15	40·76

The density of liquid of arseniate of methyl at 14°·5, compared with water at 4°, =1·5591.

When pure arseniate of methyl is distilled in the air, the greater part passes at 213°–215°, but there is a partial decomposition with separation of arsenic acid.

It resembles the arseniate of ethyl in all its properties. It is a colorless liquid, miscible in all proportions with water, and it is decomposed immediately into arsenic acid and alcohol by the action of water or moisture for the air.

ARSENIATE OF AMYL.

This body is produced by action of iodid of amyl on the arseniate of silver, but it cannot be obtained in a state of purity, because it decomposes, even when distilled in vacuo.

The crude product of the reaction, after having been heated to 170°, was mixed with pure ether, when a considerable quantity of arsenious acid was precipitated; after filtering, the solution was heated in an oil-bath, and finally a current of dry air was passed through it, while the bath was kept at 200°. An analysis of the product so obtained gave C=39·31 per cent and H=7·64 per cent, instead of C=51·14 per cent and H=9·37 per cent.

A somewhat better result was obtained by distilling the crude product in vacuo. The greater part is decomposed at about 200°, but a small quantity of ether distils unchanged. Some of this distillate was heated to 195° in current of dry air, in order to free it as far as possible from products of decomposition. The analysis of this product gave C=44·82 per cent and H=8·41 per cent instead of C=51·14 per cent and H=9·37 per cent.

The products analyzed appear to have been arseniate of amyl mixed with arsenic acid.

ARSENITE OF ETHYL.

The ethers of arsenious acid can be prepared more readily than these of arsenic acid, and several reactions may be employed to obtain them.

Arsenite of ethyl is produced in the following reactions:

By the action of arsenious acid on the silicate of ethyl. By the action of iodid of ethyl on the arsenite of silver. By the action of the chlorid or bromid of arsenic on the alcoholate of sodium. The iodid of arsenic and the alcoholate of sodium give no arsenite of ethyl.

When arsenious acid is heated with alcohol to about 200° in a sealed tube, a considerable quantity of the acid dissolves, and crystallizes out on cooling as beautifully formed octahedrons. A small quantity of arsenic is reduced and a corresponding quantity of aldehyd is formed. No arsenious ether is formed.

Arsenious acid was heated with a mixture of common ether and acetate of ethyl for 20 hours at 200°, but no reaction took place.

It seemed highly probable that arsenite of ethyl might be obtained by heating together, common ether and chlorid of arsenic according to the reaction

$$AsCl_3+3(C_2H_5)_2O=(C_2H_5)_3AsO_3+3C_2H_5Cl,$$

but this proved not to be the case, for after the two substances had been heated together 20 hours at 200°, it was found that no chlorid of ethyl and no arsenite of ethyl had been formed.

It has been already noticed that by a process of reduction arsenite of ethyl is produced, when arsenic acid is heated with silicic ether. When arsenious acid is heated to about 200° in a sealed tube with silicic ether, the only reaction which takes place is the replacement of the silicic acid by the arsenious acid, a gelatinous deposit of a silicic ether, containing a very large quantity of silicic acid, is formed in the tube; no alcohol, ether or gaseous body is produced, and the arsenious ether can easily be obtained pure by distillation.

Iodid of ethyl acts less readily upon the arsenite of silver than upon the arseniate of silver, and the amount of product obtained approaches less nearly to the theoretical quantity; there is, however, no difficulty in obtaining pure arsenious ether by heating together iodid of ethyl and dry arsenite of silver mixed with pure ether in a sealed tube to 150°. The arsenious ether can be separated by washing the silver salt with common ether and finally by distilling the solution in ether.

The action of alcoholate of sodium upon the chlorid, or, better still, upon the bromid of arsenic, furnishes the best method for obtaining the arsenite of ethyl.

It is well known that the chlorid of arsenic combines with alcohol with evolution of heat to form a definite compound,* which distils without decomposition.

When the bromid of arsenic is mixed with an equal volume of alcohol, the heat evolved is very slight (an elevation of temperature of 3° was noticed), and no definite combination appears to be formed. If alcoholate of sodium is added to a mixture of either the chlorid or bromid of arsenic with alcohol, a precipitation of chlorid or bromid of sodium takes place immediately, and arsenite of ethyl is formed, but if the addition of the alcoholate is continued until all the chlorine or bromine is combined with sodium, the arsenite of ethyl is destroyed at the same time.† If the operation is terminated before all the chlorid or bromid of arsenic has been acted upon, a mixture of the undecomposed chlorid or bromid with arsenite of ethyl is obtained.

It was thought at first that the difference between the boiling point of the bromid of arsenic (222°) and that of the arsenite of ethyl (166°) would suffice for their separation, but this proved not to be the case, for a mixture of the two bodies in any proportion distils like a definite compound, and no separation by fractional distillation is possible. This kind of combination between the chlorid or bromid of arsenic and the ethers of arsenious acid will be noticed below.

The arsenite of ethyl, which is produced in the reaction above described, can only be isolated by taking advantage of its singular property of resisting entirely the action of dry ammonia gas, while the chlorid or bromid of arsenic combines with ammonia to form a crystalline solid insoluble in alcohol and ether. The method which was usually employed to prepare the arsenite of ethyl consists in dissolving 5 parts of bromid of arsenic in absolute alcohol and in adding to the solution the liquid obtained by adding 1 part of sodium to a considerable quantity of alcohol. The reaction is not violent, and it is not necessary to cool the vessel in which it takes place.

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* De Luynes, Ann. Chem. und Pharm., cxvi, p. 368.

† See paper in Comptes Rendus, April, 1867.

The arsenite of ethyl and undecomposed bromid of arsenic are separated from the bromid of sodium precipitate by washing with alcohol. The alcoholic solution is then distilled in a water-bath, pure ether is added to the residue, and dry ammonia gas is passed through the solution in ether in order to precipitate the bromid of arsenic, which is mixed with the arsenite of ethyl. The latter is separated from precipitate by washing with ether, the ether is distilled in a water-bath, and finally a current of dry air or carbonic acid is passed through. The residue consists of arsenite of ethyl containing some arsenious acid and bromid of sodium in solution. It can be purified completely by two or three distillations. Pure arsenite of ethyl boils at 165°–166° (no correction).

Analyses were made of products obtained by the three methods of preparation described above.

I. *Substance*=0·2320 *grms.; CO_2*=0·2925 *grms.; H_2O*=0·1590 *grms.*
II. *Substance*=0·4960 *grms.; C_2O*=0·6245 *grms.; H_2O*=0·3160 *grms.*
III. *Substance*=0·2510 *grms.; arseniate of magnesium and ammonium*=0·2311 *grms.*

The determination of arsenic was made by weighing the substance in a bulb, breaking the bulb under a solution of caustic potash, oxydizing the arsenious acid by chlorate of potash and chlorhydric acid at a temperature of 50°, and precipitating by sulphate of magnesium and chlorid of ammonium in the ordinary way.

	I.	II.	III.	Calculated for $(C_2H_5)_3As\,O_3$.
C=	34·38	34·33	----	34·29
H=	7·61	7·08	----	7·14
As=	....	----	36.34	35·71

The density of liquid arsenite of ethyl compared with that of water at 4°=1·224.

Several vapor density determinations were made at different temperatures by Dumas' method.

Difference between the weights of the bulb=0·9610 *grms.*
Temperature of the balance, = 4·°2
Temperature of the oil-bath (air thermometer) =267·°0
Capacity of the bulb, =269·5 *c. c.*
Air remaining, = 0·5 *c. c.*

Vapor density=7·389 Theory=7·2673

The vapor density at 209·°5=7·615
" " 213° =7·608
" " 233° =7·197

The last determination is probably too small through some fault of manipulation. It is to be expected that the numbers should be a little too high, since the arsenite of ethyl is decomposed by the slightest trace of moisture, leaving a deposit of arsenious acid in the bulb. The arsenite of ethyl is a colorless liquid with a peculiar, but not disagreeable, odor, and sufficiently volatile to make its presence plainly perceptible in a room, where it is standing in an open test-tube.

The ethers of arsenic do not appear to have the extraordinarily poisonous properties, which are attributed to arseniuretted hydrogen, at least I have suffered no inconvenience after having worked with them for several years.

Arsenite of ethyl is miscible in any proportion with alcohol and ether, but it is decomposed by aqueous alcohol. The only products of decomposition are arsenious acid and alcohol, and no compound ether with the formula, $(C_2H_5)_4As_2O_5$, is formed. This ether is also not produced according to the reaction:

$$4(C_2H_5)_3\,AsO_3+As_2O_3=3(C_2H_5)_4As_2O_5$$

when arsenite of ethyl is heated to 300° with arsenious acid. Arsenite of ethyl at its boiling point dissolves about $\frac{1}{8}$ its weight of arsenious acid, but a considerable quantity separates out, when the solution is allowed to become cool. Arsenite of ethyl can be distilled unchanged over dry arsenious acid.

The reaction between the arsenite of ethyl and bromhydric acid is the reverse of that which is usual between bromhydric acid and the ethers of acids. Usually the bromid of the alcoholic radical is formed, and the acid is set free. In this case the reaction leads to the formation of alcohol and bromid of arsenic, and it may be expressed by the equation:

$$(C_2H_5)_3AsO_3+3HBr=3C_2H_6O+As\,Br_3.$$

All that is required to study this reaction is to pass dry bromhydric acid through arsenite of ethyl, when the alcohol can be separated by distillation, leaving a crystalline residue of bromid of arsenic.

It was thought possible that bromine might combine directly with the arsenite of ethyl to form the compound: $(C_2H_5)_3AsO_3Br_2$, but it was found that the bromine substitutes itself for hydrogen in the ethyl, and that the substituted ether thus formed is decomposed by the bromhydric acid which is evolved in its formation.

Iodine does not act upon the arsenite of ethyl, and the two bodies can be separated by distillation. Dry ammonia gas is entirely without action upon the arsenite of ethyl as well as upon the other ethers of arsenious acid, and they can even be distilled in an atmosphere of ammonia without alteration.

ARSENITE OF METHYL.

The arsenite of methyl was prepared by heating dry arsenious acid with the silicate of methyl for 12 hours at 160°, and very nearly the theoretical quantity was obtained.

It was also obtained by the action of iodid of methyl on arsenite of silver in a sealed tube. The reaction does not take place at a temperature lower than 150°, and at that temperature a considerable part of the arsenious ether is destroyed, so that the first method of preparation is to be preferred.

The arsenite of methyl boils at 128°–129° (no correction).

I. *Substance*=0·4990 *grms.*; Co_2=0·3966 *grms.*; H_2O=0·2442 *grms.*

II. *Substance*=0·3725 *grms*; As_2O_5=0·2525 *grms.*

The arsenic determination was made by decomposing the ether with water, oxydizing with nitric acid, evaporating, and combining the arsenic acid with oxyd of lead.

	I.	II.	Calculated for $(CH_3)_3AsO_3$.
C=	21·67	----	21·44
H=	5·44	----	5·36
As=	----	44·21	44·64

The density of liquid of arsenite of methyl at 9°·6 compared with that of water at 4°=1·428. The vapor density was determined from the following data:

Difference between weights of bulb=0·5265 *grms.*
Temperature of the balance, =16°·4
Temperature of the oil-bath (*air-thermometer*)=197°
Capacity of the bulb, =161·5 *c. c.*
Air remaining, = 1·0 *c. c.*
Vapor density=6·006 Theory=5·818

The determination probably came out too high on account of the decomposition of a small quantity of ether by the moisture of the air.

The arsenite of methyl resembles the arsenite of ethyl in its properties. It is decomposed immediately by water and also by the moisture of the air with formation of arsenious acid and methylic alcohol.

In endeavoring to obtain the chlorhydrines of these ethers a singular class of compounds in indefinite proportions, but with constant boiling points, was discovered. The composition of the chlorhydrines of the arsenite of methyl would be represented by the formulas: $(CH_3)_2ClAsO_2$; CH_3Cl_2AsO; and in order to obtain them, arsenious acid was heated with the chlorhydrines of the silicate of methyl.* In this way bodies can be easily obtained, having the required composition and constant boiling points, but bodies with their constituents in every intermediate proportion between these, whose formulas are given above, can also be obtained, and they have equally constant boiling points. In fact, by combining the chlorid of arsenic and an arsenious ether in indefinite proportions, a body with a constant boiling point can be obtained; and what renders this fact most remarkable, is that the boiling point is not a mean between those of the two bodies mixed, but it is higher than either of them. The boiling point of arsenite of methyl is 129° (no correction). The boiling point of chlorid of arsenic is 133° (no correction). A product containing 42 per cent of chlorid of arsenic, and 58 per cent of arsenite of ethyl distilled unchanged at 143°–144°. A product containing 11·5 per cent of chlorid of arsenic and 88·5 per cent of arsenite of ethyl distilled at 131°–132°. I am not aware that an analogous case of combination in indefinite proportions has ever been observed.

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* This Journal, II, xliii, p. 155 and 331.

The boiling point of the arsenite of ethyl is 166° (no correction). Two parts of arsenite of ethyl and one part of chlorid of arsenic mix with slight disengagement of heat, and the product distils constantly at 159°–164° (no correction).

A mixture of equal parts of chlorid of arsenic and of arsenite of ethyl distils at 150°–152°.

A body was also obtained by the action of alcoholate of sodium upon the bromid of arsenic in excess, which contained 65·75 per cent of bromid of arsenic and 34·25 per cent of arsenite of ethyl, and which distilled constantly at 182°–185° (no correction). The chlorid or bromid of arsenic in all these bodies combines directly with dry ammonia gas, leaving the arsenious ether free.

ARSENITE OF AMYL.

The arsenite of amyl was prepared by the action of the amylate of sodium upon the bromid of arsenic in the same way as was described for the preparation of the arsenite of ethyl.

The product, however, cannot be purified by distillation in the air, since it decomposes in part at its boiling point. It can, however, be purified by distillation under a pressure of 60 millimeters. The following analysis was made of a product boiling at 193°–194° (no correction) under a pressure of 60 millimeters.

Substance=0·4090 *grms.*; CO_2=0·7950 *grms.*; H_2O=0·3578 *grms.*

	Calculated for $(C^5H_{11})_3AsO_3$.
C=53·01	53·57
H= 9·72	9·82

The density of liquid arsenite of amyl at 0° compared with that of water at 4° = 1·0525. Arsenite of amyl, which has been purified by distillation under a diminished pressure, distils in the air at 288°–290°, but toward the last part of the distillation arsenious acid separates out.

Arsenite of amyl is not more stable than the other ethers of arsenious acid. It is immediately decomposed by water and also by the moisture of the air.

Arsenite of amyl has analogous properties with the arsenites of ethyl and methyl.

The relations between the boiling points of the bodies described in this paper present some curious anomalies, and do not fall within Kopp's law derived from the study of the more simple ethers.

Boiling points under a pressure of 60 millemeters:

$$(C_2H_5)_3AsO_4 = 149°$$
$$(CH_3)_3AsO_4 = 129°$$

Difference = 20°

Boiling points under the atmospheric pressure:

$$(C_2H_5)_3AsO_3 = 166°$$
$$(CH_3)_3AsO_3 = 129°$$

Difference = 37°

In the first case the difference of boiling point for each increment of CH_2 is $6\frac{2}{3}°$.

In the second case the difference is $12\frac{1}{3}°$.

I have already stated in a communication to the French Academy of Sciences, (April 2, 1867), that tungstic and antimonius acids give no ethers when they are heated with the silicate of ethyl.

Art. III.—*On the probable seat of Volcanic Action;* by T. Sterry Hunt, LL.D., F.R.S.*

The igneous theory of the earth's crust, which supposes it to have been at one time a fused mass, and to still retain in its interior a great degree of heat, is now generally admitted. In order to explain the origin of eruptive rocks, the phenomena of volcanos, and the movements of the earth's crust, all of which are conceived by geologists to depend upon the internal heat of the earth, three principal hypotheses have been put forward. Of these the first supposes that in the cooling of the globe a solid crust of no great thickness was formed, which rests upon the still uncongealed nucleus. The second hypothesis, maintained by Hopkins and by Poulett Scrope, supposes solidification to have commenced at the center of the liquid globe, and to have advanced toward the circumference. Before the last portions became solidified, there was produced, it is conceived, a condition of imperfect liquidity, preventing the sinking of the cooled and heavier particles, and giving rise to a superficial crust, from which solidification would proceed downward. There would thus be enclosed, between the inner and outer solid parts, a portion of uncongealed matter, which, according to Hopkins, may be supposed still to retain its liquid condition, and to be the seat of volcanic action, whether existing in isolated reservoirs or subterranean lakes; or whether, as suggested by Scrope, forming a continuous sheet surrounding the solid nucleus, whose existence is thus conciliated with the evident facts of a flexible crust, and of liquid ignited matters beneath.

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* From the Geological Magazine for June, 1869.

Hopkins, in the discussion of this question, insisted upon the fact established by his experiments, that pressure favors the solidification of matters which, like rocks, pass in melting to a less dense condition, and hence concludes that the pressure existing at great depths must have induced solidification of the molten mass at a temperature at which, under a less pressure, it would have remained liquid. Mr. Scrope has followed this up by the ingenious suggestion that the great pressure upon parts of the solid igneous mass may become relaxed from the effect of local movements of the earth's crust, causing portions of the solidified matter to pass immediately into the liquid state, thus giving rise to eruptive rocks in regions where all before was solid.*

Similar views have been put forward in a note by Rev. O. Fisher, and in an essay on the formation of mountain chains, by Mr. N. S. Shaler, in the proceedings of the Boston Society of Natural History, both of which appear in the *Geological Magazine* for November last. As summed up by Mr. Shaler, the second hypothesis supposes that the earth "consists of an immense solid nucleus, a hardened outer crust, and an intermediate region of comparatively slight depth, in an imperfect state of igneous fusion." In this connection it is curious to remark that, as pointed out by Mr. J. Clifton Ward, in the same Magazine for December (page 581), Halley was led, from the study of terrestrial magnetism, to a similar hypothesis. He supposed the existence of two magnetic poles situated in the earth's outer crust, and two others in an interior mass, separated from the solid envelope by a fluid medium, and revolving, by a very small degree, slower than the outer crust.* The same conclusion was subsequently adopted by Hansteen.

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* See Scrope on Volcanos, and his communication to the *Geological Magazine* for Dec., 1868.

The formation of a solid layer at the surface of the viscid and nearly congealed mass of the cooling globe, as supposed by the advocates of the second hypothesis, is readily admissible. That this process should commence when the remaining envelope of liquid was yet so deep that the refrigeration from that time to the present has not been sufficient for its entire solidification, is, however, not so probable. Such a crust on the cooling superficial layer would, from the contraction consequent on the further refrigeration of the liquid stratum beneath, become more or less depressed and corrugated, so that there would probably result, as I have elsewhere said, "an irregular diversified surface from the contraction of the congealing mass, which at last formed a liquid bath of no great depth, surrounding the solid nucleus." Geological phenomena do not, however, in my opinion, afford any evidence of the existence of yet unsolidified portions of the originally liquid material, but are more simply explained by the third hypothesis. This, like the last, supposes the existence of a solid nucleus, and of an outer crust, with an interposed layer of partially fluid matter, which is not, however, a still unsolidified portion of the once liquid globe, but consists of the outer part of the congealed primitive mass, disintegrated and modified by chemical and mechanical agencies, impregnated with water, and in a state of igneo-aqueous fusion.

The history of this view forms an interesting chapter in geology. As remarked by Humboldt, a notion that volcanic phenomena have their seat in the sedimentary formations, and are dependent on the combustion of organic substances, belongs to the infancy of geology. To this period belong the theories of Lémery and Breislak (*Cosmos*, v, 443; Otte's translation). Keferstein in his *Naturgeschichte des Erdkörpers*, published in 1834, maintained that all crystalline non-stratified rocks, from granite to lava, are products of the transformation of sedimentary strata, in part very recent, and that there is no well-defined line to be drawn between Neptunian and volcanic rocks, since they pass into each other. Volcanic phenomena, according to him, have their origin not in an igneous fluid center, nor in an oxydizing metallic nucleus (Davy, Daubeny), but in known sedimentary formations, where they are the result of a peculiar kind of fermentation, which crystallizes and arranges in new forms the elements of the sedimentary strata, with an evolution of heat as a result of the chemical process (*Naturgeschichte*, vol. i, p. 109; also *Bull. Soc. Geol. de France* [1], vol. vii, p. 197). In commenting upon these views (*Am. Jour. Science*, July, 1860), I have remarked that, by ignoring the incandescent nucleus as a source of heat, Keferstein has excluded the true exciting cause of the chemical changes which take place in the buried sediments. The notion of a subterranean combustion or fermentation, as a source of heat, is to be rejected as irrational.

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* The elevated temperature of the interior of the globe would probably offer no obstacle to the development of magnetism. In a recent experiment of M. Trève, communicated by M. Faye to the French Academy of Sciences, it was found that molten cast iron, when poured into a mould, surrounded by a helix which was traversed by an electric current, became a strong magnet when liquid at a temperature of 1300° C., and retained its magnetism while cooling (Comptes Rendus de l'Acad. des Sciences, Feb., 1869.

A view identical with that of Keferstein, as to the seat of volcanic phenomena, was soon after put forth by Sir John Herschel, in a letter to Sir Charles Lyell, in 1836 (*Proc. Geol. Soc. London*, ii, 548). Starting from the suggestion of Scrope and Babbage, that the isothermal horizons in the earth's crust must rise as a consequence of the accumulation of sediments, he insisted that deeply buried strata will thus become crystallized by heat, and may eventually, with their included water, be raised to the melting point, by which process gases would be generated, and earthquakes and volcanic eruptions follow. At the same time the mechanical disturbance of the equilibrium of pressure, consequent upon a transfer of sediments, while the yielding surface reposes on matters partly liquified, will explain the movements of elevation and subsidence of the earth's crust. Herschel was probably ignorant of the extent to which his views had been anticipated by Keferstein; and the suggestions of the one and the other seemed to have passed unnoticed by geologists until, in March 1858, I reproduced them in a paper read before the Canadian Institute (Toronto,) being at that time acquainted with Herschel's letter, but not having met with the writings of Keferstein. I there considered the reactions which would take place under the influence of a high temperature in sediments permeated with water, and containing, besides siliceous and aluminous matters, carbonates, chlorids, and carbonaceous substances. From these, it was shown, might be produced all the gaseous emanations of volcanic districts, while from aqueo-igneous fusion of the various admixtures might result the great variety of eruptive rocks. To quote the words of my paper just referred to: "We conceive that the earth's solid crust of anhydrous and primitive igneous rock is everywhere deeply concealed beneath its own ruins, which form a great mass of sedimentary strata, permeated by water. As heat from beneath invades these sediments, it produces in them that change which constitutes normal metamorphism. These rocks, at a sufficient depth, are necessarily in a state of igneo-aqueous fusion; and in the event of fracture in the overlying strata, may rise among them, taking the form of eruptive rocks. When the nature of the sediments is such as generate great amounts of elastic fluids by their fusion, earthquakes and volcanic eruptions may result, and these—other things being equal—will be most likely to occur under the more recent formations." (*Canadian Journal*, May, 1858, vol. iii, p. 207).

The same views are insisted upon in a paper "On some Points in Chemical Geology" (Quart. Jour. Geol. Soc. London, Nov. 1859, vol. xv, page 594), and have since been repeatedly put forward by me, with farther explanations as to what I have designated above, *the ruins of the crust of anhydrous and primitive igneous rock.* This, it is conceived, must, by contraction in cooling, have become porous and permeable, for a considerable depth, to the waters afterwards precipitated upon its surface. In this way it was prepared alike for mechanical disintegration, and for the chemical action of the acids, which, as shown in the two papers just referred to, must have been present in the air and the waters of the time. It is, moreover, not improbable that a yet unsolidified sheet of molten matter may then have existed beneath the earth's crust, and may have intervened in the volcanic phenomena of that early period, contributing, by its extravasation, to swell the vast amount of mineral matter then brought within aqueous and atmospheric influences. The earth, air, and water thus made to react upon each other, constitute the first matter from which, by mechanical and chemical transformations, the whole mineral world known to us has been produced.

It is the lower portions of this great disintegrated and water-impregnated mass which form, according to the present hypothesis, the semi-liquid layer supposed to intervene between the outer solid crust and the inner solid and anhydrous nucleus. In order to obtain a correct notion of the condition of this mass, both in earlier and later times, two points must be especially considered, the relation of temperature to depth, and that of solubility to pressure. It being conceded that the increase of temperature in descending in the earth's crust is due to the transmission and escape of heat from the interior, Mr. Hopkins showed mathematically that there exists a constant proportion between the effect of internal heat at the surface and the rate at which the temperature increases in descending. Thus, at the present time, while the mean temperature at the earth's surface is augmented only about one-twentieth of a degree Fahrenheit, by the escape of heat from below, the increase is to be found to be equal to about one degree for each sixty feet in depth. If, however, we go back to a period in the history of our globe when the heat passing upward through its crust was sufficient to raise the superficial temperature twenty times as much as at present, that is to say, one degree of Fahrenheit, the augmentation of heat in descending would be twenty times as great as now, or one degree for each three feet in depth (Geol. Journal, viii, 59.) The conclusion is inevitable that a condition of things must have existed during long periods in the history of the cooling globe, when the accumulation of comparatively thin layers of sediment would have been sufficient to give rise to all the phenomena of metamorphism, vulcanicity, and movements of the crust, whose origin Herschel has so well explained.

Coming, in the next place, to consider the influence of pressure upon the buried materials derived from the mechanical and chemical disintegration of the primitive crust, we find that by the presence of heated water throughout them, they are placed under conditions very unlike those of the original cooling mass. While pressure raises the fusing point of such bodies as expand in passing into the liquid state, it depresses that point for those which, like ice, contract in becoming liquid. The same principle extends to that liquefaction which constitutes solution; where, as is with few exceptions the case, the process is attended with condensation or diminution of volume, pressure will, as shown by the experiments of Sorby, augment the solvent power of the liquid.* Under the influence of the elevated temperature and the great pressure which prevail at considerable depths, sediments should, therefore, by the effect of the water which they contain, acquire a certain degree of liquidity, rendering not improbable the suggestion of Scheerer, that the presence of five or ten per cent of water may suffice, at temperatures approaching redness, to give to a granitic mass a liquidity partaking at once of the character of an igneous and an aqueous fusion. The studies by Mr. Sorby of the cavities in crystals have led him to conclude that the constituents of granitic and trachytic rocks have crystallized in the presence of liquid water, under great pressure, at temperatures not above redness, and consequently very far below that required for simple igneous fusion. The intervention of water in giving liquidity to lavas, has, in fact, long been taught by Scrope, and notwithstanding the opposition of Plutonists, like Durocher, Fournet, and Rivière, is now very generally admitted. In this connection, the reader is referred to the Geological Magazine for February, 1868, page 57, where the history of this question is discussed.

It may here be remarked that if we regard the liquefaction of heated rocks under great pressure, and in presence of water, as a process of solution rather than of fusion, it would follow that diminution of pressure, as supposed by Mr. Scrope, would cause, not liquefaction, but the reverse. The mechanical pressure of great accumulations of sediment is to be regarded as co-operating with heat to augment the solvent action of the water,

* Sorby, Bakerian Lecture, Royal Society, 1863.

and as being thus one of the efficient causes of the liquefaction of deeply buried sedimentary rocks.

[The following extracts from a note by the author to the Geological Magazine for February 1870, may be cited in further elucidation of this point:—"pressure, which in the first case, that of simple fusion of anhydrous materials, prevents liquefaction by preventing expansion, in the second case (that of igneo-aqueous fusion or liquefaction at high temperatures, by the aid of a small portion of water, as maintained by Scrope, Scheerer and Elie de Beaumont) on the contrary, favors liquefaction by promoting the solution of the water-impregnated mass. As Sorby has shown, a conversion of mechanical into chemical force appears in the increase of solubility under pressure. In other words, pressure prevents fusion when, as in most instances, it is a process of expansion, but favors solution, which is, with few exceptions, a process of contraction. Now since I place the seat of volcanic action in a region where solution, rather than simple fusion, is the cause of liquidity, I am led to consider pressure as one of the efficient causes of the liquefaction of rocks, and to regard its diminution as leading to solidification."]*

That the water intervenes not only in the phenomena of volcanic eruptions, but in the crystallization of the minerals of eruptive rocks, which have been formed at temperatures far below that of igneous fusion, is a fact not easily reconciled with either the first or the second hypothesis of volcanic action, but is in perfect accordance with the one here maintained, which is also strongly supported by the study of the chemical composition of igneous rocks. These are generally referred to two great divisions, corresponding to what have been designated the trachytic and pyroxenic types, and to account for their origin, a separation of a liquid igneous mass beneath the earth's crust into two layers of acid and basic silicates, was imagined by Philips, Durocher, and Bunsen. The last mentioned, as is well known, has calculated the normal composition of these supposed trachytic and pyroxenic magmas, and conceives that from them, either separately, or by admixture, the various eruptive rocks are derived; so that the amounts of alumina, lime, magnesia and alkalies, sustain a constant relation to the silica in the rock. If, however, we examine the analyses of the eruptive rocks in Hungary and Armenia, made by Streng, and put forward in support of this view, there will be found such discrepancies between the actual and the calculated results as to throw grave doubts on Bunsen's hypothesis.

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* See in this connection, Mr. Scrope on "The Character of Lavas," in the Geological Magazine for March 1870.

Two things become apparent from a study of the chemical nature of eruptive rocks; first, that their composition presents such variations as are irreconcilable with the simple origin generally assigned to them, and second, that it is similar to that of sedimentary rocks, whose history and origin it is, in most cases, not difficult to trace. I have elsewhere pointed out how the natural operation of mechanical and chemical agencies tends to produce among sediments, a separation into two classes, corresponding to the two great divisions above noticed. From the mode of their accumulation, however, great variations must exist in the composition of the sediments, corresponding to many of the varieties presented by eruptive rocks. The careful study of stratified rocks of aqueous origin discloses, in addition to these, the existence of deposits of basic silicates of peculiar types. Some of these are in great part magnesian, others consist of compounds like anorthite and labradorite, highly aluminous basic silicates, in which lime and soda enter, to the almost complete exclusion of magnesia and other bases; while in the masses of pinite or agalmatolite rock we have a similar aluminous silicate, in which lime and magnesia are wanting, and potash is the predominant alkali. In such sediments as these just enumerated we find the representatives of eruptive rocks like peridotite, phonolite, leucitophyre, and similar rocks, which are so many exceptions in the basic group of Bunsen. As, however, they are represented in the sediments of the earth's crust, their appearance as exotic rocks, consequent upon a softening and extravasation of the more easily liquefiable strata of deeply buried formations, is readily and simply explained.*

The object of the present communication has been to call the attention of geologists to the neglected views of Keferstein and Herschel, which I have endeavored to extend and to adapt to the present state of our knowledge. It is proposed in another paper to consider the question of the agencies which have regulated the geographical distribution of volcanic phenomena both in ancient and in modern times.

Montreal, Canada, March, 1869.

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* See in this connection the Canadian Journal for 1858, p. 203; Quart. Jour. Geol. Society for 1859. p. 494; this Jour., II, xxxvii. 255, xxxviii, 182; also Geology of Canada, 1863, pp. 643, 669, and Rep. Geol. Canada, 1866, p. 230.

ART. IV.—*Notes on some features of the Flora of Eastern Kansas;* by ELIHU HALL.

[The following consists of two articles (somewhat curtailed) published in the *Prairie Farmer*, which give so clear an exposition of the general features of the vegetation of the region under consideration, that we have sought and obtained permission to reproduce them in this Journal.—EDS.]

TREE GROWTH.—In a trip in September last in a wagon through a portion of Eastern Kansas, from Forts Scott and Humboldt north to Leavenworth and Atchison, I had excellent opportunities for observation of its general flora, and more especially so that of the autumnal months. The general character of its arborescent flora is decidedly adverse to a favorable impression of the adaptation of the country to tree growth. The question arises why all these dwarfed, distorted, abnormally developed specimens everywhere, as compared with the true type of trees, such as we are accustomed to in our own noble denizens of the Mississippi Valley forests. True, on the larger streams, the black alluvial deposit grow some very fine specimens of black walnut, cottonwood and elms, but this area is quite limited.

We should naturally infer from this character of the native arborescent growth of the country that causes had been long existing and were yet in operation to produce so general a result; but since the settlement or since the planting of trees has been commenced there by the inhabitants, everywhere living evidences are springing up directly opposing all such inferences. In some parts of Jefferson county, artificial groves only ten years old, are already 50 feet high, and appearing, through the misty haze of autumn, like natural groves on the prairies of central Illinois. There are evidences, too, that the native growth, that is, the young trees, in all the bushy regions, are making good speed in becoming saplings of proper proportions; this is particularly so of the hickory, (*Carya alba* and *C. porcina.*)

These many facts satisfy us that the causes of the abnormal tree growth here are not ascribable to an uncongenial climate, nor to aridity or sterility of soil, nor to exposure, for the flourishing artificial groves in Jefferson county are upon as high land as there is probably in the state, so near the Missouri river. The growth of fruit trees, apple, peach, pear, and the grape vine, during the past season, has been prodigious.

SPECIES AND DISTRIBUTION OF NATIVE TREES.—The Oak Family is represented by *Quercus obtusiloba* everywhere in the bushy regions, principally as dwarfed low trees and bushes.

Q. macrocarpa is less abundant, but occurs throughout, some specimens of fair proportions. *Q. Prinus*, (var. *humilis*,) every where in the bushy regions from one foot to forty feet high, fruiting abundantly; the very smallest bushes. The White Oak (*Q. alba*) was not met with. I doubt if there is a specimen in the State, authors of books on Kansas and travelers to the contrary notwithstanding. It is known to be one of the first oaks that fails westward, and probably does not reach the State at any point.

Quercus coccinea was frequently met with, but is not abundant; all dwarfed specimens.

Quercus rubra is more plentiful, principally young trees.

Quercus palustris, in the southern portion of the State is abundant, the principal tree-growth mostly bushes and young trees, and some large old specimens of fair proportions on the bottoms of the larger streams.

The Hickory Family is represented by *Juglans nigra*, on the streams, but not plentiful; a few large specimens.

Carya alba, *C. sulcata*, *C. porcina*, *C. amara* and *C. olivæformis*, (the Pecan;) the latter abounds abundantly in the southern part of the State, but so far as seen, in bushy specimens or young trees of little promise. *Carya alba* and *C. porcina* are the two species springing up thickly in the bushy regions, and are destined soon to afford an abundance of the best of fuel; are already of size for hoop-poles in many places northward. *Ulmus fulva* and *Ulmus Americana* abound on the streams. These are the two commonest elms everywhere westward; the latter principally prevailing.

Populus monilifera, the common cotton-wood, was frequently met with, but is probably not plenty after leaving the Missouri river. Neither of the Aspens, so common in the regions further north, was seen. The maples are rare trees. *Acer sacharinum* was not seen. A few well grown trees of *Negundo aceroides*, (the box elder). The *Diospyros Virginiana*, (Persimmon,) is an abundant shrub southward, and the Pawpaw (*Asimina triloba*) is scarcely less common in that region.

THE HERBACEOUS FLORA, as would be expected, partakes much of the character of the high plain flora west of the Missouri river; the country being chiefly prairie: the number of species is small, but individuals aggregate immensely. Of the grasses, Andropogon furcatus, A. scoparius and Sorghum nutans, compose probably 80 per cent. These are the chief hay grasses, and probably the most used by grazing stock in the summer; but the winter forage plant, as I learned from the inhabitants, the one upon which their cattle graze and fatten during the winter months, and from which Kansas has gained her reputation as a country where stock needed feeding a few weeks in the year, is *Sporobolus heterolepis*. This grass only abounds plentifully in certain localities; it affects the moister and flatter portions of the prairie, and is most common southward. It is a wide-spread species, however, being plentiful in Nebraska, Iowa and Illinois, and, doubtless, in other localities; more or less abundant all over the west and northwest, extending east also to Connecticut. The species may be easily known by its small contracted panicle and round seeds, and especially by its strong odor when bruised. It is inclined to grow in bunches or stools, and where most abundant it nearly occupies the ground.

The relative proportion of species as they occur in the localities mentioned below, will pretty fairly represent the *Graminea* in those regions.

In one hundred square feet on a high prairie in Jefferson county:—Andropogon furcatus, 80 per cent; Andropogon scoparius, 8 per cent; Sorghum nutans, 6 per cent; Kœleria cristata, 4 per cent; Bouteloua curtipendula, 2 per cent.

In the same area in another locality in Jefferson county: Sorghum nutans, formed 20 per cent; Andropogan scoparius, 60 per cent; Sporobolus heterolepis, 8 per cent; Panicum virgatum, 8 per cent; Panicum pauciflorum, 4 per cent.

Same area in Franklin county, high prairie:—Andropogon furcatus, 6 per cent; Andropogon scoparius, 30 per cent; Sorghum nutans, 45 per cent; Bouteloua curtipendula, 10 per cent; Panicum virgatum, 8 per cent; Panicum dichotomum, 1 per cent.

Same area in Anderson county; a thin soil, high prairie:—Andropogon scoparius, 50 per cent; Andropogon furcatus, 2 per cent; Sorghum nutans, 40 per cent; Koeleria cristata, 7 per cent; Panicum pauciflorum, 1 per cent.

These stations are all on the prairie, where no local conditions determine the character of the vegetation, the soils not affected by alkali or a superabundance of lime, but are good agricultural soils. Other stations are occupied with special species. The strongest alkaline places are almost exclusively occupied with *Vilfa cuspidata* and *V. depauperata*, two low-growing species, with very fine leaves thickly set on the ground; probably nearly worthless for grazing purposes. Another peculiar feature of the country consists of the rocky places, so-called. These are generally found in the vicinity of streams on the ground grading from the high prairies. Their peculiarity consists in being everywhere distinctly lined out and occupied abundantly by a very notable *Helianthus*, (sunflower,) *H. orgyalis*, a species peculiar to that region of the country and southward. These localities do not reach the northern portion of the State. These rocky lands furnish the botanist with his richest treasures. Another very notable plant of the southern and middle portions of Kansas, and peculiar to those regions, is *Amphiachyris dracunculoides.* This is an annual, resembling *Solidago linifolia*, but bearing an abundance of showy, yellow flowers in September. It has spread from its native habitat,—the rocky soils,—

to the road sides and into the fields of the farmers, where it has already become a formidable weed. Imagine a field of flax in full bloom, and you have a picture of many fields of the Kansas farmers, only differing in appearance by the flowers being yellow instead of blue. It has different local names, such as Butter-weed, Tumble-weed, etc. A shopkeeper in Humboldt, to whom I applied for information, rather contemptuously replied, "Curious any body didn't know tumble weed." Let not our readers imagine that the plant is *Amaranthus albus*, or *Cycloloma platyphylla*, that so often tumble over the fences and hedges in Illinois in the autumn months. But the Kansas plant does tumble some, too. When not growing too thickly, it forms a rather bushy, round head.

But probably the most notable plant on the prairies, at this season of the year, the most universally distributed, and the most showy, is *Salvia Pitcheri*, a species of sage, three to five feet high, bearing spikes of copious, rather large, blue flowers. These give coloring to the prairie landscape more or less abundantly (except in the northern counties, where it becomes much rarer) as far as the vision extends on either side, towering above the grasses and most other plants, crowning the whole verdant plain below with a canopy of blue. Another species of Sage (*Salvia trichostemoides*) is common also. This is more of a weed in appearance, and even threatens to become troublesome to the agriculturist.

The general herbaceous flora is largely represented with the *Helianthi*. *Helianthus lenticularis*, a showy species, much resembling the common annual sun-flower, is too abundant, being more or less troublesome as a weed to the farmer. *Helianthus petiolaris*, another annual species, is less common. *H. grosse-serratus*, *H. Maximiliani* on most rich soils. *H. rigidus* everywhere abounds, and *H. mollis* occurs in all the southern portions in immense quantity, but entirely disappears northward. Only *Silphium laciniatum* was seen. The other notablé prairie species of "rosin weed," *Silphium terebinthinaceum* does not probably occur. These plants I find are looked after by land hunters as indications of a good soil. I find the one species here of all heights, from a foot to six feet; the general average is about three feet this wet season, which is much below the ordinary stature of the plant as seen on the prairies of Illinois. Some other Compositæ are largely represented: *Aster multiflorus*, *Solidago rigidus*, *S. linifolius*, and *S. Missouriensis* being exceedingly abundant in specimens. The *Vernonia fascicularis* that gives so much character and notability to the bottom lands and wet prairies of Illinois in the autumn months, is here represented by an allied species, *Vernonia Arkansana*, but in much fewer specimens.

Lespedeza capitata, of the Pea family, is everywhere abundant on the prairies, and cut with the hay often forms too large a portion of tonnage for the interest of the buyer; which also must be said of *Liatris scariosa* and *L. squarrosa*; the former sometimes equal to a fourth or fifth of the gross weight of a stack. Two other species, but less abundant, are represented in the State, *L. punctata* and *L. pycnostachya*. These noble plants empurple the landscape on thousands of acres in August and September. *Gerardia asperifolia* is abundant almost everywhere on the prairie, and contributes a good share to the weedy character of the hay. *Eryngium Leavenworthii* in the extreme south is a showy, handsome plant, and, though a biennial, well deserves a place in ornamental grounds. *Mamillaria vivipara* and a form of *Opuntia Missouriensis* represent the *Cactaceæ*, so far as seen.

WEEDS.—The Kansas farmer has already introduced his quota of foreign weeds; but he also has some formidable natives to combat,—plants that thrive exceedingly with cultivation. *Amphiachyris dracunculoides*, before mentioned; *Croton capitatum*, and, perhaps, worst of all, *Solanum rostratum*, a species that probably combines the bad characters of *Datura Stramonium*, the clot bur and the sand bur. The plant is an annual, and is already easily established, particularly in the neighborhood of Leavenworth City. The common native *Polygonums* in many localities have grown so abundantly and so large in the fields as to make them almost inaccessible to a nervous pedestrian.

Setaria glauca, (the fox tail) was plentifully introduced on almost every farm in some localities, and flourishes exceedingly. Some of the farmers believe it to be native, so unaccountably has it appeared on their farms. But, like purslane, it insidiously marches westward, and often, doubtless, the seeds are abiding their time for the conditions of soil necessary to their development, for years in advance of the agriculturist. But of the introduced plants we sought in vain through all the Middle and Southern counties for *Poa pratensis*, the Kentucky blue grass. Why a plant that is so profitable to agriculture, so tenacious of life, and so encroaching on the neighboring species that few things are able to survive its neighborship (even the hazel with the aid of grazing stock is gradually dying out before it in Illinois, and in most parts, especially of central Illinois, the native grasses have long since yielded to its persistent encroachments),—why this species should be entirely absent was left to us without explanation. The residence grounds in the city of Lawrence, many of them tastefully, elegantly, and expensively improved with buildings, were waving with *Panicum capillare*, varied with species of weeds.

Can it be that all this comes from simple neglect of introduction? In the bushy regions in the neighborhood of Leavenworth and Atchison, some blue grass is met on the roadsides; a few pastures near Leavenworth (looking as if doing well) and the city residence lots are frequently occupied with it; but in no other parts of the State was the plant seen.

PRAIRIE VEGETATION.—The following catalogues represent the entire native flora on the areas and at the localities mentioned. Soon most of these things will have passed away before the restless plowshare and the all-devouring kine. I give them as a natural history record.

One hundred square feet on high prairie in Jefferson county:

Sorghum nutans, 20 per cent.
Andropogon scoparius 60 per cent.
Sporopolus heterolepis 5 " "
Helianthus rigidus 10 per cent.
Euphorbia corollata 3 plants.
Asclepias tuberosa 1 plant.
Petalostemon candidum 3 plants.
Coreopsis palmata 1 plant.
Panicum virgatum 6 plants.
Salvia Pitcheri 1 plant.
Solidago Canadensis 1 plant.
Solidago rigida 1 plant.
Solidago linifolia 1 plant.
Aster multiflorus 10 plants.
Panicum dichotomum 1 plant.
Aster azureus 5 plants.
Liatris scariosa 1 plant.
Liatris squarrosa 1 plant.
Eragrostis capillaris 1 plant.
Lespedeza capitata 1 plant.
Eryngium yuccæfolium 1 plant.
Silphium laciniatum 1 plant.
Phlox pilosa 1 plant.

Same area in Franklin county, high prairie.

Sorghum nutans, 60 per cent.
Andropogon scoparius 20 per cent.
Andropogon furcatus 2 per cent.
Lespedeza capitata 8 plants.
Lactuca elongata 1 plant.
Aster laevis 1 plant.
Aster oblongifolius 3 plants.
Salvia Pitcheri 5 plants.
Liatris scariosa 2 plants.
Bouteloua curtipendula 8 plants.
Panicum virgatum 6 plants.
Erigeron canadense 1 plant.
Kuhnia eupatorioides 1 plant.
Petalostemon violaceum 1 plant.
Rosa blanda 3 plants.
Petalostemon candidum 1 plant.
Silphium laciniatum 2 plants.
Panicum dichotomum 1 plant.
Helianthus rigidus 3 plants.
Euphorbia corollata 1 plant.
Solidago rigida 1 plant.

Same area in Anderson county, high prairie, thin soil.

Andropogon scoparius, 40 per cent.
Andropogon furcatus 1 plant.
Sorghum nutans 30 per cent.
Gerardia asperifolia, 50 plants.
Kœleria cristata, 10 per cent.
Helianthus rigidus 25 plants.
Aster multiflorus 1 plant.
Solidago Missouriensis 2 plants.
Amorpha canescens 1 plant.
Panicum panciflorum 3 plants.
Linum Boottii 10 plants.
Solidago linifolia 4 plants.
Polygala verticillata 3 plants.
Baptisia leucantha 1 plant.
Antenaria dioica 3 plants.
Ruellia ciliosa 1 plant.
Coreopsis tinctoria 1 plant.
Psoralea floribunda 1 plant.
Eryngium yuccæfolium 1 plant.

Same area in Linn county; high prairie.

Andropogon furcatus 80 per cent.
Andropogon scoparius 5 per cent.
Sorghum nutans 8 per cent.
Lespedeza capitata 5 plants.
Solidago Missouriensis 6 plants.
Ceanothus ovalis 1 plant.
Aster azureus 3 plants.
Aster oblongifolius 5 plants.
Solidago linifolia 1 plant.
Salvia Pitcheri 1 plant.
Amorpha canescens 5 plants.
Helianthus rigidus 3 plants.
Liatris squarrosa 8 plants.
Euphorbia corollata 1 plant.
Panicum dichotomum 3 plants.
Kœleria cristata 1 plant.
Oxalis violacea 1 plant.
Echinacea angustifolia 1 plant.
Linum Boottii 1 plant.
Polytænia Nuttallii 1 plant.
Bouteloua curtipendula 1 plant.
Ambrosia pycnostachya 1 plant.
Petalostemon violaceum 1 plant.
Polygala incarnata 1 plant.

ART. V.—*Notice of some Minerals from New Jersey;* by Prof. W. T. RŒPPER, of Bethlehem, Pa.

1. *Iron, Manganese, Zinc, Chrysolite.*

THE Stirling Hill, Sussex county, N. J., which with its neighbor, the Minehill, seems to be an inexhaustible storehouse of interesting minerals, both scientifically and commercially, has furnished an antitype to Prof. Brush's Hortonolite from the adjoining Orange county.

Some years ago I examined a black crystalline massive mineral from this locality, and found it to be a unisilicate of the protoxyds of iron, manganese, zinc and magnesium, and as it showed many of the characteristic physical and chemical properties of chrysolitic minerals, especially that peculiar mottled coloring, which is so marked in olivines, I supposed it a variety of tephroite, that peculiar subspecies of the group having shortly before been rediscovered by Prof. Brush. During a visit to the locality in the course of last year, I succeeded in finding distinct crystals, which at once ranged the mineral unmistakably among the chrysolites.

Crystallization.—The crystals occur in great numbers, grouped together, and of all sizes, from an eighth of an inch to two inches in length and nearly one inch in breadth. They are mostly rounded, owing to an incipient alteration of the surface through meteoric waters, black and dull on the outside, but with lustrous and brilliant cleavages on being broken. Some of them, however, are sharp, and allow of a measurement of angles at least by the hand-goniometer. The dominant forms are: $i\bar{2}$ (angle over $i\breve{i}$, 130°), $i\breve{i}$ ($i\bar{2} \wedge i\breve{i}$, 115°), $1\bar{i}$ (angle at top, 77°). Generally subordinate I have observed the following forms: $i\bar{i}$, $1\text{-}\breve{i}$, $1\text{-}\bar{2}$, O, and a face replacing combination edge $i\text{-}\bar{2} \wedge 1\text{-}\bar{i}$ with parallel intersection-edges, in which therefore $n' = \frac{2m'}{m'-m}$, to which among simple coefficients the forms $2\text{-}\bar{4}$ or $3\text{-}\bar{3}$ would answer. The face is too small and dull to allow of a measurement of angle; $2\text{-}\bar{4}$ appears however the more likely form, as chrysolites seem to have a preference for the ratio 1:2. *Cleavages*, three rectangular; O and $i\text{-}\breve{i}$ very and almost equally eminent, with vitreo-pearly luster approaching the sub-adamantine; $i\text{-}\bar{i}$ splintery. *Hardness*=5·5–6. *Sp. G.*=3·95–4·08. The average of nine determinations with Jolly's spring balance gave 4·023. *Color*, dark green to black but eminently mottled, so that thin splinters or laminæ transmit a pale yellow light. *Streak*, light-yellowish-reddish-gray. The powder is slightly attracted by the magnet. BB. rather refractory, fusing at thin edges to a dull black slag. On charcoal gives a zinc coating, more distinctly on addition of soda. With the fluxes the usual reactions for silica, iron and manganese. The borax and microcosmic beads give in the O. F. the characteristic brownish purple color, indicating mixtures of iron and manganese, which becomes green in the reducing flame. With acids gelatinizes readily and completely. Some specimens leave a bright green undissolved residue, which I judge to be spinel both from its hardness, its not being attacked by fusion with soda, and complete decomposition by bisulphate of soda.

In the following analyses the silica was separated in the usual manner, the filtrate from the silica neutralized by carbonate of soda, then acidified with acetic acid and a current of sulphuretted hydrogen passed through the solution, which separated the zinc as sulphid. This was filtered off, redissolved in HCl, and then precipitated from the uninterruptedly boiling solution by slowly adding $\dot{N}a\ddot{C}$, and the $Zn\ddot{C}$ finally converted into Zn by ignition. The filtrate after the separation of the zinc was then boiled with the addition of $1KO,ClO_5$ to sesquioxydize the iron, the iron precipitated in the usual manner as subacetate, redissolved and reprecipitated by ammonia, the manganese separated by bromine and determined as pyrophosphate with the precautions pointed out by Dr. Gibbs, (this Journal, No. XXXI, p. 216), and lastly the magnesia determined as pyrophosphate. I will here remark, that I did not succeed in separating the oxyd of zinc from the iron by the usual acetate of soda process, but that a great and often the greater part of the Zn went down with the iron, which I attribute to the necessary boiling of a dilute solution (vid. Johnson's Qual. Fresenius). Hence my former analyses were not correct, gave too little zinc and resulted in uni-oxygen ratio only on account of the near proximity of the equivalents of iron, manganese and zinc-oxyds. I may, however, not have handled the method correctly.

The samples No. 1 and 2 were fresh pieces of cleavage crystals carefully examined by the lens so as to avoid all visible admixtures. No. 1 was lighter in color than No. 2, *a* and *b*, which latter are analyses of the same powder. No. 3 are analyses of two different powders of the massive variety.

	1.	2.		3.		Oxygen				
		a	*b*	*a*	*b*	1.	2, *a*.	2, *b*.	3, *a*.	3, *b*.
$\ddot{\mathrm{S}}\mathrm{i}$ ...	30·76	29·90	30·56	30'67	30·42	16·41	15·95	16·30	16·36	16·22
$\dot{\mathrm{F}}\mathrm{e}$ ..	33·78	35·60	35·44	35·37	34·20	7·51	7·91	7·88	7·86	7·60
$\dot{\mathrm{M}}\mathrm{n}$ ..	16·25	16·90	16·93	17·81	17·67	3·66	3·81	3·82	4 01	3·98
$\dot{\mathrm{Z}}\mathrm{n}$ ..	10·96	10·66	10·70	9·87	9·09	2·16	2·11	2·11	1·95	1·80
$\dot{\mathrm{M}}\mathrm{g}$ ..	7·60	5·81	5·44	5·69	6·49	3·04	2·32	2·18	2·28	2·60
Insol.		1·03	1·04	1·39	2·65					
	99·35	99·90	100·11	100·80	100·52	16·37	16·15	15·99	16·10	15·98

The foregoing oxygen ratios make the mineral a unisilicate. The crystallization being orthorhombic with the parametric ratios of the chrysolite group, which is confirmed by the other physical and chemical characters; it is hence an iron-manganese-*zinc* chrysolite, the first, to the best of my knowledge, of the group, into the composition of which zinc enters as a constituent.

It occurs, as before said, on Stirling hill, accompanied by Willemite, Franklinite, Jeffersonite and spinel.

2. *Manganesian Dolomite.*

In the vast vein of Willemite, which is being worked on Minehill by the New Jersey Zinc Company, there occur small masses of a beautiful delicate pink mineral with a rhombohedral cleavage, which by their contrast with the purely apple-green Willemite make exceedingly pretty specimens. An analysis gave the following composition:

$\dot{\mathrm{C}}\mathrm{a}\ddot{\mathrm{C}}$	$\dot{\mathrm{M}}\mathrm{n}\ddot{\mathrm{C}}$	$\dot{\mathrm{F}}\mathrm{e}\ddot{\mathrm{C}}$	$\dot{\mathrm{M}}\mathrm{g}\ddot{\mathrm{C}}$	Insol.	
50·40	43·54	·76	5·69	0·08	= 100·47

Specific gravity = 3·052. Hardness = 4.

The mineral differs from the known dialogites by its greater proportion of carbonate of lime, and may be considered either as a dialogite in which a little more than one-half of the $\dot{\mathrm{M}}\mathrm{n}$ is replaced by lime, or as a dolomite in which about five-sixths of the magnesia is replaced by $\dot{\mathrm{M}}\mathrm{n}$.

3. *A pseudomorph of opal after a micaceous mineral probably some chlorite.*

On Scotch mountain, Warren county, N. J., not far from New Village, among the Laurentian syenitic gneiss formation of that region, there occur, scattered over the ground, numerous masses of a white quartzose mineral apparently of agglutinated rounded granules of about $\frac{1}{8}$ inch diameter. Upon close examination, many of these granules show distinct cleavages, which exhibit a hexagonal outline. Searching the ground carefully I found wormlike contorted crystals, in shape like the similar forms of some chloritic minerals. The substance is distinguished from quartz by its low specific gravity =1·961, and inferior hardness (near 6). It is mostly soluble in caustic potash, leaving only 8 per cent insoluble, which seemed to consist, in part at least, of the original mineral. On ignition it loses 7·27 per cent water. It is therefore manifestly amorphous quartz or opal. Indeed small masses of unquestionable opal of various colors are found in the neighborhood.

It hence appears, that micaceous structure is not, as is frequently assumed, the absolute closing scene of the metamorphism of minerals, but that the replacing power of silica is able to overcome the antimetamorphic energies of minerals even, which have arrived at the micaceous stage.

Bethlehem, April 22, 1870.

ART. VI.—*On the Size of Atoms;* by Prof. Sir W. THOMSON, F.R.S.*

THE idea of an atom has been so constantly associated with incredible assumptions of infinite strength, absolute rigidity, mystical actions at a distance, and indivisibility, that chemists and many other reasonable naturalists of modern times, losing all patience with it, have dismissed it to the realms of metaphysics, and made it smaller than "anything we can conceive." But if atoms are inconceivably small, why are not all chemical actions infinitely swift? Chemistry is powerless to deal with this question, and many others of paramount importance, if barred by the hardness of its fundamental assumptions, from contemplating the atom as a real portion of matter occupying a finite space, and forming a not immeasurably small constituent of any palpable body.

More than thirty years ago naturalists were scared by a wild proposition of Cauchy's, that the familiar prismatic colors proved the "sphere of sensible molecular action" in transparent liquids and solids to be comparable with the wave-length of light. The thirty years which have intervened have only confirmed that proposition. They have produced a large number of capable judges; and it is only incapacity to judge in dynamical questions that can admit a doubt of the substantial correctness of Cauchy's conclusion. But the "sphere of molecular action" conveys no very clear idea to the non-mathematical mind. The idea which it conveys to the mathematical mind is, in my opinion, irredeemably false. For I have no faith whatever in attractions and repulsions acting at a distance between centers of force according to various laws. What Cauchy's mathematics really proves is this: that in palpably homogeneous bodies, such as glass or water, contiguous portions are not similar when their dimensions are moderately small fractions of the wave-length. Thus in water, contiguous cubes, each of one one-thousandth of a centimeter breadth, are sensibly similar. But contiguous cubes of one ten-millionth of a centimeter must be very sensibly different. So in a solid mass of brickwork, two adjacent lengths of 20,000 centimeters each, may contain, one of them nine hundred and ninety-nine bricks and two half bricks, and the other one thousand bricks: thus two contiguous cubes of 20,000 centimeters breadth may be considered as sensibly similar. But two adjacent lengths of forty centimeters each might contain, one of them one brick and two half bricks, and the other two whole bricks; and contiguous cubes of forty centimeters would be very sensibly dissimilar. In short, optical dynamics leaves no alternative but to admit that the diameter of a molecule, or the distance from the center of a molecule to the center of a contiguous molecule in glass, water, or any other of our transparent liquids and solids, exceeds a ten-thousandth of the wave-length, or a two-hundred-millionth of a centimeter.

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* From Nature, No. 22, March 31.

By experiments on the contact electricity of metals made eight or ten years ago, and described in a letter to Dr. Joule, which was published in the Proceedings of the Literary and Philosophical Society of Manchester, I found that plates of zinc and copper connected with one another by a fine wire attract one another, as would similar pieces of one metal connected with the two plates of a galvanic element, having about three-quarters of the electro-motive force of a Daniel's element.

Measurements published in the Proceedings of the Royal Society for 1860 showed that the attraction between parallel plates of one metal held at a distance apart small in comparison with their diameters, and kept connected with such a galvanic element, would experience an attraction amounting to two ten-thousand-millionths of a gram weight per area of the opposed surfaces equal to the square of the distance between them. Let a plate of zinc and a plate of copper, each a centimeter square and a hundred-thousandth of a centimeter thick, be placed with a corner of each touching a metal globe of a hundred-thousandth of a centimeter diameter. Let the plates, kept thus in metallic communication with one another be at first wide apart, except at the corners touching the little globe, and let them then be gradually turned round till they are parallel and at a distance of a hundred-thousandth of a centimeter asunder. In this position they will attract one another with a force equal in all to two grams weight. By abstract dynamics and the theory of energy, it is readily proved that the work done by the changing force of attraction during the motion by which we have supposed this position to be reached, is equal to that of a constant force of two grams weight acting through a space of a hundred-thousandth of a centimeter; that is to say, to two hundred-thousandths of a centimeter-gram. Now let a second plate of zinc be brought by a similar process to the other side of the plate of copper; a second plate of copper to the remote side of this second plate of zinc, and so on till a pile is formed consisting of 50,001 plates of zinc and 50,000 plates of copper, separated by 100,000 spaces, each plate and each space one hundred-thousandth of a centimeter thick. The whole work done by electric attraction in the formation of this pile is two centimeter-grams.

The whole mass of metal is eight grams. Hence the amount of work is a quarter of a centimeter-gram per gram of metal. Now 4,030 centimeter-grams of work, accord-ing to Joule's dynamical equivalent of heat, is the amount required to warm a gram of zinc or copper by one degree centigrade. Hence the work done by the electric attraction could warm the substance by only $\frac{1}{16120}$ of a degree. But now let the thickness of each piece of metal and of each intervening space be a hundred-millionth of a centimeter instead of a hundred-thousandth. The work would be increased a million-fold unless a hundred-millionth of a centimeter approaches the smallness of a molecule. The heat equivalent would therefore be enough to raise the temperature of material by 62°. This is barely, if at all, admissible, according to our present knowledge, or, rather, want of knowledge, regarding the heat of combination of zinc and copper. But suppose the metal plates and intervening spaces to be made yet four times thinner, that is to say, the thickness of each to be four-hundred-millionth of a centimeter. The work and its heat equivalent will be increased sixteen-fold. It would therefore be 990 times as much as that required to warm the mass by 10 cent, which is very much more than can possibly be produced by zinc and copper in entering into molecular combination. Were there in reality anything like so much heat of combination as this, a mixture of zinc and copper powders would, if melted in any one spot, run together, generating more than heat enough to melt each throughout; just as a large quantity of gunpowder if ignited in any one spot burns throughout without fresh application of heat. Hence plates of zinc and copper of a three-hundred-millionth of a centimeter thick, placed close together alternately, form a near approximation to a chemical combination, if indeed such thin plates could be made without splitting atoms.

The theory of capillary attraction shows that when a bubble —a soap-bubble for instance—is blown larger and larger, work is done by the stretching of a film which resists extension as if it were an elastic membrane with a constant contractile force. This contractile force is to be reckoned as a certain number of units of force per unit of breadth. Observations of the ascent of water in capillary tubes shows that the contractile force of a thin film of water is about sixteen milligrams weight per millimeter of breadth. Hence the work done in stretching a water film to any degree of thinness, reckoned in millimeter-milligrams, is equal to sixteen times the number of square millimeters by which the area is augmented, provided the film is not made so thin that there is any sensible diminution of its contractile force. In an article "On the Thermal effect of drawing out a Film of Liquid," published in the Proceedings of the Royal Society for April, 1858, I have proved from the second law of thermodynamics that about half as much more energy, in the shape of heat, must be given to the film to prevent it from sinking in temperature while it is being drawn out. Hence the intrinsic energy of a mass of water in the shape of a film kept at constant temperature increases by twenty-four milligram-millimeters for every square millimeter added to its area.

Suppose then a film to be given with a thickness of a millimeter, and suppose its area to be augmented ten thousand and one fold: the work done per square millimeter of the original film, that is to say per milligram of the mass, would be 240,000 millimeter-milligrams. The heat equivalent of this is more than half a degree centigrade of elevation of temperature of the substance. The thickness to which the film is reduced on this supposition is very approximately a ten-thousandth of a millimeter. The commonest observation on the soap-bubble (which in contractile force differs no doubt very little from pure water) shows that there is no sensible diminution of contractile force by reduction of the thickness to the ten-thousandth of a millimeter; inasmuch as the thickness which gives the first maximum brightness round the black spot seen where the bubble is thinnest, is only about an eight-thousandth of a millimeter.

The very moderate amount of work shown in the preceding estimates is quite consistent with this deduction. But suppose now the film to be further stretched, until its thickness is reduced to a twenty-millionth of a millimeter. The work spent in doing this is two thousand times more than that which we have just calculated. The heat equivalent is 1,130 times the quantity required to raise the temperature of the liquid by one degree centigrade. This is far more than we can admit as a possible amount of work done in the extension of a liquid film. A smaller amount of work spent on the liquid would convert it into vapor at ordinary atmospheric pressure. The conclusion is unavoidable, that a water-film falls off greatly in its contractile force before it is reduced to a thickness of a twenty-millionth of a millimeter. It is scarcely possible, upon any conceivable molecular theory, that there can be any considerable falling off in the contractile force as long as there are several molecules in the thickness. It is therefore probable that there are not several molecules in a thickness of a twenty-millionth of a millimeter of water.

The kinetic theory of gases suggested a hunded years ago by Daniel Bernouilli has, during the last quarter of a century, been worked out by Herapath, Joule, Clausius, and Maxwell, to so great perfection that we now find in it satisfactory explanations of all non-chemical properties of gases. However difficult it may be even to imagine what kind of thing the molecule is, we may regard it as an established truth of science that a gas consists of moving molecules disturbed from rectilineal paths and constant velocities by collisions or mutual influences, so rare that the mean length of proximately rectilineal portions of the path of each molecule is many times greater than the average distance from the center of each molecule to the center of the molecule nearest it at any time. If, for a moment, we suppose the molecules to be hard elastic globes all of one size, influencing one another only through actual contact, we have for each molecule simply a zigzag path composed of rectilineal portions, with abrupt changes of direction. On this supposition Clausius proves, by a simple application of the calculus of probabilities, that the average length of the free path of a particle from collision to collision bears to the diameter of each globe, the ratio of the whole space in which the globes move, to eight times the sum of the volumes of the globes. It follows that the number of the globes in unit volume is equal to the square of this ratio divided by the volume of a sphere whose radius is equal to that average length of free path. But we cannot believe that the individual molecules of gases in general, or even of any one gas, are hard elastic globes. Any two of the moving particles or molecules must act upon one another somehow, so that when they pass very near one another they shall produce considerable deflexion of the path and change in the velocity of each. This mutual action (called force) is different at different distances, and must vary, according to variations of the distance, so as to fulfil some definite law. If the particles were hard elastic globes acting upon one another only by contact, the law of force would be —zero force when the distance from center to center exceeds the sum of the radii, and infinite repulsion for any distance less than the sum of the radii. This hypothesis, with its "hard and fast" demarcation between no force and infinite force, seems to require mitigation. Without entering on the theory of vortex atoms at present, I may at least say that soft elastic solids, not necessarily globular, are more promising than infinitely hard elastic globes. And, happily, we are not left merely to our fancy as to what we are to accept for probable in respect to the law of force. If the particles were hard elastic globes, the average time from collision to collision would be inversely as the average velocity of the particles. But Maxwell's experiments on the variation of the viscosities of gases with change of temperature prove that the mean time from collision to collision is independent of the velocity, if we give the name collision to those mutual actions only which produce something more than a certain specified degree of deflection of the line of motion. This law could be fulfilled by soft elastic particles (globular or not globular); but, as we have seen, not by hard elastic globes. Such details, however, are beyond the scope of our present argument. What we want now is rough approximations to absolute values, whether of time or space or mass—not delicate differential results. By Joule, Maxwell, and Clausius we know that the average velocity of the molecules of oxygen or nitrogen or common air, at ordinary atmospheric temperature and pressure, is about 50,000 centimeters per second, and the average time from collision to collision a five-thousand-millionth of a second. Hence the average length of path of each molecule between collisions is about $\frac{1}{100000}$ of a centimeter. Now, having left the idea of hard globes, according to which the dimensions of a molecule and the distinction between collision and no collision are perfectly sharp, something of apparent circumlocution must take the place of these simple terms.

First, it is to be remarked that two molecules in collision will exercise a mutual repulsion in virtue of which the distance between their centers, after being diminished to a minimum, will begin to increase as the molecules leave one another. This minimum distance would be equal to the sum of the radii, if the molecules were infinitely hard elastic spheres; but in reality we must suppose it to be very different in different collisions. Considering only the case of equal molecules, we might, then, define the radius of a molecule as half the average shortest distance reached in a vast number of collisions. The definition I adopt for the present is not precisely this, but is chosen so as to make as simple as possible the statement I have to make of a combination of the results of Clausius and Maxwell. Having defined the radius of a gaseous molecule, I call the double of the radius the diameter; and the volume of a globe of the same radius or diameter I call the volume of the molecule.

The experiments of Cagniard de la Tour, Faraday, Regnault, and Andrews, on the condensation of gases do not allow us to believe that any of the ordinary gases could be made forty thousand times denser than at ordinary atmospheric pressure and temperature, without reducing the whole volume to something less than the sum of the volume of the gaseous molecules, as now defined. Hence, according to the grand theorem of Clausius quoted above, the average length of path from collision to collision cannot be more than five thousand times the diameter of the gaseous molecule; and the number of molecules in unit of volume cannot exceed 25,000,000 divided by the volume of a globe whose radius is that average length of path. Taking now the preceding estimate, $\frac{1}{100000}$ of a centimeter, for the average length of path from collison to collision, we conclude that the diameter of the gaseous molecule cannot be less than $\frac{1}{500000000}$ of a centimeter; nor the number of molecules in a cubic centimeter of the gas (at ordinary density) greater than 6×10^{21} (or six thousand million million million).

The densities of known liquids and solids are from five hundred to sixteen thousand times that of atmospheric air at ordinary pressure and temperature; and, therefore, the number of molecules in a cubic centimeter may be from 3×10^{24} to 10^{26} (that is, from three million million million million to a hundred million million million million). From this (if we assume for a moment a cubic arrangement of molecules), the distance from center to nearest center in solids and liquids may be estimated at from $\frac{1}{140000000}$ to $\frac{1}{400000000}$ of a centimeter.

The four lines of argument which I have now indicated, lead all to substantially the same estimate of the dimensions of molecular structure. Jointly they establish with what we cannot but regard as a very high degree of probability the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centers of contiguous molecules is less than the hundred-millionth, and greater than the two thousand-millionth of a centimeter.

To form some conception of the degree of coarse-grainedness indicated by this conclusion, imagine a rain drop, or a globe of glass as large as a pea, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be coarser grained than a heap of small shot, but probably less coarse grained than a heap of cricket-balls.

ART. VII.—*Miscellaneous Optical Notices*; by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.

§ 1.

On the measurement of wave lengths by means of indices of refraction.[*]

IN a brief notice[†] communicated to the British Association for the Advancement of Science at its meeting in 1849, Prof. Stokes has given a method for measuring wave lengths, which depends upon the fact that, in substances of medium refractive power, the increment of the index of refraction in passing from one point of the spectrum to another is nearly proportional to the increment of the square of the reciprocal of the wave length. The author showed that even when the intervals were taken much longer than necessary, the error in the wave length was usually only in the eighth place of decimals. At the date of the publication of this notice the subject of wave lengths possessed but little interest. The recent development of the spectral analysis of light has given a new impulse to this branch of optics, and has rendered necessary the construction of a normal map of the entire solar spectrum. This has been most successfully accomplished by Angström,[‡] but an attentive study of his work, as well as of the elaborate researches of Van der Willigen[§] and Ditscheiner,[‖] will show that new measurements will be far from superfluous. The imperfections even of the best ruled glasses are so great that it may be reasonably doubted whether the wave lengths of very fine lines can be satisfactorily measured directly. Methods of determining such wave lengths, depending upon the comparison of the refraction and diffraction spectra, have been given by myself[¶] and by Thalen.[**] As it seems at least desirable to multiply such methods, I will here give first a discussion of the method of Stokes in its original form, and afterward a simplification of that method which will also have its uses.

If Cauchy's formula for dispersion, $n=a+\frac{b}{\lambda^2}+\frac{c}{\lambda^4}$, be reduced to its two first terms, and if we then eliminate the constants *a* and *b* from three equations of the form

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[*] Read before the National Academy of Sciences, April 12th, 1870.

[†] Report of the British Association for the Advancement of Science for 1849. Notices and abstracts, p. 10.

[‡] Recherches sur le Spectre Solaire. Berlin, 1869.

[§] Archives du Musée Teyler, vol. i, p. 1.

[‖] Sitzungsberichte der k. k. Akad. der Wissenschaften Bd. l, 1864.

[¶] This Journal, xlvii, March, 1869.

[**] Mémoire sur la determination des longueurs d' onde des raies métalliques, 1868.

$$n=a+\frac{b}{\lambda^2}.$$

we shall obtain the three following equations, involving only wave lengths and indices of refraction:

$$\lambda_1^2=\frac{(n_3-n_2)}{(n_3-n_1)\frac{1}{\lambda_2^2}+(n_2-n_1)\frac{1}{\lambda_3^2}} \qquad (1)$$

$$\lambda_2^2=\frac{(n_3-n_1)}{(n_2-n_1)\frac{1}{\lambda_3^2}+(n_3-n_2)\frac{1}{\lambda_1^2}} \qquad (2)$$

$$\lambda_3^2=\frac{(n_2-n_1)}{(n_3-n_1)\frac{1}{\lambda_2^2}+(n_3-n_2)\frac{1}{\lambda_1^2}} \qquad (3)$$

Of these equations (1) and (3) serve for extrapolation and (2) for interpolation. To test the degree of accuracy attainable in determining wave lengths by these formulas, I have selected the measurements made by Van der Willigen.* The indices of refraction determined by the Dutch physicist are in fact the only indices which are at once sufficiently exact and sufficiently numerous. In addition they have the great advantage of having been made with reference to lines in the solar spectrum the wave lengths of which had been measured by the same observer. There can therefore be no question of identity. As a first example of the method, I give a determination of the wave length of C, taking B as one of the lines exterior to C, and taking in succession 7 other exterior lines more refrangible than C, to combine with B. Formula (2) was therefore employed, and with the following data and results:—

B	1·61079	687·48		
C	1·61252	656·56		
D	1·61436	628·11	656·70	+0·14
11	1·61537	613·96	656·71	+0·15
13	1·61560	610·52	656·56	0·00
14	1·61728	589·56	656·71	+0·15
16	1·61978	561·80	656·76	+0·20
17	1·62064	553·19	656·79	+0·23
19	1·62143	545·83	656·87	+0·31
		Mean of the errors		+0·17

In this table the first column gives the designation or number of the lines, the second its index of refraction, as determined by a Steinheil prism of 60°, the third the corresponding wave length, according to Van der Willigen, and the fourth the wave length as found by formula (2) by combining B with each line after C in succession.

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* Archives du Musée Teyler, vol. i, p. 70.

The mean of the seven values of the wave length of C thus found is 656·70, which is in excess of Van der Willigen's own determination of the value of C by 0·14. From this it appears that the method may be applied with a tolerable degree of approximation, even in the case of a flint glass prism of high dispersive power, and for indices of refraction which refer to lines at considerable angular distances from each other. The increase in the computed values of C, as the intervals between B and the second line of comparison are increased, will however clearly appear from the table. The following results were obtained with the indices of a second Steinheil prism, No. 2, of 46° 52′ 25″·8, also of flint glass.

B	1·60521	B and 8*a*	656·21	—0·35
C	1·60694	B and 11	656·33	—0·23
8*a*	1·60872	B and 13	656·31	—0·25
11	1·60973	B and 16	656·38	—0·18
13	1·60998	B and 17	656·47	—0.09
16	1·61408			
17	1·61495			

The mean of which is 656·28, the error being −0·28. To determine to what extent the method applies, when flint glass prisms are used, and the indices are selected from the more refrangible portion of the spectrum, the following data were assumed:—

F	1·62917	486·39	F and G	438·88	+0.30
35	1·63244	467·00	F and 39	438·76	+0·18
37	1·63828	438·58	F and 38	438·82	+0·24
38	1·63931	434·28	G and 35	438·76	+0·18
39	1·63965	432·74	35 and 38	438·75	+0·17
G	1·64006	431·12	35 and 39	438·67	+0.09

In this table, line 37 is taken as the middle line in applying formula (2), and the results obtained by combining the other lines in pairs are given in columns 4, 5 and 6. It will be seen that, as in the case of the less refrangible portion of the spectrum, the results obtained are with this prism always too high. For the purpose of comparison, I have computed the same wave length from the indices of refraction of the second prism. The data and results are as follows:—

F	1·62332	486·39	F and G	439·07	+0·49
35	1·62657	467·00	F and 39	438·89	+0·31
37	1·63221	438·58	F and 38	438·92	+0·34
38	1·63324	434·28	G and 35	439·00	+0·42
39	1·63358	432·74	35 and 38	438·89	+0·31
G	1·63400	431·12	35 and 39	438·84	+0·26

In the case of the first prism the mean of the errors is +0·21, while for the second the mean of the errors is +0·35. From this it appears that in the more refrangible portion of the spectrum the errors are considerably greater than in the less refrangible portion, even for equal differences of wave length, and further, that the advantage in precision is with the prism having the higher dispersive power. As the probable errors of the measurements of the indices of refraction are not given, it is impossible to determine to what extent the errors in the computed wave lengths are due solely to want of precision in the indices. It is also to be remarked that, while with the second prism the errors in the less refrangible portion of the spectrum are affected with the sign −, in the more refrangible portion they are largely positive. The close agreement in the value of the wave length of 37, as found by Van der Willigen, with the values as found by Ditscheiner and Angström—438·27 and 438·28—proves that the source of error is not an erroneous determination of this quantity. It seems therefore certain that the nearly constant errors noted above are due in part to the fact that the indices of refraction are determined only to five places of decimals, and in part to the high dispersive powers of the prisms employed, which would render it necessary to employ more than two terms in Cauchy's formula to obtain a closer approximation. As the formulas for interpolation would in this way be rendered extremely complicated, it is better, in the case of any series of observations embracing a particular part of the scale, simply to determine the mean of the errors, and to apply this mean with its proper sign to the computed values of the particular wave length to be determined by the measurement of indices of refraction. If we apply such a correction in the cases of the four series of data and results given above, we find for the corrected values of the wave-lengths the following numerical results:—

(1)	(2)	(3)	(4)
656·53	656·49	438·67	438·72
656·54	656·61	438·55	438·54
656·38	656·59	438·61	438·57
656·54	656·66	438·55	438·65
656·59	656·75	438·54	438·54
656·62		438·44	438·49

The true values being respectively 656·56 and 438·56. These results are, I think, sufficient to show that a valuable control for the accuracy of measurements of wave lengths may be obtained even when prisms of high dispersive power are employed, provided that the intervals taken are not too large. It seems at least probable that a greater degree of precision is attainable in measuring indices in the case of substances of high than in those of low dispersive power, partly because the angular deviations to be measured are larger, and partly because the spectral lines are less crowded together.

The following example will serve to illustrate the advantage of taking shorter intervals:—

Lines.	λ	Indices.	λ	Indices.	λ
25b	518·63	1·62459	-----	1·61882	-----
26	517·51	1·62472	517.61	1·61895	517·56
27β	517·07	1·62479	-----	1·61901	-----

The data are here also taken from Van der Willigen's measures with the same prisms.

When the angular distances between three spectral lines are not too great, the angular deviation of the lines may, as I find, be substituted for the indices of refraction in formulas (1), (2) and (3). The differences between the angular deviations are of course to be converted into seconds. The following results will show the degree of accuracy attainable by this method, the data being taken from Ditscheiner's* measurements of the indices of a flint prism by Steinheil of refracting angle 60° 4′ 59″.

	Kirchhoff's scale.	λ	Angular deviations.	λ	Indices.	λ
B	593	687·06	47° 40′ 55″	-----	1·61358	-----
C	694	655·95	47° 51′ 19″	655·97	1·61537	655.82
--	877	613·57	48° 8′ 8″	-----	1·61824	-----

From this it appears that the error in the determination of the wave-length of the middle line C is only +0·02 when the angular deviations are employed, but amounts to −0·13 when the indices of refraction are taken as the elements of the calculation. Yet the interval between B and 877 is very large.

The following data are taken from another part of the scale, the measurements being made with the same prism:—

	Kirchhoff's scale.	λ	Angular deviations.	λ	Indices.	λ
b	1648·8	517·13	49° 4′ 16″	-----	1·62775	-----
	1655·6	516·58	49° 4′ 44″	516·56	1·62782	516·61
	1693·8	514·08	49° 6′ 47″	-----	1·62817	-----

Hence the error in the determination of the wave length of 1655·6 is, when the angular deviations are taken, only −0·02 and when the indices are taken +0·03. It must be borne in mind that in all the above mentioned examples the angles are those of minimum deviation. As the numbers upon Kirchhoff's scale also represent angular though not *minimum* deviations, it seemed worth while to determine how far for a short interval, these could be employed. Taking the three scale numbers of the last example, the error in the wave-length of

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* Bestimmung der Wellenlängen der Fraunhoferschen Linien des Sonnenspectrums, p. 43.

1655·6 was found to be − 0·38, and when the scale numbers were taken as the sines or tangents of corresponding angles, +0·09.

The following data are taken from the more refrangible part of the spectrum, the measurements being also those of Ditscheiner, and made with the same prism:—

	2822·8	433.34	50° 34′ 57″	-----	1·64287	-----
G	2854·7	430·88	50° 37′ 52″	430·68	1·64334	430·83
	2969·7	429·90	50° 38′ 47″	-----	1·64352	-----

In this case the error in the wave-length of the middle line, 2854·7, is −0·20, as determined from the angular deviations, and −0·05, as determined from the indices. It must be borne in mind that, in this part of the spectrum, the determination both of wave lengths and of indices of refraction is difficult on account of the feeble intensity of the light.

Since only the differences between the angular deviations of the spectral lines are employed in the formulas above given, it follows that in determining wave lengths by the method in question, it is not necessary to employ a spectrometer with a divided circle and appliances for the measurement of large angles. A common spectroscope will be sufficient, if the observing telescope be provided with a filar micrometer by means of which the angular distances of any given line from two other lines of which the wave lengths are known may be measured. The researches of Angström leave nothing to be desired as regards the wave-lengths of standard lines, and the method given may prove a convenient means of determining with all requisite precision the wave lengths of metallic lines.

§ 2.

On liquids of high dispersive power.—Of the liquids which have hitherto been proposed for the construction of prisms, bisulphid of carbon unquestionably presents the greatest advantages. It is cheap, colorless, and unites a moderately high mean refractive to a very high dispersive power. By tacit consent a prism of 60° filled with this liquid has come to be adopted as a sort of standard. The disadvantages of the bisulphid are equally well known, and I have spent no little time and labor in the endeavor to find a liquid with a still higher dispersive power, less volatile, less sensitive optically to changes of temperature, and less offensive in odor. In these efforts I have not been altogether successful, no one liquid examined possessing all the qualities desired. Many organic liquids with high dispersive powers are difficult to prepare in a state of purity and speedily become discolored by absorption of oxygen from the air. Such are oil of cassia, the colorless oil obtainable from balsam of

Peru and others. The thallic alcohol of Lamy[*] is far too costly. The solution of silico-tungstate of sodium,[†] of metatungstate of sodium [‡] and of soluble tungstic acid [§] as obtained by dialysis, all promised good results from their extraordinary densities, but all proved difficult to prepare in a state of purity and extremely easy of decomposition.

A solution of phosphorus in bisulphid of carbon has, according to Messrs. Dale and Gladstone,[‖] a dispersion of 0·225,[¶] or nearly one and a half times as great as bisulphid of carbon alone, but becomes turbid on exposure to sunlight from the formation of amorphous phosphorus. It occurred to me, that, by dissolving sulphur with the phosphorus, the formation of amorphous phosphorus might be prevented, and experiment proved that this was the case. The solution, as thus obtained, has a pale yellow color, but is perfectly clear and undergoes no change by the action of light even when long continued. I have been in the habit of preparing it by dissolving one part of dry flowers of sulphur and two parts of phosphorus, in four or five parts of bisulphid of carbon, and filtering the liquid through a well dried ribbed paper filter, which is easily done. The refractive and dispersive power of the solution will of course vary with the quantity of phosphorus and sulphur dissolved. By a gentle heat the whole, or nearly the whole, of the bisulphid of carbon may be driven off, a liquid compound of sulphur and phosphorus remaining which has so high a mean refractive power that it cannot be employed with prisms having a refractive angle of more than 45°–50°. The same end may, however, also be attained by continually adding phosphorus to a saturated solution of sulphur in bisulphid of carbon, in which phosphorus appears to be soluble without limit.

With a strong and probably saturated solution of sulphur in CS_2 the angle between Li and D was 0° 50′ 10″. When phosphorus was added the angle was 2° 25′ 30″, the refracting angle of the prism being 60°. In this last case the angle between Na_1 and Na_2 was 0° 2′ 20″. The spectrum was perfectly clear, the definition of the dark lines leaving nothing to be desired. In consequence, however, of the yellow color of the liquid there is always a marked absorption of the violet end of the spectrum.

In working with the above described solution I have employed hollow glass prisms with refracting plates cemented on with a mixture of glue and molasses. These were found to be perfectly tight and to last for months without change. The great disadvantage in the use of a solution of sulphur and phosphorus consists in the danger of breaking the prisms; the liquid taking fire spontaneously when it has been a few seconds in contact with any porous material like wood or paper. On the other hand, however, the large quantity of sulphur present prevents the fire from spreading, a drop placed upon a piece of wood leaving after combustion only a charred spot. When not in use the prisms should be kept in an iron pot with a tight cover. In this manner I have employed and preserved two during a long and hot summer. The viscid, or rather oily, nature of the solution serves to prevent, to a great extent, the formation of ascending and descending currents from slight changes of temperature, and when the prisms are well shaken before use the definition remains perfect for a long time. In my spectroscope the prisms rest upon a plate of glass instead of upon one of metal.

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[*] Ann. de Chimie et de Physique, 4th series, vol. iii, p. 373.

[†] Ann. de Chimie et de Physique, 4th series, vol. iii, p. 5.

[‡] Scheibler in Journal für prakt. Chimie, lxxxiii, p 273.

[§] Graham in Journal of the Chemical Society, vol. ii, p. 318

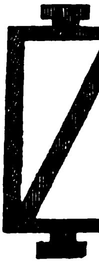
[‖] L. and E. Phil. Mag., vol. xviii, p. 30.

[¶] The number 0·225 is the difference between the indices for the extreme red and violet rays.

§ 3.

On an advantageous form of apparatus for the study of the absorption of light in colored liquids.

In his examination of the spectra of colored fluids, Mr. Gladstone employed a hollow wedge of glass, the two refracting surfaces of which made with each other an acute angle. The wedge was filled with the liquid to be studied and so placed that the refracting edge of the analyzing prism was at right angles to the line of intersection of the two faces of the wedge. In this manner a beam of light was obtained which represented different thicknesses of the absorbing liquid, and the resulting spectrum became a complete absorption diagram. In using this apparatus I found the angular deviation produced by the wedge a source of considerable inconvenience. In addition it is easy to see that the wedge itself produces a certain amount of chromatic dispersion. To remedy these defects and at the same time retain the advantages of the method, I have devised what may be termed a double wedge. Two hollow wedges, of glass or metal, are placed together in such a manner that the first and last surfaces of the bounding plates of glass are parallel. The two wedges are separated by a single plate of glass with parallel surfaces. The base of each wedge, or acute-angled prism, is bored for the insertion of a cork or stopper. The construction of the apparatus will be readily understood from the diagram. In using it with a colored aqueous solution one of the hollow prisms, or wedges, is to be filled with distilled water, the other with the aqueous solution to be examined. The incident beam of sunlight is then allowed to fall perpendicularly upon



either surface, and the slit of the spectroscope is placed so as to be perpendicular to the two lines of intersection of the three refracting plates of the double wedge. In this manner a complete absorption diagram is obtained by the various thicknesses of the liquid examined. For substances soluble in alcohol, ether, &c., one of the hollow wedges must be filled with the colorless solvent, whatever be its nature. By this means all angular deviation and prismatic dispersion are avoided, as the coloring matter does not sensibly change the refractive power of the liquid in which it is dissolved, and the incident beam passes through without change in direction. In my apparatus the wedges have acute angles of about 15°. This I find to be quite sufficient for most purposes, as it is easy to increase or diminish the quantity of substance dissolved. When it is wished to examine the absorption produced by a definite thickness of liquid or by different thicknesses in succession, the double wedge is to be so placed that the slit of the spectroscope shall be parallel to the lines of intersection of the faces of the wedges. By moving the wedge to one side or the other all thicknesses of liquid, from 0 to the maximum, obtainable with the apparatus used, may be successively examined.

§ 4.

On tests for the perfection and parallelism of plane surfaces of glass.

When a plano-convex lens of long radius of curvature is placed upon a plane surface of glass and the system is illuminated by an obliquely incident beam of monochromatic light as, for example, by a sodium flame, the well known phenomenon of Newton's rings is observed with remarkable distinctness and perfection of definition. The symmetry of the rings will depend, in part, on the perfection of figure of the lens, in part on that of the plane surface. An extremely minute deviation from a perfect plane will produce a marked distortion of the circular figure of the ring nearest the center. That this distortion is, or is not due to the lens may be determined by rotating the lens round its optical axis normal to the plane. No change of figure will be seen if the lens is perfect in form and the inequality is in the plane surface only. Different parts of the plane surface may of course be tested in succession, by moving the lens from point to point, and if necessary the rings may be observed with a telescope.

Prof. Rood, of New York, has suggested for the observation of Newton's rings a method which permits of the employment of a lens of comparatively small radius of curvature and a microscope. In his arrangement the lens and plate of glass are placed upon the stage of the microscope, the light from beneath being cut off, and monochromatic light is then thrown down upon the system by means of a plate of glass with parallel surfaces inclined to the axis of the microscope at a convenient angle and placed between the objective and the plano-convex lens. In this manner the rings are seen with great distinctness and beauty, and the arrangement is particularly compact and convenient.

The interference bands of Talbot afford a method not merely of observing with great precision the inequalities of surface and want of parallelism of the faces of plates of glass, but also of photographing these defects and obtaining a permanent chart of the glass which may be of material assistance in correcting its figure. It is only necessary for this purpose to place the glass to be examined near to the object glass of the collimator and perpendicular to its axis, so as to intercept that half of the bundle of parallel rays which falls upon the first surface of the first prism nearest its refracting edge. If the plate has perfectly plane and parallel surfaces the interference bands will be sharply defined and parallel in the whole field of view. The slightest inequality of surface, or inclination of the faces, will produce curvature or distortion of the bands, and, if the eyepiece of the observing telescope be removed, the image may be received on a sensitive plate and photographed. The number of prisms to be employed in a particular case will depend upon the thickness of the plate of glass examined and, in general terms, upon its dispersive power. For a piece of French plate glass four millimeters in thickness, two bisulphid of carbon prisms of 60° must be used to produce a sufficient separation of the interference bands to enable them to be seen distinctly. More prisms must be used for thicker plates and in this way a limit is soon reached at which the method ceases to be applicable.

Cambridge, May 1st, 1870.

ART. VIII.—*On the Occurrence of a Peat bed beneath Deposits of Drift in Southwestern Ohio.*

A BED of peat has lately been found one mile east of Germantown, Montgomery county, Ohio, and twelve miles west of south from Dayton—in the occurrence and connections of which there are several facts of unusual interest.

It lies in, and directly above, the channel of Twin creek, a tributary of the Miami river. The general course of the creek is southeasterly, but just above the point where the peat bed is exposed, it has made a sudden change in direction from east to west of south. Its northern and eastern banks for ¼ of a mile in each direction from the point of deflection, are precipitous walls of stratified clay and gravel, from 50 to 100 feet in thickness; kept nearly vertical by the constant undermining action of the stream.

Beneath these heavy deposits and occupying 40 rods of the east bank of the creek, the peat bed is found, varying in thickness, in different portions of its extent, from 12 to 20 feet. The amount of the bed that is exposed depends upon the stage of water in the stream. The stream is bedded for 10 or 15 rods upon the peat, but in deeper portions of the channel, upon the eastern bank, an underlying formation of gravel can be detected. The uppermost layers of the peat contain undecomposed sphagnous mosses, grasses and sedges, but in other portions of the bed, the vegetable structure is generally indistinct, with the exception of abundant fragments of coniferous wood, which in many instances can be identified as Red Cedar (*Juniperus virginianus*). At the southern extremity of the bed in particular, there is a great accumulation of wood, in trunks, roots, branches and twigs, much of which has been flattened by the pressure of the 80 feet of clay and gravel that overlie it. Branches that were originally two inches in diameter now afford lenticular sections with no more than a ¼ inch for the shorter axis, while many of the smaller stems have been compressed into ribbons. The berries of the cedar are abundant in the upper layers of the peat. At a point ½ mile higher up the stream, trunks of cedar nearly two feet in diameter, have been taken from beneath these same drift beds and turned to account for fencing posts.

There are indications that the peat bed has a considerable extent to the northward and eastward. A bed of "black earth" was found underlying clay and gravel in digging a well 1¾ miles east of this locality. The bed occurred at a depth of 30 feet and was itself from 10 to 15 feet in thickness. The waters of springs in the same neighborhood are discolored, as if by contact with such deposits.

It may be added in this connection that there is a large amount of wood buried beneath the drift throughout this region generally. It is not a circumstance of infrequent occurrence to meet with it in the digging of wells. There is scarcely a square mile in the thickly settled portions of the adjacent country in which instances of this kind can not be found, and three instances are on record within the limits of a single village.

The wood is in great part coniferous, but not exclusively so; for according to the testimony of intelligent and observing, practical men who deem themselves entirely competent to give a judgment in the case, ash, hickory and sycamore, together with grape-vines and beech leaves, have been found covered with drift deposits.

A stratum of soil, one or two feet in thickness is often associated with these vegetable remains. The soil and the wood occur at various depths, but in the cases already noted, between the limits of 10 feet and 90 feet. A large proportion, however, of the instances on record, have been found at about 30 feet in depth, immediately beneath the yellow clays that constitute the last of the drift series in this region.

Through all portions of the peat above mentioned, sand and pebbles are scattered. The pebbles are mostly of small size, seldom larger than a pea, but occasionally three or four inches in diameter. They agree in general character with the gravel of the country.

At the lower extremity of the peat bed, the formation thins out and the bottom layers are found above the water, resting upon a surface of gravel that slopes downward at an angle of about 30 degrees. All the limestone pebbles which the peat overlies at this point, appear to have been "burned." They are white and soft, as much so as they would have been if they had been converted into hydrates of lime by the ordinary processes. Analysis, however, shows them to be in the state of carbonates.

In the inclined strata, heavy beds of ochreous gravel occur. The ochre is easily separated from the gravel by washing and furnishes a marketable paint of fair quality. The nature and arrangement of the materials of these inclined beds indicate that they were brought from the eastward by a torrent-like stream and deposited over a precipitous bank.

In pockets of the gravel and also in the clay that immediately covers the peat, small quantities of vivianite, "blue earth," or phosphate of iron, are found. From one of the largest accumulations of this substance, a tusk or tooth was taken. It was described as resembling a hog's tusk, except that it was much larger. It may also be added that two mastodon tusks, each measuring eight feet in length, were taken in the spring of 1870, from the northern part of the same drift bed to which the peat belongs and at about the same level.

The reference of the phosphoric acid of the vivianite to vertebrate bones will, therefore, hardly be questioned.

From the above named facts, we seem warranted in concluding that the coniferous wood in question grew in the region where we find it buried. The amount of the wood renders this probable and the nature of the remains forbids any other supposition. In this connection, it is only needful to recall the facts, that cedar berries in notable quantity, and that branching twigs, the veriest spray of the cedar, sometimes still covered with bark, are well preserved in the peat.

We learn furthermore that the date, at which this vegetation grew, was in the closing or Champlain epoch of the Drift period, for it is underlain by stratified drift deposits. A subsidence of the continent below its present level had already occurred, during which these underlying beds were formed, but there would seem to have been a restoration of this southern border of the drift-swept region at least, to dry land once more, and this restoration must have continued through a period of considerable length. It was followed by another movement of depression, during which the highest of the yellow clays, the latest formation of the drift, were deposited. There seem materials in this line of facts for a more orderly division of the later formed deposits of the post-tertiary than has heretofore been recognized.

We also learn that mammalian life was associated with this intercalated period of vegetable growth. The mammoth and the mastodon subsisted on the coniferous wood which is represented so largely here. The series of changes in level already referred to, must have exterminated these earlier representatives of elephantine life, but we find the same species returning to their old dwelling places when the waters of the drift seas had finally abated.

ART. IX.—*On the Theoretical Temperature of the Sun; under the Hypothesis of a Gaseous Mass maintaining its Volume by its Internal Heat, and depending on the Laws of Gases as known to Terrestrial Experiment;* by J. HOMER LANE, Washington, D. C.

[Read before the National Academy of Sciences at the session of April 13–16, 1869.]

MANY years have passed since the suggestion was thrown out by Helmholtz, and afterwards by others, that the present volume of the sun is maintained by his internal heat, and may become less in time. Upon this hypothesis it was proposed to account for the renewal of the heat radiated from the sun, by means of the mechanical power of the sun's mass descending toward his center. Calculations made by Prof. Pierce, and I believe by others, have shown that this provides a supply of heat far greater than it is possible to attribute to the meteoric theory of Prof. Wm. Thomson, which, I understand, has been abandoned by Prof. Thomson himself as not reconcilable with astronomical facts. Some years ago the question occurred to me in connection with this theory of Helmholtz whether the entire mass of the sun might not be a mixture of transparent gases, and whether Herschel's clouds might not arise from the precipitation of some of these gases, say carbon, near the surface, with their revaporization when fallen or carried into the hotter subjacent layers of atmosphere beneath; the circulation necessary for the play of this Espian theory being of course maintained by the constant disturbance of equilibrium due to the loss of heat by radiation from the precipitated clouds. Prof. Espy's theory of storms I first became acquainted with more than twenty years ago from lectures delivered by himself, and, original as I suppose it to be, and well supported as it is in the phenomena of terrestrial meteorology, I have long thought that Prof. Espy's labors deserve a more general recognition than they have received abroad. It is not surprising, therefore, in a time when the constitution of the sun was exciting so much discussion, that the above suggestions should have occurred to myself before I became aware of the very similar, and in the main identical, views of Prof. Faye, put forth in the Comptes Rendus. I sought to determine how far such a supposed constitution of the sun could be made to connect with the laws of the gases as known to us in terrestrial experiments at common temperatures. Some calculations based upon conjectures of the highest temperature and least density thought supposable at the sun's photosphere led me to the conclusion that it was extremely difficult, if not impossible, to make out the connection in a credible manner. Nevertheless, I mentioned my ideas to Prof. Henry, Secretary of the Smithsonian Institution, when he immediately referred me to a number of the Comptes Rendus, then recently received, containing Faye's exposition of his theory. Of course nothing is further from my purpose than to make any kind of claim to any thing in that publication. After becoming acquainted with his labors I still regarded the theory as seriously lacking, in its physical or mechanical aspect, the direct support of confirmatory observations, and even as being subject to grave difficulty in that direction. In this attitude I allowed the subject to rest until my friend Dr. Craig, in charge of the Chemical Laboratory of the Surgeon General's office, without any knowledge of Faye's memoir, or of my own suggestions previously made to Prof. Henry and another scientific friend, fell upon the same ideas of the sun's constitution, availing himself, precisely as I had done, of Espy's theory of storms. Dr. Craig's ideas were communicated to a company of scientific gentlemen early last spring, and soon after, Prof. Newcomb, of the U. S. Naval Observatory, entered into a general survey of the nebular hypothesis. These communications of Dr. Craig and Prof. Newcomb led me to enter into a renewed examination of the mechanical embarrassment under which I had believed the theory to labor. Not any longer relying on my first rough estimate based on assumed high temperatures at the photosphere, the question was now inverted. Assuming the gaseous constitution, and assuming the laws expressed in Poisson's formulæ, known to govern the constitution of gases at common temperatures and densities, what shall we find to be the temperatures and densities corresponding to the observed volume of the sun supposing it were composed of some known gas such as hydrogen, or supposing it to be composed of such a mixture of gases as would be represented by common air. Pure hydrogen will, of course, give us the lowest temperature of all known substances, under the general hypothesis.

The question was resolved, and the results were communicated in graphical and numerical form in May or June last to two or three scientific friends, but their publication has been delayed by an unavoidable absence of several months from home.

Premising that the unit of density shall correspond to a unit of mass in the cube of the unit of length, the unit of force to the force of terrestrial gravity in the unit of mass, and the unit of pressure or elasticity in the gas to the unit of force on a surface equal to the square of the unit of length:

Let r = the distance of an element of the sun's mass from the sun's center,

t = the temperature of the element,

σt = its atmospheric subtangent, referred to the force of gravity at the earth's surface, or height of the column of homogeneous gas, whose terrestrial gravitating force would equal its elasticity,

ϱ = its density, or mass of its unit volume,
= force of terrestrial gravity in its unit volume,

$\varrho \sigma t$ = its elasticity, or elastic force per unit surface,

R = the earth's radius,

M = the earth's mass,

m = the mass of the part of the sun's body contained in the concentric sphere whose radius is r,

$\frac{M r^2}{m R^2} \sigma t$ = the subtangent of the gas under its actual gravitating force in the sun.

The condition of equilibrium between the gravitating force of a thin horizontal layer of gas whose thickness is dr, and the difference of elastic force between its lower and upper surfaces, is expressed by the equation,

$$d \cdot \varrho \sigma t = -\frac{m R^2}{M r^2} \varrho \, d r.$$

Under the hypothesis that the law of Mariotte and the law of Poisson prevail throughout the whole mass, and that this mass is in convective equilibrium, we have

$$\sigma = \text{a constant}, \qquad (1)$$
$$t = t_1 \varrho^{k-1},^*$$

t_1 representing the value of t in the part of the mass where the density is a unit.

The theoretical difficulties which, if the supply of solar heat is to be kept up by the potential due to the mutual approach of the parts of the sun's mass consequent on the loss of heat by radiation, come in when we suppose a material departure from these laws of Mariotte and of Poisson at the extreme temperatures and pressures in the sun's body, or how far such difficulties intervene, will be considered further on.

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* k represents the ratio of the specific heat of a gas under constant pressure to its specific heat under constant volume.

By means of the constant value of σ, and the value of t given in (1), the above differential equation is transformed into

$$k\,\sigma\,t_1\,\varrho^{k-2}d\varrho=-\frac{m}{M}\,\frac{R^2}{r^2}d\,r,$$

the integral of which gives

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{k-1}=\frac{k-1}{k}\,\frac{R^2}{\sigma M t_1\,\varrho_0{}^{k-1}}\int_0^r\frac{mdr}{r_2}, \tag{2}$$

in which ϱ_0 is the value of ϱ at the sun's center.

We have also

$$m=4\pi\int_0^r \varrho r^2dr=4\pi\varrho_0\int_0^r\frac{\varrho}{\varrho_0}r^2dr. \tag{3}$$

If now we put

$$r=\left(\frac{k\sigma Mt_1}{4(k-1)R^2\pi\varrho_0{}^{2-k}}\right)^{\frac{1}{2}}x, \tag{4}$$

we shall have

$$m=4\pi\varrho_0\left(\frac{k\sigma Mt_1}{4(k-1)R^2\pi\varrho_0{}^{2-k}}\right)^{\frac{3}{2}}\mu, \tag{5}$$

in which

$$\mu=\int_0^x\frac{\varrho}{\varrho_0}x^2dx, \tag{6}$$

and equation (2) becomes

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{k-1}=\int\frac{\mu dx}{x^2}. \tag{7}$$

In equations (6) and (7) it is plain that upon the value of k alone depends: first the form of the curve that expresses the value of $\frac{\varrho}{\varrho_0}$ for each value of x; secondly, the value of the upper limit of x corresponding to $\frac{\varrho}{\varrho_0}=0$; and thirdly, the corresponding value of μ. These limiting, or terminal, values of x and μ, cannot be found except by calculating the curve, for equations (6) and (7) seem incapable of being reduced to a complete general integral. But when these values have been found for any proposed value of k, they may be introduced once for all into equations (4) and (5), from which the values of ϱ_0, and of $\sigma\,t_1$, are at once deduced.

I have made these calculations for two different assumed values of k, viz., $k=1{\cdot}4$, which is near the experimental value it has in common air, and $k=1\frac{2}{3}$, which is the maximum possible value it can have in the light of Clausius' theory of the constitution of the gases. The calculation of the curve of $\frac{\varrho}{\varrho_0}$, or of $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$, begins at the sun's center where $x=0$. For the small values of x, integration by series enables us readily to deduce from equations (6) and (7) the following approximate numerical equations:

For $k=1{\cdot}4$,

$$\mu=\tfrac{1}{3}x^3-\tfrac{1}{12}x^5+\tfrac{5}{336}x^7-\tfrac{125}{54432}x^9+\text{\&c.} \qquad (8)$$

$$1-\left(\frac{1}{\varrho_0}\right)^{\cdot 4}=\tfrac{1}{6}x^2-\tfrac{1}{48}x^4+\tfrac{5}{2016}x^6-\tfrac{125}{435456}x^8+\text{\&c.} \qquad (9)$$

For $k=1\frac{2}{3}$,

$$\mu=\tfrac{1}{3}x^3-\tfrac{1}{20}x^5+\tfrac{1}{240}x^7-\tfrac{1}{3888}x^9+\text{\&c.} \qquad (10)$$

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{3}}=\tfrac{1}{6}x^2-\tfrac{1}{80}x^4+\tfrac{1}{1440}x^6-\tfrac{1}{31104}x^8+\text{\&c.} \qquad (11)$$

For larger values of x, until $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ becomes sufficiently small as there is no need of great precision in these calculations, I have merely developed the values of μ and $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ corresponding to $x=1{\cdot}1$, $x=1{\cdot}2$, $x=1{\cdot}3$, &c., by means of differences taken from the differential co-efficients at the middle of each increment of x, and for the same reason have thought it sufficient to begin with $x=1$, in equations (8) and (9) or (10) and (11). After arriving at a sufficiently small value of $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ the calculation is finished by aid of the following approximate equations also derived by integration fron (6) and (7).

$$\mu'-\mu=\frac{k-1}{k}\,\mu'^{\frac{1}{k-1}}\,x'^{\,2-\frac{2}{k-1}}\,(x'-x)^{1+\frac{1}{k-1}}\,(1+X) \qquad (12)$$

$$\left(\frac{\varrho}{\varrho_0}\right)^{k-1}=\frac{\mu'(x'-x)}{x'x}-\frac{(k-1)^2}{k(2k-1)}\mu'^{\frac{1}{k-1}}\,x'^{-\frac{2}{k-1}}\,(x'-x)^{2+\frac{1}{k-1}}$$
$$-\frac{(k-1)(2k^2-3k+2)}{k(2k-1)(3k-2)}\,\mu'^{\frac{1}{k-1}}\,x'^{-1-\frac{2}{k-1}}\,(x'-x)^{3+\frac{1}{k-1}} \qquad (13)$$

In these equations x' and μ' are the values of x and μ corresponding to $\frac{\varrho}{\varrho_0}=0$, or the upper limit of the supposed solar atmosphere, and

$$X = -\frac{k(2k-3)}{(k-1)(2k-1)}\,\frac{x'-x}{x'} + \frac{k(k-2)(2k-3)}{2(k-1)^2(3k-2)}\,\frac{(x'-x)^2}{x'^2} + \text{\&c.}$$

$$-\frac{k-1}{2k(2k-1)}\mu'^{-1+\frac{1}{k-1}}\,x'^{2-\frac{2}{k-1}}\,(x'-x)^{1+\frac{1}{k-1}} + \text{\&c.}$$

With the values of x' and μ' determined, using r' and m' to express in like manner the corresponding values of r and m at the upper limit of the theoretical atmosphere, we find from equations (4) and (5)

$$\varrho_0 = \frac{m'x'^3}{4\pi\mu'r'^3}, \qquad (14)$$

$$\sigma t_1 = \frac{4\pi(k-1)R^2r'^2\varrho_0{}^{2-k}}{kMx'^2},$$

and by equation (1), $$\sigma t = \frac{4\pi(k-1)R^2r'^2\varrho_0}{kMx'^2}\left(\frac{\varrho}{\varrho_0}\right)^{k-1}, \qquad (15)$$

$$= \frac{k-1}{k}\,\frac{m'R^2x'}{\mu'Mr'}\left(\frac{\varrho}{\varrho_0}\right)^{k-1} \qquad (16)$$

A glance at equation (7) will show that $\frac{\mu'(x'-x)}{x'x}$, equation (13), or $\frac{\mu'}{x'}\,\frac{r'-r}{r}$ may be taken equal to $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ throughout the considerable upper part of the volume of the hypothetic gaseous body in which $1-\frac{\mu}{\mu'}$, or $1-\frac{m}{m'}$, is sufficiently small to be neglected. This substitution in the last equation gives

$$\sigma t = \frac{k-1}{k}\,\frac{m'R^2}{Mrr'}\,(r'-r), \text{ nearly,} \qquad (17)$$

and also $$\varrho = \left(\frac{\mu'}{x'}\right)^{\frac{1}{k-1}}\varrho_0\left(\frac{r'-r}{r}\right)^{\frac{1}{k-1}} \text{ nearly,}$$

$$= \frac{1}{4\pi}\mu'^{-1+\frac{1}{k-1}}\,x'^{3-\frac{1}{k-1}}\,\frac{m'}{r'^3}\left(\frac{r'-r}{r}\right)^{\frac{1}{k-1}} \qquad (18)$$

Now the mechanical equivalent of the heat in the mass ϱ of a cubic unit in volume of any perfect gas whose atmospheric subtangent is σt, is $\frac{1}{k-1}\,\varrho\cdot\sigma t$, and the mechanical equivalent of the heat that it would give out, in being cooled down under constant pressure to absolute zero, is $\frac{k}{k-1}\,\varrho\cdot\sigma t$. If the density ϱ is taken in units of the density of water, and the unit of length be the foot, this expression is multiplied by $62\frac{1}{2}$ to give for the mechanical equivalent in foot pounds

$$62\tfrac{1}{2}\frac{k}{k-1}\,\varrho\cdot\sigma t=\frac{62\frac{1}{2}}{4\pi}\,\mu'^{\,-1+\frac{1}{k-1}}\,x'^{\,3-\frac{1}{k-1}}\frac{m'^2R^2}{Mr'^4}\left(\frac{r'-r}{r}\right)^{1+\frac{1}{k-1}} \qquad (19)$$

The mechanical equivalent $\frac{1}{k-1}\,\varrho\cdot\sigma t$, of the heat in the mass ϱ, viewed in the light of Clausius' mechanical theory of the gases, includes the motions of the separate atoms of each supposed compound molecule relatively to each other, as well as the motion of translation which each compound molecule makes in a straight path through free space till it impinges upon another compound molecule. If we wish to find the mechanical equivalent which would be due to this motion of translation alone, we must put $k=1\frac{2}{3}$ in the factor $\frac{1}{k-1}$ by which $\varrho\cdot\sigma t$ is multiplied, and this gives $\frac{3}{2}\varrho\cdot\sigma t$. To find from this the mean of the squares of the velocities of translation of the compound molecules, we divide by the mass ϱ, and, if the foot be the unit of length, multiply by 64·3, whence we have for the velocity found by taking the square root of this mean of the squares

$$8{\cdot}02\sqrt{\tfrac{3}{2}\sigma t}=8{\cdot}02\left(\frac{3}{2}\,\frac{k-1}{k}\,\frac{m'R^2x'}{\mu'Mr'}\right)^{\frac{1}{2}}\left(\frac{\varrho}{\varrho_0}\right)^{\frac{k-1}{2}} \qquad (20)$$

Determination of the curve of density for $k=1{\cdot}4$.—Beginning with $x=1$, in equations (8) and (9), we find $\mu=\cdot2626$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{4}{10}}=\cdot8520$. Developing the values of μ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{4}{10}}$ for $x=1{\cdot}1$, $x=1{\cdot}2$, &c., by means of differences we arrive at the values $\mu=2{\cdot}145$ and $\left(\frac{\varrho}{\varrho^0}\right)^{\frac{4}{10}}=\cdot1378$ when $x=4{\cdot}0$. Putting these values into equations (12) and (13) we find

$$x'=5{\cdot}355,\quad \mu'=2{\cdot}188.$$

If we now allow $\frac{1}{12}$d of the radius of the photosphere, or about 20,000 miles, for the height of the theoretic upper limit of the solar atmosphere above the photosphere, and if we take the mean specific gravity of the earth's mass at $5\frac{1}{2}$, and the mean specific gravity of the sun within the photosphere at $\frac{1}{4}$ that of the earth, as it is known to be, these values of x' and μ' give us in equation (14)

$$\varrho_0=28{\cdot}16,$$

so that the density of the sun's mass at the center would be nearly one-third greater than that of the metal platinum.

Curve of density for $k=1\frac{2}{3}$.—For this value of k the numerical coefficients in equations (8) and (9) are replaced by those in (10) and (11). Otherwise, the same process employed with the value $k=1{\cdot}4$, gives, starting with $x=1$, $\mu={\cdot}2875$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{5}}={\cdot}8452$, and developing for $x=1{\cdot}1$, $x=1{\cdot}2$, &c., brings us to $\mu=2{\cdot}557$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{5}}={\cdot}1591$, for $x=3{\cdot}0$, and finally gives us

$$x'=3{\cdot}656, \quad \mu'=2{\cdot}741,$$

and if we now assume the same height as before for the theoretic upper limit of the sun's atmosphere, instead of $\varrho_0=28{\cdot}16$, we find

$$\varrho_0=7{\cdot}11.$$

The new curve of density is found in the same way as the first, and is presented to the eye in the diagram in comparison with it. In the upper part of both curves the scale of density is increased ten fold, and it is, in part only, evident to the eye how immensely different, for the two values of k, becomes the density in the upper parts of the sun's mass. It appears to the eye only in part because the ratio of the two densities multiplies itself rapidly in approaching the upper limit of the atmosphere.

The above was communicated in writing as here given, to the Academy at its late session.* The draft of the following, and a part of the details of its substance, have been prepared since.

Equation (20) gives in feet the square root of the mean square of velocity of translation of molecules $(8{\cdot}02\sqrt{\tfrac{3}{2}\sigma t})$. At the sun's center we find this would be 331 miles per second for the curve of density corresponding to $k=1\tfrac{2}{3}$, and 380 miles per second for the curve of density corresponding to $k=1{\cdot}4$.

In 1838 Pouillet, following the law of heat radiation given by Dulong and Petit, estimated the temperature of the radiating surface of the sun, from observations by himself of the quantity of heat it emits, at from 1461° C. to 1761° C. Herschel, from Pouillet's observations, and his own made at the Cape of Good Hope about the same time, adopts, after allowing one-third for the absorption of our atmosphere, forty feet as the thickness of ice that would be melted per minute at the sun's surface. The temperature of the radiating surface calculated from this datum by the formula of Dulong and Petit, and with the co-efficient of radiation found by Prof. W. Hopkins for sandstone, the smallest co-efficient he found, is 1550° C. or 2820° Fah. But then the solar radiation is many thousands of times greater than the greatest in Dulong and Petit's experiments, so that these calculations of the temperature of the sun's photosphere have little weight notwithstanding the simplicity and accuracy with which the formula represents the experiments from which it was derived. Nothing authorizes us to accept the formula as more than an empirical one. It seems desirable that experiments similar to those of Dulong and Petit should be made on the rate of cooling of intensely heated bodies, such as balls of platinum not too large. By placing the heated ball in the center of a hollow spherical jacket of water, either flowing or in an unchanged mass, the quantities of heat radiated in successive equal spaces of time will be determined, and the corresponding differences of temperature in the heated ball can at least be estimated with whatever probability we may rely on our knowledge of the specific heat of its material. At present the best means we have of forming any judgment of the probable temperature of the source of the sun's radiation, is perhaps to be found in a comparison between the effects of the hydro-oxygen blowpipe, and the recorded effects of Parker's great burning lens. I am not aware that this method has before been resorted to.

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* I desire here to state that the formulæ which show the relation between the temperature, the pressure, the density, and the depth below the upper limit of the atmosphere, so far as they apply to the upper part of the sun's body, were independently pointed out by Prof. Peirce, in a very interesting paper which that distinguished physicist read before the Academy at the same session, and prior to the presentation of this paper. Also to recall a fact which I first learned from Prof. Peirce's mention of it to the Academy, viz. that Prof. Henry long ago threw out the idea of the atmospheric condition to which Prof. Thomson has more recently given the term convective equilibrium, viz., such that any portion of the air, on being conveyed into any new layer above or below, would find itself reduced, by its expansion or compression, to the temperature of the new layer.

If the angle of aperture at the focus of a burning lens, or combination of lenses, be called $2a$, the radiation received by a small flat surface at the focus will be $\sin^2 a$, if a unit be taken to represent the radiation the same small flat surface would receive just at the sun's surface. Parker's lens, with the small lens added, had, at the focus so formed, an angle of aperture of about 47°. A small flat surface at its focus would therefore receive about one-sixth the radiation that it would just at the sun, making no allowance for absorption by the atmospheres of the earth and sun and rays lost in transmission through the lenses. Pouillet, from the experiments already alluded to made by himself, found his atmosphere in fine weather transmitted, of the sun's heat rays, about the fraction $\frac{4}{5}$ raised to a power whose exponent is the secant of the sun's zenith distance. This, of course, leaves out of view the heat rays of low intensity which are totally absorbed by the atmosphere. He also concluded from comparison with other experiments of his own with a moderately large burning glass, that that glass transmitted $\frac{7}{8}$ of the heat rays incident on it. If we assume the same fraction for each of the two lenses of Parker's combination, and assume further that the sun's zenith distance did not exceed 48° in the experiments made with it, we find for the fractional multiplier expressing the part of the sun's heat radiation which arrived at the focus unintercepted, $(\frac{4}{5})^{1\ 1\cdot5}(\frac{7}{8})^2 = \cdot55$. Hence the radiation actually received by a small flat surface at the focus was ·09, or about one-eleventh, of what it would receive just at the sun. The heat so received by any body so placed in the focus, must, after the body has acquired its highest temperature, be emitted from it at the same rate. The heat so emitted will consist: first, of heat radiated into that part of space toward which the radiating surface of the body looks; secondly, of heat carried of by convection of the air; thirdly, of heat conducted away by the body supporting the body subjected to experiment; fourthly, of heat rays, if any, reflected, and not absorbed, by the body subjected to experiment. Assuming it as a reasonable conjecture that full half of all this* consists of heat *radiated* into the single *hemisphere* looking upon a flat surface, we may conclude that the body, at its highest acquired temperature, radiated not less than $\frac{1}{25}$th as much heat as is radiated by an equal extent of surface of the sun's photosphere, over and above such part of that radiation as may be intercepted by the sun's atmosphere, and such rays of low intensity as are *totally* absorbed by our own atmosphere, the whole of which apparently cannot be great. No allowance seems necessary for the chromatic and spherical dispersion of the lenses, since the diameter of the focus is stated at half an inch, while the true diameter of the sun's image would be not less than one-third of an inch.

Now we are not without the means of forming a probable approximate estimate of this temperature at which the radiation becomes $\frac{1}{25}$th, more or less, of that of the sun's photosphere. We are told that in the focus of Parker's compound lens 10 grains of very pure lime ("white rhomboidal spar") were melted in 60 seconds. We may presume that in that length of time the temperature of the lime, after parting with its carbonic acid, made a near approximation to the maximum at which it would be stationary, a presumption confirmed by the period of 75 seconds said to have been occupied in the fusion of 10 grains of carnelian, and by the considerable period of 45 seconds for the fusion of a topaz of only 3 grains, and 25 seconds for an oriental emerald of but 2 grains, and in fact sufficiently proved, it would seem, by observing that the heat we have estimated to fall at the focus, upon a flat surface, would suffice, if retained, to raise the temperature of a quarter of an inch thick of lime 4000° Fah. in 5 seconds. If, then, we may take the temperature *maintained* at the focus of Parker's lens to have been at the melting point of lime, we may conclude that it is also not far from the temperature given by the hydro-oxygen blowpipe. Dr. Hare, who was the first inventor of this instrument, and the discoverer of its great power, melted down, by its means, in partial fusion, a very small stick of lime cut on a lump of that material, which we understand to have been a very pure specimen. Burning glass and blowpipe seem each to have been near the limit of its power in this apparently common effect. But Deville found the temperature produced by the combination of hydrogen and oxygen under the atmospheric pressure to be 2500° Cent. As the lime in the heated blast would radiate rapidly, its temperature must have been lower than that of combined hydrogen and oxygen, and I have called it 2220° Cent. or 4000° Fah.

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* As to the heat carried off by convection of the air, if its quantity be calculated by the formula given by Dulong and Petit for that purpose, it comes out utterly insignificant in comparison with the heat received from the burning glass. The conjectural allowance of $\frac{4}{5}$ths in all, of this, is likely, therefore, to be much too large. Not much reliance, indeed, can be placed upon the formula here mentioned, at such a temperature as 4000° Fah., yet, as by it the convection is taken proportional to the 1·233 power of the difference of temperature, it seems unlikely that it gives a quantity very many fold less than the truth.

The formula of Dulong and Petit, with the co-efficient found by Hopkins, as already mentioned, gives for the quantity of heat radiated in one minute by a square foot of surface of a body whose temperature is $\theta+t$ centigrade, into a chamber whose temperature is θ centigrade, when expressed with the unit employed by Hopkins,

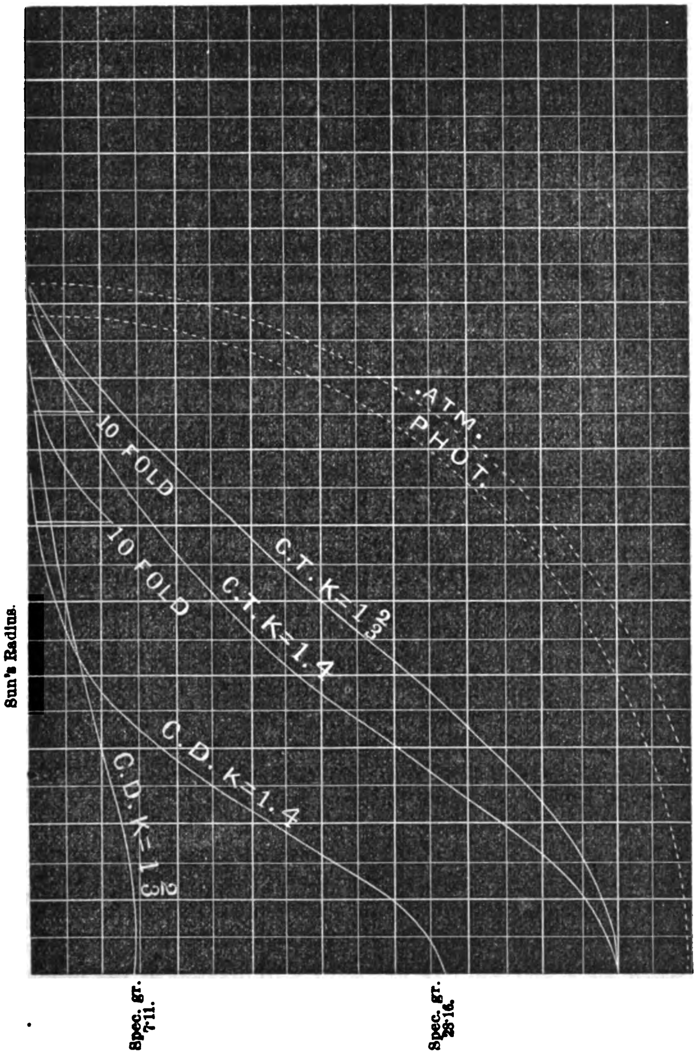
$$8{\cdot}377\,(1{\cdot}0077)^{\theta}\,[(1{\cdot}0077)^{t}-1].$$

It will be convenient, and, in the discussion of the high temperatures with which we are concerned, will involve no sensible error, to use the hypothesis that the space around the radiating body is at the temperature of 0° C. and the formula for the radiation then becomes,

$$8{\cdot}377\,[(1{\cdot}0077)^{t}-1]. \qquad (21)$$

The unit used by Hopkins, in the formula here given, is the quantity of heat that will raise the temperature of 1000 grains of water 1° centigrade. Expressed by the same unit, the quantity adopted by Sir J. Herschel as the amount of the sun's radiation, viz. that which would melt 40 feet thick of ice in a minute (at the sun's surface), is 1,280,000. The $\frac{1}{20}$th of this, or 64,000, expresses, therefore, the quantity which we have estimated the lime under Parker's lens to have radiated, per square foot of its surface, at its estimated temperature of 4000° Fah. If now we calculate its temperature by the above formula, from the estimated radiation, the result is 1166° Cent. or 2130° Fah. This is manifestly much below the real temperature, and so far below that there can be no doubt the formula of Dulong and Petit has failed at the melting point of lime. If instead of the co-efficient 8·377 we had used the larger co-efficient 12·808 which Hopkins gives for unpolished limestone, the formula would have been reduced only 53° Cent. It best suits the direction of our inquiry to use the smallest co-efficient which Hopkins' experiments gave, since we are seeking the *highest* temperature which can be plausibly deduced from the sun's radiation. For ease of expression, the curve which we will imagine for representing the actual relation of radiation to temperature, the horizontal ordinate standing for the temperature and the vertical ordinate for the radiation corresponding thereto, may be called the curve of radiation. The course of this curve from the freezing point of water to a point somewhat below the boiling point of mercury is correctly marked out to us by the formula. Beyond that we have but the rough approximation which we can get by means of the above comparison, to the single point of the curve where the radiation is $\frac{1}{20}$th that of the sun's photosphere. The attempt, from these data, to extend the curve till it reaches the full radiation of that photosphere, must be mainly conjectural. As a basis for the most plausible conjecture I am able to make let us assume: first, that the upward concavity of the curve of radiation, which increases very rapidly with the temperature as far as the curve follows the formula of Dulong and Petit, is at no temperature greater than that formula would give it at the same temperature; secondly, that the curve of radiation is nowhere convex upward. If, then, we set out from these two conjectural assumptions—of the degree of probability of which each one must form his own impression—the greatest temperature the sun's photosphere could have consistently with the radiation of 64,000 at the temperature of 4000° Fah., is found by drawing through the point representing that radiation and that temperature a straight line tangent to the curve of the formula. The line so drawn would cross the real curve of radiation in a greater or less angle at the radiation of 64,000 and temperature of 4000° Fah., and at higher temperatures would fall more or less *below* that curve, and its intersection with the sun's radiation of 1,280,000 would be at a temperature greater than that of the curve, that is to say, greater than the temperature of the sun's photosphere. This greater temperature is 55,450° Fah.

A different train of conjecture led me at first to assume a temperature of 54,000° Fah., and this last number I will here retain since it has been already used as the basis of some of the calculations we now proceed to give. It must be here recollected that we are discussing the question of *clouds* of *solid* or at least *fluid* particles floating in non-radiant gas, and constituting the sun's photosphere. If the amount of *radiation*



Explanation.—Atm., Assumed theoretic upper limit of atmosphere; Phot.. Photosphere; C.T.K $=1\frac{2}{3}$, Arbitrary Curve of temperature for $k=1\frac{2}{3}$; C.T.K. $=1·4$, Arbitrary Curve of temperature for $k=1·4$; C.D.K. $=1·4$, Absolute Curve of density for $k=1·4$; C.D.K. $=1\frac{2}{3}$, Absolute density for $k=1\frac{2}{3}$.

would lead us to limit the temperature of such clouds of solids or fluids, so also it seems difficult to credit the *existence* in the solid or fluid form, at a higher temperature than 54,000° Fah. of any substance that we know of.

If then we suppose a temperature of 54,000° Fah., what would be the density of that layer of the hypothetic gaseous body which has that temperature, and what length of time would be required, at the observed rate of solar radiation, for the emission of all the heat that a foot thick of that layer would give out in cooling down under pressure to absolute zero? The latter question depends on the mechanical equivalent of this heat for a cubic foot of the layer of gas, and the two questions, together with that of the depth at which the layer would be situated below the theoretic upper limit of the atmosphere, are answered by equations (17), (18), and (19), provided we knew the value of k and the value of σ in the body of gas. The less the atomic weight of the gas the greater the value of σ, and the greater the density of the layer of 54000° Fah. and the greater the quantity of heat which a cubic foot of it would give out in cooling down. I therefore base the first calculation on hydrogen as it is known to us. The value of σ is in that case about 800 feet, and the value of k about 1·4, nearly the same as in common air. These values would give for the layer of 54000° Fah. a specific gravity about ·00000095 that of water, or about one 90th that of hydrogen gas at common temperature and pressure, and the mechanical equivalent of the heat that a cubic foot of the layer would give out in cooling down under pressure to absolute zero would be only about 9000 foot pounds, whereas the mechanical equivalent of the heat radiated by one square foot of the sun's surface in one minute is about 254,000,000 foot pounds. The heat emitted each minute would, therefore, be fully half of all that a layer ten miles thick would give out in cooling down to zero, and a circulation that would dispose of volumes of cooled atmosphere at such a rate seems inconceivable.

It may possibly appear to some minds that the difficulty presented by this aspect of the case will vanish if we suppose the photosphere, or its cloudy particles, to be maintained by radiation at a temperature to almost any extent lower than that of convective equilibrium. This would enable us to place the theater of operations in a lower and denser layer of atmosphere, but the supposition seems to me difficult to realize unless, as the hot gases rise from beneath, precipitation could commence at a temperature many times higher than the 54000° Fah. which we have estimated for the upper visible surface of the clouds, and this, as before intimated, seems to me itself extremely improbable.

I may mention here that my friend Dr. Craig, in an unpublished paper, following the hint thrown out by Frankland, is disposed to favor the idea that the sun's radiation may be the radiation of hot gases instead of clouds. At present I shall offer no opinion on that point one way or the other, but will only state it as my impression that if the theory of precipitated clouds, as above presented, is the true one, something quite unlike our present experimental knowledge, or at least much beyond it, is needed to make it intelligible.

The first hypothesis which offers itself in an attempt to make the theory rational is suggested by one point in Clausius' theory of the constitution of the gases, already alluded to. In forming his theory Clausius found that the known specific heats of the gases are all much too great for free simple atoms impinging on one another, and he therefore introduced the hypothesis of compound molecules, each compound molecule being a system of atoms oscillating among each other under forces of mutual attraction. Now if this were accepted as the actual constitution of the gases it is of course easy enough to conceive that in the fierce collisions of these compound molecules with each other at the temperatures supposed to exist in the sun's body, their component atoms might be torn asunder, and might thenceforth move as free simple molecules. In this case, still retaining the hypothesis of Clausius' theory, that the average length of the path described by each between collisions is large compared with the diameter of the sphere of effective attraction or repulsion of atom for atom, the value of k would reach its maximum of $1\frac{2}{3}$. Experiment has not shown us any gas in this condition, and for the present it is hypothetical. Even in hydrogen the value of k does not materially, if any, exceed the value of 1·4 which it has in air. But if it were found that the hydrogen molecule is compound, and that in the body of the sun the heat splits this molecule into two equal simple atoms, and in fact that all the matter in the sun's body is split into simple free atoms equally as small, then, while the value of k would be $1\frac{2}{3}$, the value of σ would be about 1600 feet. If with these values we repeat the calculation of the density of the layer of 54000° Fah. we find its specific gravity to be 0·000363 of that of water, or 4·35 times that of hydrogen gas at common temperature and pressure and in its known condition, or 8·7 times that which the hydrogen in the hypothetic condition would have if it retained that condition at common temperature and pressure. We find also that the mechanical equivalent of all the heat that a cubic foot of the layer would give out in cooling down, under pressure, to zero, would be no less than 13,500,000 foot pounds. Instead, therefore, of a layer ten miles thick, it would now require only a thickness of 38 feet to give out, in cooling down to zero, twice the heat emitted by the sun in one minute. It will be seen, (equations (17) and (19)), that this thickness, retaining the constant value $k=1\frac{2}{3}$, would diminish with the $2\frac{1}{2}$ power of the masses of the atoms into which the sun's body is hypothetically resolved (the reciprocal of the value of σ), and I leave each to form his own impression how far this view leads towards verisimilitude.

It is important to add that the depth of the layer of 54000° Fah. below the theoretic upper limit of atmosphere, when calculated with value $k=1{\cdot}4$, $\sigma=800$ feet, comes out only 1107 miles, and with the values $k=1\frac{2}{3}$ and $\sigma=1600$ feet only 1581 miles. This calculation of the depth, unlike the other results above, may be said to be independent of the question of the constitution of the sun's interior mass. It is alike difficult, on any plausible hypothesis, to reconcile a temperature no higher than 54000° Fah. with any perceptible atmosphere extending many thousand miles above, and yet no less an authority than Prof. Peirce has assigned a hundred thousand miles as the height of the solar atmosphere above the photosphere, at the same time, however, pointing out the enormous temperature which, under convective equilibrium, this would imply at the level of the photosphere. But all are not yet agreed that the appearances seen at such distances from the sun are proof of the existence of a true atmosphere there. It will be seen that the numbers I give above were obtained from a first hypothesis of an atmospheric limit 20,000 miles above the photosphere, but for the purpose of this paper it is of no consequence to repeat the calculation from a different limit.

It is, I believe, recognized on theoretical grounds that in an atmosphere containing a mixture of gases of unequal density the lighter gases might be expected to diffuse in greater proportion into the higher parts of the atmosphere and the heavier gases into the lower parts. But perhaps the supposed circulation which the emission of heat maintains within the photosphere must renew mixture at a rate sufficient to mask the rate which theory would assign for diffusion. I have not attempted a theoretic comparison between these two tendences. It will suffice here to repeat that the above numerical results, so far as they may be thought to give countenance to the theory in its mechanical aspect, require that the entire inner mass of the sun shall have, at a mean, (in the supposed state of dissociation), the very small atomic weight specified. We may notice in this connection the uniform proportion of oxygen and nitrogen gases in our atmosphere at the height of four miles or more at which the analysis has been made. Without having gone into a critical examination of the question, I suppose that at that height the proportion of oxygen which the theory of diffusive equilibrium would assign is notably diminished, and that it would be found that the circulation of the air is sufficiently active to mask the theoretic rate of diffusion.

The second hypothesis which might offer itself in an attempt to make the theory rational, but which a very little reflection is, I think, sufficient to set aside, is that which would modify Clausius' theory of the gases by assuming that in the sun's body the average length of the excursion made by each molecule between two consecutive collisions, becomes very short compared with the radius of the sphere of repulsion of molecule for molecule, and with the average distance of their centers at nearest approach. This way of harmonizing the actual volume of the sun with such a temperature as 54000° Fah. in the photosphere, and with the smallest density which we can credit in the photosphere, would involve the consequence that the existing density of almost the entire mass of the sun is very nearly uniform and at its maximum possible, or at all events that any further sensible amount of collapse must be productive of but a very small amount, comparatively, of renewed supplies of heat, for the obvious reason that this hyphothesis carries with it almost the entire neutralization of the force of gravity by the forces of molecular repulsion. In like manner it involves the consequence that in any such small contraction of the photosphere as can have taken place within the history of total eclipses, it is but a very small fraction of the sun's mass, near its surface, that can have taken part in the collapse to any thing like a proportionate extent. Hence it also extremely restricts the period during which we could suppose the sun to have existed under anything like its present visible magnitude in the past, consistently with the production in the way supposed of the supplies of heat it has been sending out. Another thing involved in this second hypothesis is the fact which Prof. Peirce has pointed out to the Academy, viz.: that the existing molecular repulsion in the sun's body would immensely exceed such as would be indicated by the modulus of elasticity of any form of matter known to us.

In conclusion, I do not mean to say that there is any *invincible logical exclusion* of any law of the action of gases different from what is specified or alluded to above. I only mean that, so far as I can see, any theory of heat which is based *simply* and *solely* upon *molecular attraction* and *repulsion* dependent on molecular distance alone, cannot in its application to the sun, escape from the conditions indicated in this paper. It is certainly not absurd to imagine heat to be an agent of some kind so constituted that it cannot be thus represented by the sole conditions of motion and of molecular attraction and repulsion, but yet so constituted that in its effects upon matter it follows the conditions of mechanical equivalency as defined by Joule. In fact, such exceptional cases as the expansion of water in freezing seem to favor such a view, though the range of that phenomenon is very limited. One way of forming a mechanical representation of such a constitution would be by associating molecular motion with the mechanical powers, either with or without molecular attraction or repulsion; the manner in which the imagined mechanical power (or link) attaches itself to the molecules which it connects—so as to make their motion determine their mutual approach or recession or change of relative direction—being dependent on the existing motions and other conditions in such a way as to produce the observed phenomena. The possibility of such a mechanical *representation* is sufficient to show that such a supposed *constitution* is not *logically excluded*, but to accept such a mechanical representation as a physical fact is quite another matter, and, as it seems to me, a very difficult one. Of course this difficulty does not present itself when we suppose that heat is not motion.

ART. X.—*On the mode of observing the coming Transits of Venus;* by SIMON NEWCOMB.*

Transits of Venus over the disk of the sun have more than any other celestial phenomena occupied the attention and called forth the energies of the astronomical world. In the last century they furnished the only means known of learning the distance of the sun with an approach to accuracy, and were therefore looked for with an interest corresponding to the importance of this element. Although other methods of arriving at this knowledge with about equal accuracy are now known, the rarity of the phenomenon in question insures for it an amount of attention which no other system of observation can command. As the rival method, that of observations of Mars at favorable times, requires, equally with this, the general coöperation of astronomers, the power of securing this coöperation does in itself give the transits of Venus an advantage they would not otherwise possess.

Although the next transit does not occur for four years, the preliminary arrangements for its observation are already being made by the governments and scientific organizations of Europe. It is not likely that our government will be backward in furnishing the means to enable its astronomers to take part in this work. The principal dangers are, I apprehened, those of setting out with insufficient preparation, with unmatured plans of observation, and without a good system of coöperation among the several parties. For this reason I beg leave to call the attention of the Academy to a discussion of the measures by which we may hope for an accurate result.

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* Read before the National Academy of Sciences, April 13, 1870.

In planning determinations of the solar parallax from the transits of Venus, it has until lately been the custom to depend entirely upon observations of the internal contact of the limbs of the sun and planet, as proposed by Halley. It is a little remarkable that while astronomical observations in general have attained a degree of accuracy wholly unthought of in the time of Halley, this particular observation has never been made with a precision at all approaching that which Halley believed that he himself had actually attained. In his paper he states that he was sure of the time of the internal contact of Mercury and and the sun within a second.* The latest observations of a transit of Mercury, made in November, 1868, are, as we shall presently see, uncertain by several seconds. It is also well known that the observations of the last transit of Venus, that of June, 1769, failed to fix the solar parallax with the certainty which was looked for, the result of the standard discussion being now known to be erroneous by one-thirtieth of its entire amount.

The discrepancies which have always been found in the class of observations referred to, when the results of different observers have been compared, have been generally attributed to the effect of irradiation. The phenomenon of irradiation presents itself in this form: When we view a bright body, projected upon a dark ground, the apparent contour of the bright body projects beyond its actual contour. It may be generalized as follows:—A lucid point, however viewed, presents itself to the sense, not as a mathematical point, but as a disc of appreciable extent, and, usually, of irregular outline. But, for our present purposes the form of the disc may be considered circular. Its outline is necessarily quite indefinite, and its magnitude increases with the brilliancy of the point. A bright body, being composed of an infinity of lucid points, its apparent enlargement is an evident result of this law.

The following diagrams show the effect of this law upon the time of internal contact of a planet with the disk of the sun. The planet being supposed to approach the solar disk, figure 1 shows the geometrical form of a portion of the apparent surface of the sun, or the phenomenon as it would be if there were no irradiation immediately before the moment of internal contact. Figure 2 shows the corresponding appearance immediately after the contact. To indicate the effect of irradiation, or to show the phenomenon as it will actually appear on the theory of irradiation, we have only to draw an infinity of minute circles, one from each point of the sun's disk visible around the planet.

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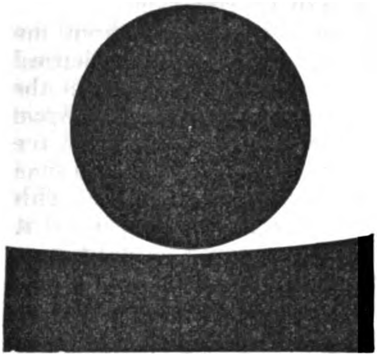
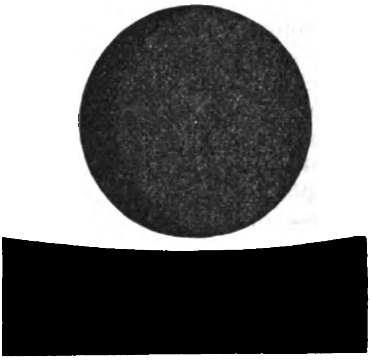
* Philosophical Transactions, No. 348, p. 454.

1.

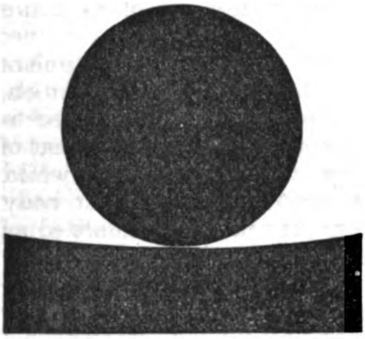


1*a*.

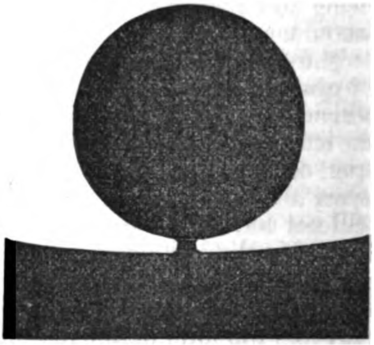


The effect of this is shown in the figures 1*a* and 2*a*. The exceedingly thin thread shown in figure 1 is thus thickened as in 1*a*, and the sharp cusps of figure 2 are rounded off as shown in figure 2*a*. The apparent radius of the planet is diminished by an amount equal to the radius of the circle of irradiation, and the radius of the sun is increased by the same amount. Comparing figures 1*a* and 2*a*, it will be seen that the moment of internal contact is marked by the apparent formation of a ligament, or "black drop," between the limbs of the sun and the planet. This formation is of so marked a character that it

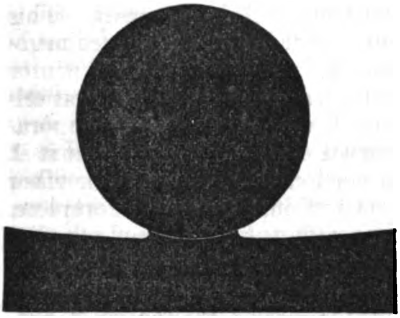
2.



2*a*.



3.



has been generally supposed there could be little doubt of the moment of its occurrence. The remarks of the observers have given color to this supposition, the black drop being generally described as appearing suddenly at a definite moment.

Examining figure 2*a*, it will be seen that the planet still appears entirely within the disk of the sun. The geometrical circle which bounds the latter, and that which bounds the planet, instead of touching, are separated by an amount equal to double the irradiation. If the observer waits until the planet has moved over this space, the two circles will appear in that position in which they would touch if completed. This phase commonly called "apparent contact," is shown in fig. 3. Neither circle is visible at the point of tangency, the black drop, now greatly widened and flattened, extending on both sides this point. The estimate of the moment of contact must therefore be very rough, the means of estimating being far less accurate than those afforded by a common filar micrometer. In the actual case the eye has to continue the two circles to the point of contact by estimation through a distance depending on the amount of irradiation, while measures with a micrometer are made by actual contact of a wire with a disk. Such estimates have, therefore, been generally rejected by investigators, not only from their necessary inaccuracy, but because the time of "apparent contact" depends upon the amount of irradiation, which varies with the observer and the telescope. If there is no irradiation at all, the time of apparent contact and that of true contact will be the same, as shown in figure 2; while, when the cusps are enlarged by irradiation, apparent contact will not occur until the planet has moved through a space equal to double the irradiation.

Let us return to the phenomena at actual contact. According to the theory as it has been presented, the formation or rupture of the black ligament connecting the dark body of Venus with the dark ground of the sky is a well-marked phenomenon, occurring at the moment of true internal contact. This was, I believe, the received theory until Wolf and André made their experiments on artificial transits in the autumn and winter of 1868 and 1869. They announced, as a result of these experiments, that the formation of the ligament was not cotemporaneous with the occurence of internal contact, but followed it at the ingress of the planet, and preceded it at egress. In other words, it appeared while the thread of light was still complete. They furthermore announced that with a good telescope the ligament did not appear at all, but the thread of light between Venus and the dark sky broke off by becoming indefinitely thin.

The result is not difficult to account for. Irradiation has already been described as a spreading of the light emitted from each point of the surface viewed, so that every such point appears as a small circle. The obvious effect of this spreading is a dilution of the light emitted by a luminous thread or point whenever the diameter of the thread is less than that of the circle of irradiation. In consequence of this dilution the thread may be invisible while it is really of sensible thickness, a given amount of light producing a greater effect on the eye the more it is concentrated. Since the thread of light must seem to break when it becomes invisible at its thinnest point, the formation or rupture of the thread marks, not the moment of actual contact, but the moment at which the thread of light becomes so thick as to be visible, or so thin as to be invisible. The greater the irradiation, and the worse the definition, the thicker will be the thread at this moment.

An interesting observation illustrative of this point was made by Liais at Rio Janeiro during the transit of Mercury of November 1, 1868. He had two telescopes, one much smaller than the other. He watched the planet in the small one till it seemed to touch the disk of the sun. Then looking into the large one he saw a thread of light distinctly between the planet and the sun, and they did not really touch until several seconds later.*

Reference to the figures will make it clear that there is no generic difference between the phenomenon commonly called the rupture of the black drop and that of the formation of the thread of light. If the bright cusps are much rounded, as in figure 2*a*, the appearance between them is necessarily that of a drop, while if they are seen in their true sharpness, as in figure 1, the form of the drop will not appear. It has been shown that with different instruments the phenomenon of contact may exhibit every gradation between these extremes. The only well defined phenomenon which all can see is the meeting of the bright cusps and the consequent formation of the thread of light at ingress and the rupture of the thread at egress.

To recapitulate our conclusions—

1. The moment of observed internal contact at ingress is that at which the thread of light between Venus and the sun becomes thick enough to be visible.

2. The least visible thickness varies with the observer and the instrument, and, perhaps, with the state of the atmosphere.

3. The apparent initial thickness of the thread varies with the irradiation of the telescope.

Two questions are now to be discussed. The observed times varying with the observer and the instrument, we must know how wide the variation may be. If it be wide enough to render uncertain the results of observation, we shall inquire how its injurious effects may be obviated.

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* Astronomische Nachrichten, B. 73, S. 209.

The first question can be decided only by comparison of the observations of different observers upon one and the same phenomenon. For such comparison I shall select the observations of the egress of Mercury on the occasion of its last transit over the disk of the sun. This selection is made for the reason that this egress was observed by a great number of experienced observers with the best instruments, while former transits, whether of Venus or Mercury, have been observed less extensively or at a time when practical astronomy was far from its present state of perfection, and that the transit in question would therefore furnish much better data of judging what we might expect in future observations. The comparison was made in the following way: I selected from the "Astronomische Nachrichten," the "Monthly Notices of the Royal Astronomical Society," and the "Comptes Rendus" all the observations of internal contact at egress which there was reason to believe related to the breaking of the thread of light, and which were made at stations of known longitude. Each observation was then reduced to Greenwich time and to the center of the earth.

The results are exhibited in the following table. The third column of the table shows the corrected reduced times of contact. Next is given the aperture of the telescope, and the magnifying power employed in the observation. Lastly, we present a classification of the phenomena of contact as described by the observers.

The letters V. C., (vanishing contact,) indicate that the thread of light broke by becoming indefinitely thin, so that there was no irradiation or distortion of the planet.

The letters B. D. (black drop) indicate the appearance of the ligament, as shown in fig. 2*a*.

R. R. Indicates that the time is that of the rupture of the thread, but that it cannot be inferred from the statements of the observer whether the thread had or had not a sensible thickness immediately before breaking.

A. C. Signifies an apparent contact, as shown in fig. 3. When there is no black drop the letters V. C. and A. C. have the same meaning. The letters are therefore employed only when there is reason to believe from the remarks of the observer that the cusps were rounded.

(Inst.) indicates that the observer described the phenomenon as instantaneous.

The letters *b* and *g* refer to the definition, the first indicating that it was bad, the second that it was good.

When there is no indication in this column the observer gives no information respecting the phenomenon.

Observer.	Place.	Reduced Time of Int. Contact.			Aperture.	Power.	Character of Phenomenon.
		h.	m.	s.			
Oppolzer,*	Vienna,	20	59	49·3	4	--	B. D. b.
Oppenheim,	Bonn,			51·8	--	--	b.
Rayet,	Paris,			57·0	5·5	222	V. C.
Le Verrier,†	Marseilles,			57·6	--	--	B. D. (Inst.) g.
Lynn,	Greenwich,			59·2	?	170	B. D.
P. J. Kaiser,	Leiden,			59·2	3	--	R. R. (?) b.
Wolff,	Bonn.,	21	0	1·3	--	--	b.
Liais,‡	Atalaïa,			1·5	--	78	V. C. (g.)
Rosen,	Pulkowa,			2·7	5	59	
Stone,§	Greenwich,			4·0	12¼	245	B. D.
Lais,	Rome,			4·6	2	40	R. R.
Kartazzi,	Pulkowa,			4·7	2·9	145	
Wagner,	"			4·7	3·7	148	
Nyren,	"			4·7	7·4	57	
Fuss,	"			4·7	2·4	63	
Dunkin,‖	Greenwich,			4·7	3·7	300	B. D.
André,	Paris,			4·9	5	188	V. C.
Argelander,	Bonn,			5·3	--	--	b.
Oppolzer,	Vienna,			6·3	4	--	A. C.
Dr. Peters,	Altona,			7·3	4	111	b.
Villarceau,	Paris,			8·3	7·3	163	V. C.
Lebedeff,	Pulkowa,			8·7	2·2	70	
Dollen,	"			8·7	2·2	81	
Miroschnit schenko,	"			8·7	3·7	106	
Leskinen,	"			8·7	3·9	117	
Mancini,¶	Rome,			9·5	6	60	R. R.
H. J. Carpenter,	Greenwich,			9·6	?	70	R. R.
Kasavinoff,	Pulkowa,			10·7	1·1	36	
Kaiser,	Leiden,			10·8	7		B. D. b.
Criswick,**	Greenwich,			11·3	4?	170	B. D.
Wolf,	Paris,			11·4	8	200	V. C.
Secchi,	Rome,			11·5,	7	200	R. R.
J. Carpenter,	Greenwich,			12·6	3·7	90	B. D. (Inst.)
Prof. Peters,	Altona,			13·3	3	145	b.
Weiss,	Vienna,			13·3	5	40	b.
Struve,	Pulkowa,			13·7	2·5	207	R. R.
Stephan,	Marseilles,			13·7	--	--	R. R. (Inst.)
Meriuo,	Madrid,			14·1	5	100	R. R. g.
Duner,	Lund,			14·2	9	320	V. C. g.
Stone,	Greenwich,			16·0	--	--	A. C.
Buckingham,	Walworth,			17·3	--	--	B. D.
Sokolov,	Pulkowa,			18·7	2·8	226	
Kam,	Leiden,			19·0	6	--	R. R. (?) b.

* "8^s vor dem Brechen des Lichtfadens schien Mercur gleichsam den Sonnenrand etwas hinauszuchieben so dass an der Stelle des Austrittes ein nach aussen convexer Lichtfaden den Mercur umschloss und dessen Verschwinden ich als den Moment der inneren Berührung notirte."

† Le Verrier used a seven-inch glass with a diaphragm over the objective.

‡ Three seconds before this time the contact was not completed, and three seconds later it was more than completed.

§ A fine ligament seen several seconds before the time recorded.

‖ The planet was suddenly noticed to assume a balloon or pear shape. The apparent contact about 10^s later.

¶ Observed by image in camera obscura, 22 c. m. in diameter.

** Instantaneous. Time perhaps 2^s or 3^s late.

TABLE—continued.

Observer.	Place.	Reduced Time of Int. Contact.	Aperture.	Power.	Character of Phenomenon.
		h. m. s.			
Ventosa,*	Madrid,	21·0	1·0	150	B. D. (Inst.)
Kaiser,	Leiden,	23·8	7	--	A. C. b.
Lindeman,	Pulkowa,	24·7	4·2	85	
Prince,	Uckfield,	28·5	--	--	A. C. (?)
Pohl,	Kahlenberg,	29·6	4	135	V. C.
Lynn,†	Greenwich,	31·1	?	170	A. C.
Penrose,	Wimbledon,	52·2	2·2	70	R. R.

In the preparation of this table, the original intention was to exclude all observations unaccompanied with any statement of the phenomena. An exception was afterward made in favor of the observations of Bonn and Pulkowa, so that in this respect there is a lack of homogeneousness in the table. The few observations of "apparent contact," have also been added, in the belief that they would not be devoid of interest.

The principal conclusion, to be drawn from the comparison here exhibited, is that there is no discoverable relation between the time of observation on the one hand, and the size of the telescope, the magnifying power, (so it exceed 50 or 60) or the character of the phenomenon on the other. We find the phenomenon of the apparently instantaneous formation of the "black drop" to range from 20^{h} 59^{m} 58^{s} (Le Verrier) to 21^{h} 0^{m} 21^{s}·0 (Ventosa). The times of "Vanishing Contact" on which Wolf and André lay so much stress, range from 20^{h} 59^{m} 57^{s} to 21^{h} 0^{m} 29^{s}. If we reject Pohl's observation, the range will still be 17^{s}. I think if the observations of external contact were collected and compared in the same way, it would be found that their agreement was as good as in the case of internal contact. So far as we have data for judging, these differences would seem to be due to the accidental errors of observation. Their amount in arc may be inferred from the fact that 15^{s} of time correspond to a change of 1″ in the relative position of Mercury and the sun.

I conceive, therefore, that we shall fail if we rely mainly on observations of internal contact. Still, there are two measures by which the reliableness of the determination of ingress and egress may be greatly increased. The first consists in having the observer occupy the entire time of partial ingress and egress in making very careful measures of the distance of cusps with such micrometer as may be best adapted for the purpose. The second consists in bringing the observers at opposite stations together, both before and after the transit, and causing them to make observations on artificial transits with the same instruments employed in observing the transit of Venus, in order to determine what correction should be applied to the observations of one to make them comparable with those of the other. It would be a comparatively simple operation to erect an artificial representation of the sun's disk at the distance of a few hundred yards, and to have an artificial planet moved over it by clockwork. The actual time of contact could be determined by electricity, and the relative positions of the planet and the disk by actual measurement. With this apparatus it would be easy to determine the personal errors to which each observer was liable, and these errors would approximately represent those of the observations of actual transit.

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* Contact seemed to be established simultaneously at several points.

† Doubtful. Limbs "certainly in contact by at least that time."

Still, it would be very unsafe to trust entirely to any determination of ingress or egress. Understanding the uncertainty of such determinations, the German astronomers have proposed to trust to measures with a heliometer, made while the planet is crossing the disk. The use of a sufficient number of heliometers would be both difficult and expensive, and I think we have an entirely satisfactory substitute in photography. Indeed, Mr. De la Rue has proposed to determine the moment of internal contact by photography. But the result would be subject to the same uncertainty which affects optical observations—the photograph which first shows contact will not be that taken when the thread of light between Venus and the sun's disk was first completed, but the first taken after it became thick enough to affect the plate, and this thickness is more variable and uncertain than the thickness necessary to affect the eye. We know very well that a haziness of the sky which very slightly diminishes the apparent brilliancy of the sun, will very materially cut off the actinic rays, and the photographic plate has not the power of adjustment which the eye has.

But, although we cannot determine contacts by photography, I conceive that we may thereby be able to measure the distance of the centers of Venus and the sun with great accuracy. Having a photograph of the sun with Venus on its disk, we can, with a suitable micrometer, fix the position of the center of each body with great precision. We can then measure the distance of the centers in inches with corresponding precision. All we then want is the value in arc of an inch on the photograph plate. This determination is not without difficulty. It will not do to trust the measured diameters of the images of the sun, because they are affected by irradiation, just as the optical image is. If the plates were nearly of the same size, and the ratio of the diameters of Venus and the sun the same in both plates, it would be safe to assume that they were equally affected by irradiation. But should any difference show itself, it would not be safe to assume that the light of the sun encroached equally upon the dark ground of Venus and upon the sky, because it is so much fainter near the border.

If the photographic telescope were furnished with clock-work, it would be advisable to take several photographs of the Pleiades, both before and after the transit, to furnish an accurate standard of comparison free from the danger of systematic error. There is little doubt that if the telescopes and operators practice together, either before or after the transit, data may be obtained for a satisfactory solution of the problem in question.

To attain the object of the present paper, it is not necessary to enter into details respecting choice of stations and plans of observation. I have endeavored to show that no valuable result is to be expected from hastily-organized and hurriedly-equipped expeditions; that every step in planning the observations requires careful consideration, and that in all the preparatory arrangements we should make haste very slowly. I make this presentation with the hope that the Academy will take such action in the matter as may seem proper and desirable.

ART. XI.—*On the Geology of Eastern New England;* by Dr. T. STERRY HUNT, F.R.S. (From a letter to Prof. JAMES D. DANA.

WHEN, more than twenty years since, my attention was turned to the geology of New England, there was no evidence of the existence between the old gneisses of the Adirondacks and the coal measures, of any other stratified rocks than those of the Huronian series, and the New York system, from the Potsdam formation, upward. It is true that Emmons had, before that time, maintained the presence, in western Vermont and Massachusetts, of a system of fossiliferous sediments, lying unconformably beneath the Potsdam, but the evidence up to this time adduced with regard to these so-called Taconic rocks, has failed to show that they include any strata more ancient than the Potsdam, while most of them are certainly younger. The researches of Sir William Logan, up to 1848, had led him to refer to a period not older than the Lower Silurian the crystalline sediments of the Appalachian region of Canada, between Lake Champlain and Quebec. These form a chain of hills, the continuation of the Green Mountains, and were found by him to be followed immediately, to the southeast, by more or less calcareous and somewhat altered strata, associated with Upper Silurian fossils, and succeeded, across the strike, near the sources of the Connecticut River, by a series, several miles in breadth, of micaceous schists and quartzose strata, occasionally containing chiastolite, garnet and hornblende. These two series of rocks, extending from the base of the Green Mountains to Canaan on the Connecticut, it was suggested by Sir William Logan, in his Report on the Geological Survey, 1847–1848, might be the altered representatives of the rocks of Gaspé, including the Lower Helderberg group, and the succeeding members of the New York system to the top of the Chemung. I then, as now, conceived that these micaceous and argillaceous schists, often holding garnets and chiastolite, were identical with those which make so conspicuous a figure in the White Mountains, and elsewhere in Eastern New England, and when, in 1849, I laid before the American Association at Cambridge, the results of the Geological Survey of Canada (this Jour., II, ix, 19), suggested that to the Gaspé series, as above defined, "may perhaps be referred, in part, the rocks of the White Mountains." Lesley, subsequently, in 1860 (Proc. Philad. Acad. Nat. Sciences, page 363), adduced many reasons for believing that the rocks of these mountains might be strata of Devonian age.* In the large geological map of Canada and the northern United States, lately published by Sir William Logan, no attempt is made to delineate the geology of New Hampshire, but the rocks in question, to the north of the United States boundary, are represented as Upper Silurian, with the exception of a belt of the Quebec group, which has been recognized in that region.

In fact, the schists and gneisses of the White Mountains are clearly distinct, lithologically, from the Laurentian, the Labradorian and the Huronian, as well as from the crystalline rocks of the Green Mountains, and from the fossiliferous Upper Silurian strata which lie at the southwestern base of the Canadian prolongation of the latter. Having thus exhausted the list of known sedimentary groups up to this horizon, it was evident that the crystalline strata of the White Mountains must be either (1) of Devonian age, or (2) something newer (which was highly improbable); or (3) must belong to a lower and hitherto unknown series. In the absence of any proof, at that time, of the existence of such a lower system, the first view, which referred these strata to the Devonian period, was the only one admissible.

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* In this connection should be recalled the views put forth in 1846, by Messrs. H. D. and W. B. Rogers, in a paper on the Geological Age of the White Mountains, (this Journal, II, i, 411). They there, for the first time, pointed out that the great mass of these mountains consists of more or less altered sedimentary strata, which, upon the evidence of supposed organic remains, they referred, with some little doubt, to the Clinton division of the Upper Silurian. In 1847, however, they announced that the supposed fossils, on which this identification had been founded, were not really such, (this Journal, II, v, 116). Future explorers may, it is hoped, be more successful, and yet discover among the strata of the White Mountains evidences of organic life, probably of primordial Silurian age.

When, however, further investigation showed that the great and progressive thickening which takes place in the paleozoic formations from the west, eastward, is not confined to the augmentations of existing subdivisions, but includes the intercalation of new ones; when the few hundred feet of typical Potsdam sandstone in New York are represented in Vermont, Quebec and Newfoundland, by thousands of feet of strata lithologically very unlike the type; while the Quebec group, not less in volume, appears representing the beds of passage between the Calciferous and Chazy divisions of New York, we begin to conceive that conditions of sedimentation, very unlike anything hitherto suspected in the west, prevailed to the eastward. When, moreover, we find widely separated areas of Labradorian and Huronian rocks,—remaining fragments of great series,—resting upon the Laurentian, from Lake Huron to Newfoundland, we get evidences of a process of denudation in past ages, not less remarkable than the sedimentation.

My observations of last year have led me to a conclusion, which had previously been taking shape in my mind, that there exists above the Laurentian, a great series of crystalline schists, including mica-slates, staurolite and chiastolite-schists, with quartzose and hornblendic rocks, and some limestones, the whole associated with great masses of fine-grained gneisses, the so-called granites of many parts of New England. The first suggestions of this were given me by the observation of Dr. Bigsby, confirmed by specimens since received from the region, that there exists to the northwest of Lake Superior, an extended series of crystalline schists, unlike the Laurentian, and resembling those of the White Mountains. I have already called attention to this resemblance in, a review of the progress of American Geology, in 1861 (this Jour., II, xxxi, 395). It was contrary to my notions of the geological history of the continent to suppose that rocks of Devonian age could, in that region, have assumed such lithological characters, and I was therefore led to compare these rocks with a great series of crystalline schists, abounding in mica-slates and micaceous limestones, which occupy considerable areas in the Laurentian region in Hastings county, to the north of Lake Ontario. The distribution of this series has been traced out by Mr. Vennor, who, in 1869, was able to show that, although much contorted, it rests unconformably upon the old Laurentian gneisses, while it is, at the same time, overlaid by the horizontal limestones of the Trenton group. This intermediate series, which attains a thickness of several thousand feet, is terminated by calcareo-micaceous schists, in which *Eozoon Canadense* has been found, both in Madoc and in Tudor. In these localities, as shown by Dawson and Carpenter (this Jour., II, xliv, 367), the calcareous skeleton of the Eozoon, instead of being injected by serpentine or another silicate, is simply filled with impure calcareous and carbonaceous matter. The presence of this fossil serves to connect these rocks with the Laurentian system, with which they had provisionally been classed, although their lithological dissimilarity had long been noticed, and in 1866 Sir William Logan had remarked their resemblance to the mica-slate series found near the sources of the Connecticut River (Report Geol. Survey, 1866, p. 93).

Mr. Alex. Murray's report of his explorations in Newfoundland, published in 1866, throws much light on the history of the rocks immediately succeeding the Laurentian in that region. He found in the great northern peninsula, about the Cloud Mountains and Canada Bay, not less than 5400 feet of strata, referred by him to the Potsdam group. Of these the lower 2500 feet consist of bluish-gray slates, holding near the summit, beds which become conglomerate from the presence of quartz pebbles, and are followed by a mass of purplish amygdaloidal diorite, holding epidote and jaspery red iron ore. Then follow 2000 feet of argillaceous and somewhat micaceous slates, with beds of quartzite and of limestone, generally impure. These contain, besides numerous fucoidal markings, the remains of a Lingula, and of *Olenellus Vermontanus*, a fossil characteristic of the Potsdam group. To this second division succeeds a third, consisting of about 900 feet additional of limestones and slates. Somewhat farther southward, at Great and Little Coney Arms, the lower half of the above series is not observed, but a succession of strata, supposed to represent the upper portion of the Potsdam, is more particularly described. It consists, at the base, of 300 feet of pale bluish-gray mica-slates, with iron stains, "softer, more finely laminated, and more uniform both in color and in texture" than some micaceous strata described by Mr. Murray as occurring in the Laurentian in that region. To these succeeded 430 feet of similar soft bluish-gray mica-slates, holding numerous thin seams of dark colored limestone, and followed by 1000 feet of impure limestones and slates, often micaceous and calcareous, among which are a few beds of white compact marble. No indications of fossils, save fucoidal markings, were met with in this section. At Coney-Arm Head there is seen a series of "whitish granitoid, very quartzose mica-slates," which appear to have a thickness of from 1500 to 2000 feet. The same rock is found in White Bay, where it overlies what is supposed to be Laurentian gneiss. The relations of these whitish granitic mica-slates are still obscure, but Mr. Murray was inclined to regard them as occupying a position beneath the Potsdam group. The latter, in Canada Bay, is immediately followed by the unaltered fossiliferous limestones and shales of the Quebec group. From these investigations of Mr. Murray we learn that between the Laurentian and the Quebec group, there exists a series of several thousand feet of strata, including soft bluish-grey mica-slates and micaceous limestones, belonging to the Potsdam group; besides a great mass of whitish granitoid mica-slates, whose relation to the Potsdam is still uncertain. To the whole of these we may perhaps give the provisional name of the Terranovan series, in allusion to the name Newfoundland.

Imperfect gneisses and micaceous schists are found in several parts of the province of New Brunswick, associated with what has been described as a great granitic belt. These rocks have been examined by Prof. Hind, and by Mr. Robb, on the St. John and Mirimichi rivers; and the former of these observers some years since pointed out the indigenous character of the so-called granites. In the summer of 1869 I had an opportunity of examining, with Prof. L. W. Bailey, the region about St. Stephen, on the river St. Croix, where he had already observed a series of ferruginous quartzites and imperfect gneisses, accompanied by soft bluish mica-slates sometimes holding chiastolite, staurolite, and garnet. These highly crystalline schists are not more than five miles removed from unaltered shales of the Gaspé series, containing fossils of Upper Silurian or Lower Devonian types, and rest unconformably upon older granitoid rocks, which Prof. Bailey regards as probably Laurentian. We subsequently examined the crystalline schists of the St. John, which are apparently identical with those of the St. Croix, and these also overlie, unconformably, an older granitoid gneiss.

More recently Prof. Hind has pointed out that some of the so-called granites of Nova Scotia are ancient gneisses, probably of Laurentian age, and have shown that between these and the gold-bearing slates of that province, there is found, near Windsor, and near Sherbrooke, a series of beds of no great thickness, consisting of imperfect gneisses, quartzites and micaceous schists, which rest unconformably on the Laurentian, and are sometimes wanting altogether. These include mica-schists with chiastolite and garnet, and appear identical with those already observed by Dr. Dawson in other parts of Nova Scotia, which I had already recognized as the same with those of the White Mountains, and those of the St. Croix, just noticed. Prof. Hind, in a late paper, has called these, from their position in Nova Scotia, Huronian; but the Cambrian or Huronian rocks recognized by Messrs. Matthew and Bailey in New Brunswick, where they are widely spread along the north side of the Bay of Fundy, consist of massive diorites and quartzose feldspar-porphyries, with occasional sandstones and conglomerates, and are very unlike the gneissic and micaceous rocks in question, which I believe to belong, like those of the St. Croix and the St. John rivers, to the great Terranovan series. The micaceous and hornblendic schists, with interstratified fine grained whitish gneisses (locally known as granites) which I have seen in Hallowell, Augusta, Brunswick and Westbrook, in Maine, appear to belong to the same series; which will also probably include much of the gneiss and mica-schist of Eastern New England. If this upper series is to be identified with the crystalline schists which, in Hastings County, Ontario, overlie, unconformably, the Laurentian, and yet contain *Eozoon Canadense*, the presence of this fossil can no longer serve to identify the Laurentian system. To this lower horizon however, I have referred a belt of gneissic rocks in Eastern Massachusetts, which are lithologically unlike the present series, and identical with the Laurentian of New York and Canada. To the upper series appear to belong the great endogenous granitic veins so well known to mineralogists as containing beryl, tourmaline and other fine crystallized minerals.

The fine-grained, white granitoid gneisses, often present an apparently bedded structure, which enables them to be removed in large plates or layers, lying at no great angle, and apparently conformable to the present surface of the country. This structure, which I conceive to have been superinduced by superficial changes of temperature, is often quite independent of the bedding, as may be seen in the quarries near Augusta in Maine, and in the cuttings on the Grand-Trunk Railway near Berlin Falls, New Hampshire. It is also observed in exotic or intrusive granites, like those of Biddeford, Maine. This is, in fact, the concentric lamination of granite, long since observed by Von Buch, and, I believe, correctly explained by Prof. N. S. Shaler to be due to movements of contraction and expansion in the mass, caused by variation of temperature during the changes of the seasons. He has not however observed this structure at greater depths than from three to five feet, while in some rocks I have found it penetrating probably twenty feet. (See Shaler's paper, read before the Boston Nat. History Society, Feb. 3, 1869, and published in the Proceedings of the Society, vol. xii, page 289).

While however I admit the existence in the Dominion of Canada and in Eastern New England, of a great series of crystalline schists, distinct from the Laurentian, and apparently the same with those found by Mr. Murray between the Laurentian and the Quebec group in Newfoundland, it is not less certain that we have in these regions rocks of Upper Silurian and Lower Devonian age, holding characteristic fossils. These strata in Maine and New Brunswick are generally but little altered. In the Connecticut valley at Bernardston, Massachusetts, near Lake Memphremagog in Vermont, and further northward in the province of Quebec, fossils of this horizon are found in rocks which, in some localities, are more or less altered and crystalline. I believe however that much of the calcareous mica-slate of Eastern Vermont will be found to belong to the Terranovan series. The extent of these newer rocks, and the limits between them and the more ancient schists, of the ruins of which they are probably in part composed, remain problems for farther investigation. For the solution of these, Prof. C. H. Hitchcock, by his labors in Vermont, is already well prepared, and it cannot be doubted that he, with his able assistants, will in the Survey of New Hampshire, now in progress, throw much light on New England geology. It is worthy of remark, that strata holding fossils of Lower Helderberg age, or thereabouts, are not confined to the shores of Maine and New Brunswick, and the valleys of the Connecticut and St. John rivers, but are found beyond the Green Mountains, in the valley of the St. Lawrence, near Montreal; where, on the island of St. Helen, they rest unconformably on the Utica slate, and at Belœil Mountain, near by, on intrusive diorites, which there break through the shales of the Hudson River group.

The relations of this Terranovan series to the porphyries and diorite rocks which, in New Brunswick, have been called Cambrian and Huronian by Mr. Matthew (first distinguished by him as the Coldbrook group), yet remains to be determined. These rocks are found near to the city of St. John resting directly on what has been regarded as Laurentian, and are overlaid by the uncrystalline schists which contain the primordial fauna now so well known by the descriptions of Prof. Hartt. Rocks which I regard as identical with this same Coldbrook or Cambrian group, are found along the coast of New Brunswick, and constitute the diorites and porphyries of Eastport, Maine. They appear moreover to be the same with those met with near Newburyport, and at Salem, Lynn, and Marblehead, Massachusetts. Farther researches about Passamaquoddy Bay, where the mica-slates are found not far removed from these porphyries, will probably enable us to determine their relations to each other.

It will be remembered that Gümbel has found, in Bavaria, beneath the oldest fossiliferous clay-slates, a mica-schist (and hornblende-schist) series, reposing upon the Hercynian gneiss, which contains crystalline limestones, with graphite, serpentine and *Eozoon Canadense*, and which he has identified with the Laurentian of North America. He distinguishes beneath this a great mass of red gneiss, apparently without limestones, to which he has given the name of the Bojian gneiss. It will however be remembered, that in his studies of the Laurentian system on the

Ottawa, Sir William Logan has shown that this immense series (his Lower Laurentian), some 20,000 feet in thickness, includes four great masses of gneiss and quartzite, divided by three limestone formations, and that it is in the uppermost of these, which is, in some parts, 1500 feet thick, that the *Eozoon Canadense* has been found. Some of the lower gneisses of this vast system may very well represent the Bojian of Gümbel, who has not recognized in Bavaria either the Labradorian (Upper Laurentian) or Huronian series. (See Gümbel on the Laurentian of Bavaria, translated and published in the Canadian Naturalist for December, 1866). Comparative studies of this kind should not be neglected in the investigation of our American rocks.

Montreal, May 10, 1870.

ART. XII.—*Mineralogical Contributions;* by CHARLES UPHAM SHEPARD, Sr.

1. *A new variety (species?) of Columbite.*

THE Columbites of New England deserve a closer examination than they have yet received. Those of New Hampshire, Massachusetts and Connecticut may embrace at least two, perhaps three, mineralogical species. A new locality, recently opened at Haddam (Conn.), has certainly disclosed a variety so different from the productions of the old one on the farm of Mr. Brainard, as to have led to doubts among collectors whether it fairly belongs to the Columbite group.

For my first knowledge of the new variety, I am indebted to several of my pupils who visited Haddam during the past year; and particularly to Mr. Charles H. Ames of the Senior class in Amherst College, who has furnished me with several dozen specimens, mostly isolated crystals, from one ounce in weight, downward, to that of a few grams. They all occur at a single repository, contiguous to the center of the village, and directly in rear of the house of Mr. Nathaniel Cook, the well known collector and dealer in Haddam minerals. The locality was opened by explorers for porcelain stone; but the feldspar proving too ferruginous, being of a somewhat flesh-red color, has led to an abandonment of the enterprise. Although this discovery is a recent one, it is not unlikely that specimens from the same spot had previously found their way into cabinets; and may thus have led to some of the discrepancies existing in the various descriptions of the Connecticut columbites.

The general aspect of the crystals is rather peculiar. Instead of flattened prisms, they are nearly all square and often, through imperfection, decidedly rhombic. With the common goniometer, many of them give angles nearly as oblique as pyroxene. Nevertheless, when more carefully studied by means of the reflective goniometer, accompanied by observations on the faces replacing the lateral prismatic edges, no essential distinction of form can be made out between these crystals and those of columbite or of tantalite. They are all strictly isomorphous. An unimportant, though striking peculiarity is observed in these crystals, proceeding from the unusual development of the octahedral faces, whereby they possess a four-sided pyramidal summit at one, or each extremity. Very rarely are traces of the terminal plane visible; and when seen, the eye or the common goniometer would always report it as slightly wanting in verticality. Its smoothness is insufficient for measuring its position with the reflective goniometer.

The faces of the crystals are wanting in that high luster so frequent in those from other localities. In luster, color and even in general shape, they resemble on first inspection, the average specimens of Elba yenites nearly as much as they do the ordinary Haddam columbite,—the chief exception being, that the narrow faces about the prismatic edges are bright.

No pavonine tints are seen in the present variety. Their absence is so marked, not only on natural planes but also on fractured surfaces, as to afford a ready and almost sure criterion for distinguishing specimens of this locality from those obtained at the old one of Brainards, where every crystal and almost every fragment displays the blue or yellowish tarnish.

The color of the fracture is perfectly black approaching pitchy. It is more firm, and less prone to crumble than the ordinary columbite. The fracture is occasionally small conchoidal. No distinction in hardness can be made out. It is nevertheless more easily ground to a fine powder than the usual mineral; and, when perfectly powdered, presents an almost black color, while columbite is only brownish black. This distinction is so marked, as to be recognized by candle light.

But the most remarkable distinction resides in its lower specific gravity, which may be said to be strictly uniform. Eight examples (varying in weight from 2 to 16 grams) gave 5·31 as the average. Three of these gave respectively, 5·32, 5·34 and 5·35; while four specimens of columbite from the Brainard locality gave 6·02, 6·03, 6·10 and 6·19,—the average being 6·085.

Its powder, strongly heated in an open platinum crucible, lost only ·02 p. c. in weight, but changed in color, by several shades, to a brownish black.

Alone before the blow-pipe, small fragments had their edges decidedly rounded by fusion. Treated in powder in small quantity, in the outer flame with borax on platinum wire, a clear, pale yellow globule (while hot) with faint tinge of brown, was obtained, which on cooling became almost colorless. In the interior flame, no change took place, except that of greater paleness in the globules. The addition of nitre produced the reaction for manganese.

The columbite powder from Brainards' gave similar results, with the difference, of deeper tints to the glass.

With microcosmic salt, the behavior was similar, excepting when the mineral was added in excess; a brownish red globule was then afforded.

Heated with carbonate of soda on charcoal, it yielded, in common with the columbite, minute granules of tin.

It is feebly, if at all, attacked by hydrochloric acid; but is perfectly decomposed by heated sulphuric acid. The action is evinced almost immediately,—the mixture becoming first yellowish, then greenish yellow, and finally deep yellow, which by the affusion of water turns white. The Haddam columbite, on the other hand, is only imperfectly decomposed by the same treatment. Both minerals, however, are speedily decomposed by fusion with bisulphate of potash,* but with a marked difference in the aspect of the fused mass; the new variety yields a honey yellow, transparent glass, while the columbite gives one which is grayish white and opaque.

A portion of the white insoluble residue (after decomposition of mineral with sulphuric acid) on being treated on tinfoil with hydrochloric acid, instantly produced a deep Prussian blue liquid and precipitate. On dilution with water, the color grew paler; and, in half an hour, even the sediment had nearly changed to a grayish blue.

Another portion of the above moist powder was treated with dilute sulphuric acid and a strip of zinc. The metal was instantly coated by a dark blue precipitate, which after two hours also changed to ash-gray.

The original sulphuric acid solution, slightly diluted with water, after separation from the insoluble white precipitate, was boiled; whereupon a considerable white precipitate of the metallic acid, (or acids,) was thrown down. This on being heated was of a faint yellow color, and nearly white after cooling. It tinged borax pale citron yellow while hot, but left it colorless on cooling; and gave similar results with microcosmic salt.

A portion of the metallic acids was fused with caustic soda in a silver crucible. The mass obtained was dissolved in water; and through the solution a stream of carbonic acid was conducted to complete saturation, producing no precipitate, whereby the absence of tantalic acid was inferred.*

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* This easy decomposition, which is the most striking in the new variety, points also to the comparative absence of tantalic acid.

Analysis, the decomposition being effected by fusion with potash, gave the composition as follows:

Metallic acids	78·30
Protoxyd of Iron	13·86
Protoxyd of Manganese	7·72
Tin only in traces.	
	100·28

In two processes for the metallic acids, one by the decomposition of the mineral by sulphuric acid, the other by potash, an exact agreement in specific gravity was found, viz.: 4·06; from which, perhaps, it may be just also to infer the absence of tantalic acid, whose density when pure, though varying somewhat according to its mode of preparation, averages fully one-third higher than the present result.

Hermann distinguishes three varieties of columbite, viz.: (1) with gravity above 5·9, (2) with gravity 5·5 to 5·9, and (3) with gravity 5·4; but it is very doubtful whether an example of the mineral under notice has ever fallen into his hands, as this locality is of recent discovery. In conclusion, it may be added that minute coatings of uranochre were observed in patches and specks upon a few of the crystals; though no evidence of the presence of uranium was afforded in the mineral itself.

2. *Unknown mineral (microlite?) in Haddam columbite.*

This mineral I suspect to be new. It has never fallen under my notice until found among the productions of a recent blasting of the columbite at the Brainard locality. It occurs very sparingly (for the most part requiring the use of the microscope for its detection), occupying the sides of open crevices in large crystals of columbite, and also between the surfaces of these crystals and feldspar. At first, I was led to suspect them to be cassiterite; but blow-pipe examination with soda on charcoal only afforded minute traces of tin. The high adamantine lustre next suggested zircon and tungsten. The color was intermediate between that of wöhlerite and monazite; but a closer examination showed a wide divergence in other properties from either of these species.

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* The following reactions were obtained in testing a solution of the above compound of soda with the metallic acid; with protonitrate of mercury, a curdy, dense precipitate of a pale yellowish white, which after fifteen minutes assumed a green tint; with corrosive sublimate, no precipitate, but by slight evaporation, it thickened, and by drying, still farther, a red-brown oxychlorid of mercury was deposited; sulphate of soda gave no precipitate, but sulphate of potash a feeble one after a little standing; ferrocyanid of potassium, no change at first, but a pale yellowish one afterwards; phosphoric acid rendered the solution feebly opaline at the end of two hours; cyanid of potassium produced immediately a thick, white precipitate; infusion of nut-galls no effect until a drop of hydrochloric acid was added, when a thick, orange-red precipitate appeared; nitric acid gave an abundant precipitate that did not dissolve by heating; hydrochloric acidproduced a voluminous precipitate; a portion of the solution was evaporated to ⅛th its bulk, whereby a portion of the salt was thrown down, which was redissolved, with exception of a faint opalinity in the liquid; nitrate of silver added to this last solution produced an abundant pale yellow precipitate that was redissolved by boiling, a few drops of ammonia changed its color to brown, a larger quantity dissolved it entirely, and by drying in porcelain, it became first brown, than black. The foregoing reactions are those heretofore supposed to belong to the soda salts of niobium, or columbium; but it is probable that they may be shared to some extent by those of the allied metal, ilmenium.

Apparently equilateral triangular planes were detected; and finally portions of both pyramids, which leave little doubt that the form is that of the regular octahedron, especially as two of the solid angles were each similarly replaced by four planes, resting on the octahedral faces. To verify this conjecture of a cubic primary, will require the breaking up of the only specimens I possess of the crystals, which for the present I defer in the hope of obtaining other examples. The hardness is 5· to 5·5. Brittle, fracture conchoidal, semi-transparent to translucent. Luster of fracture, resinous; that of surface of crystals, adamantine. Streak yellowish white.

Before the blow-pipe, infusible. After long heating, it loses its brown tint, and while still hot, assumes a feeble citron yellow color, which becomes paler on cooling, when it is seen to have lost much of its luster, and is also less translucent. With microcosmic salt in the outer flame, it slowly dissolves into a perfectly colorless, transparent glass; but in the interior flame, it becomes, while warm, slightly milky with a tinge of blue. An intermittent flame produced the same transparency and tinge of blue. Long continued heating produced no trace of brown, violet, amethyst or yellow. The addition of tin gave a bluish white semi-opaque pearl.

It approaches most nearly to the microlite, but differs from it in the absence of all cleavages as well as in luster and in its behavior before the blow-pipe. Its mode of occurrence and properties lead to the suspicion of its being chiefly, if not wholly, composed of one of the metallic acids of the columbium group. The microlite, though occurring within a few yards of the columbite at Chesterfield and in the same ledge of granite, was never seen associated with this species, but invariably presents itself either in the albite or crystals of tourmaline, while the present mineral at Haddam occurs solely in, or upon, the columbite.

3. *New locality of Bismuthine and Bismutite in Haddam.*

About thirty years since I pointed out the existence of both these species and a third bismuthic mineral, the bismucone, as existing together in small quantity, associated with chrysoberyl, yellow beryl, columbite, garnet and a zircon mineral which I afterwards called calyptolite. Mr. Ames has lately procured for me large crystalline masses of bismuthine, more or less coated by bismutite, from a quarryman who had discovered them on the iolite hill, situated about one mile to the southwest of the meeting-house. They had apparently formed a narrow seam in the common orthoclase granite, which had been opened with a view to working as a porcelain-feldspar quarry. One of the specimens weighs about half a pound; and constitutes a deeply striated or channeled crystal. The mineral was supposed to be molybdenite. Examined before the blow-pipe, it affords no indications either of tellurium or selenium.

4. *On the metallic acid in Microlite.*

It having been suggested by Prof. Brush, from blow-pipe experiments[*] and the higher percentage of the metallic acid in this mineral, that it is the tantalic instead of the columbic, I made the following experiments upon two small crystals whose weight together was 0·350 gram. It was decomposed by fusion with bisulphate of potash, cold water dissolved the mass, which afterwards deposited a white granular precipitate, the clear supernatant liquid not showing even an opaline tendency on ebulition. The white precipitate after ignition gave 72·80 p. c. for the metallic acid. It evinced scarcely the slightest yellow tint when hot; and when cold, was perfectly white.

A portion of it gave with microcosmic salt, a perfectly clear glass, which showed only a faint yellowish tinge while hot. The addition of sulphate of iron produced a blood red tint when heated in the inner flame; on cooling, it changed to dirty yellow. With borax, it dissolves almost without color, being only a faint greenish yellow; but the globule inclines to opacity. No tinge of violet or pink is visible.

Fused with soda, the metallic acid afforded a bluish gray mass. The excess of alkali being removed by cold water, the addition of more water took up the residuum. A portion of this solution on being acidulated with hydrochloric acid, and afterwards treated with solution of nutgalls, gave a very yellow orange precipitate, quite different from that produced by columbic acid, which when contrasted therewith was seen to have a deep reddish tint[†] as compared with columbic acid.

The chief portion of the sodic solution was then supersaturated with carbonic acid, and gave rise to a decided precipitate. From the foregoing results, it would seem little doubt remains that microlite is a tantalate rather than a columbate of lime.

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[*] Dana's Mineralogy, p. 514.

[†] There was a want of correspondence also in the reactions of the metallic acid and soda compound, with nitrate of silver, nitrate of mercury, corrosive sublimate and ferrocyanid of potassium.

5. *Redondite.*

I have examined still farther the hydrous phosphate of alumina and iron described by me in a previous number of this Journal (vol. xlvii, p. 338), as occurring so abundantly in the island of Redonda, W. I., and am now of the opinion that it constitutes a species distinct from barrandite, from which it differs in several particulars, and essentially in specific gravity. Barrandite has gr.=2·576. Redondite gives gr.=2·019, which may be presumed to be a little too high, as the three examples used in the determination were found on analysis to give 8·8 p. c. of silica.

The phosphoric acid was determined on the same specimens by the molybdate of ammonia process; and amounted to 40·192 p. c. But as the silica (8·8 p. c.) was obviously accidental, the proportion of phosphoric acid in the pure mineral will stand at 44·07 p. c. The water is 24·73 p. c.

6. *Phosphoric acid in the Diaspore of Chester, Mass.*

Heermann having found phosphoric acid in the diaspores of several localities, I thought it worth while to analyze a good crystal of this mineral from Chester. It was semi-transparent and of a rich hair-brown color with a faint tinge of violet. Sp. gr.=3·343. The phosphoric acid was determined by the molybdate of ammonia. I obtained

Water,	15·80
Phosphoric acid,	0·32
Protoxyds iron, with traces of manganese	0·38
Alumina not determined; but by difference	83·50
	100·00

7. *The Pelham Vermiculite?*

It occurred to me as possibly interesting to make some chemical examination of this rather curious exfoliating mica, described by Mr. Adams in a late number of this journal (vol. xlix, p. 271). It is very abundant, and to the eye apparently as homogeneous as other softened micas.

It loses on an average 7 p. c. of water by ignition. By digestion in hot aqua regia, half its weight comes into solution, leaving behind 50 p. c. of minute, colorless scales, closely resembling margaric acid. They are so remarkable for their uniformity of size, freedom from color, and pearly luster, as scarcely to suggest their inorganic composition. Under the microscope, however, they very nearly give the distinct lateral cleavage lines of mica; nor have they the perfect elasticity of a true mica. They show no tendency to farther exfoliation when heated; before the blow-pipe they melt with difficulty on the edges into a colorless glass. Prof. DesCloizeaux was kind enough to examine some of these scales optically; and found them to be uniaxial, from whence it is probable that they belong to the species biotite.

The solution in aqua regia afforded the following result in reference to 100 parts of the dissolved material.

Alumina	14·00
Magnesia	13·68
Peroxyd of iron	32·00
Silica	24·00

No search was made for alkalies; but from the result obtained it is apparent that the magnesia in part explains the use made by the farmers of this material for a fertilizer, as described by Mr. Adams.

The point, however, which most interested me was the discovery thus accidentally made of the cause of the worm-like exfoliation in the mineral, viz: from the coating of the mica scales with a hydrated, argillaceous mixture, which probably owes its origin to the decomposition of some other species of the micaceous or chloritic family. In subjecting a fragment of the vermiculite of Millbury, Mass., to a similar action of aqua regia, the result was an abundance of brilliant green scales, probably belonging to the species ripidolite. It may therefore be suggested, that many earthy species of minerals (silicates) will under analogous treatment be found to be less homogeneous than has been supposed.

Amherst, May 7, 1870.

ART. XIII.—*Notice of a new Species of Gavial from the Eocene of New Jersey*; by Professor O. C. MARSH, of Yale College.

THE Museum of Yale College has recently received some interesting reptilian remains from the Eocene greensand of New Jersey, which indicate a new species of Gavial, considerably smaller than any Crocodilian heretofore discovered. These specimens, which were found together, and are evidently parts of the same skeleton, consist of various fragments of the skull, and ten vertebræ. The coössification of the neural arches of the vertebræ, and the almost entire obliteration in some of them of the sutures, would imply that the individual, although diminutive, was nearly or quite mature.

The few portions of the skull preserved are sufficiently characteristic to show that the animal had an elongated muzzle, and that the upper and posterior parts of the head were of the gavial type. The form of the parietal bone indicates, moreover, that the temporal apertures were large, and near together, with their adjoining sides nearly vertical. The quadrates are elongate, unusually straight on the inner side, and their condylar surfaces deeply and obliquely notched. The pneumatic foramen, on the upper surface of the quadrate near the inner edge, is very large, and characteristic. The teeth of this specimen were unfortunately not secured.

The vertebræ are well preserved, and present marked characters. The articular cup is transversely oval in the cervicals and anterior dorsals, and has its upper margin depressed in the posterior dorsals. The hypapophyses are simple and elongate. The neural canal of the cervical and anterior dorsal series is transverse, and sub-rectangular in outline, and the floor unusually broad and flat. In the posterior dorsals the canal, although still transverse, becomes less rectangular, with the broader portion above.

The principal dimensions of the tenth, or first dorsal, vertebra are as follows:—

Length of centrum, - - - - -	15·	lines.
Transverse diameter of cup, - - - -	8·75	“
Vertical diameter of cup, - - - -	7·	“
Width of neural canal, in front, - - -	6·	“
Height of neural canal, in front, - -	3·50	“

The species here described may perhaps prove to be generically identical with the one named by the writer *Thecachampsa Squankensis*, which is the only Crocodilian hitherto found in the Eocene of New Jersey. The genus *Thecachampsa*, however, as proposed by Prof. Cope, cannot yet be regarded as a valid one, since the concentric structure of the dentine, on which it was founded,* is not a character of generic importance; for it occurs in various other Crocodilians, and also in some of the Cetacea. The present remains, therefore, may be placed provisionally in the genus *Gavialis*, and the species be called *Gavialis minor*. It may readily be distinguished from *Thecachampsa Squankensis*, aside from its greatly inferior size, by the posterior dorsal vertebræ, which have the cup and neural canal transverse, instead of vertical, and especially by having the bodies of these vertebræ proportionally more elongate, and without the remarkable constriction, which is one of the most striking characters of the latter species.

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* Proceedings Philadelphia Acad. Nat. Sciences, 1867, p. 143.

The present specimens indicate that the animal to which they belonged was quite slender, and about six feet in length. They were found by Hugh Hurley, Esq., in the Eocene greensand of Shark River, Monmouth county, New Jersey, and by him presented to the Museum of Yale College.

New Haven, Conn., June 10th, 1870.

ART. XIV.—*On Hydrogenium-amalgam;* by O. LOEW, of the College of the City of New York.*

WHEN zinc-amalgam is shaken with water, a slow decomposition of the latter takes place, recognized by the formation of flocculi of hydrated oxyd of zinc, and the evolution of small bubbles of hydrogen on allowing the mixture to stand for a time. This decomposition of water by zinc is intensified when a small quantity of bichlorid of platinum is present; a spongy body then being formed on the surface of the zinc-amalgam. This body I have found to be an alloy of hydrogenium and mercury.

In order to obtain the hydrogenium-amalgam on a larger scale, zinc-amalgam containing a few per cents of zinc† is shaken thoroughly with about an equal volume of a solution of bichlorid of platinum containing about 10 per cent of the chlorid, care being taken to keep the mixture cool. The zinc-amalgam swells up considerably, precisely as in the ammonium-amalgam experiment, and continues to evolve hydrogen till the decomposition of the amalgam is complete. I have found that the volume of the hydrogen thus developed was, in several experiments from 100 to 150 times that of the mercury employed. This hydrogen possesses a faint but peculiar odor.

When this amalgam of hydrogenium (prepared with about 5 per cent of zinc) is pressed directly after its preparation, between sheets of filtering paper and then spread out in a layer (not too thin) to the air, the temperature soon rises considerably, and vapors of water are formed, which may be condensed in a glass receiver. The finely divided platinum present is obviously the cause of this rapid oxydation of the hydrogenium. In this action of platinic chlorid upon zinc-amalgam oxychlorid of zinc is at the same time formed; and though this may be removed by means of hydrochloric acid, yet by this treatment a part of the hydrogenium-amalgam is destroyed. If, after this, it be washed with water, it undergoes a very slow decomposition, the volume increases and bubbles of hydrogen escape through the water above. An addition of zinc-amalgam or sodium-amalgam greatly accelerates the decomposition of the hydrogenium-amalgam.

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* Read before the Lyceum of Natural History, Chemical Section, Apr. 11.

† A zinc-amalgam containing more than 5 per cent of zinc is a solid, and in this state, is not well fitted for use. though it would doubtless produce more hydrogenium in the amalgam. A zinc-amalgam of 5 per cent is readily liquified at a moderate heat and should then be immediately used.

Platinum after perfect amalgamation does not act as energetically as in its nascent state; i. e., when precipitated on the amalgam. When platinum-amalgam is mixed with zinc-amalgam the decomposition of the water by zinc is extremely slow and the hydrogenium-amalgam does not appear for some time. Under certain conditions moreover, the hydrogenium-amalgam is formed without the aid of platinic chlorid. I had at one time about twenty pounds of mercury containing zinc, which was left standing in a bottle with water for three weeks; the hydrogenium-amalgam formed on the surface of the mercury, gradually decomposing above and being renewed from below.

Graham compares hydrogenium to the active modification of oxygen, and no doubt there is much analogy between them. We may distinguish three modifications of hydrogen; namely, (1) common *nascent* hydrogen formed by the action of sodium-amalgam on water, or by that of zinc and hydrochloric acid; it has a strong reducing power but cannot form hydrogenium-amalgam; (2) common gaseous hydrogen which has at common temperatures only a very weak reducing power; and (3) hydrogenium. There are reasons for believing that these differences may be expressed by the following formulæ:

$[O]$	$[OO]$	$[OO]O$
Antozone α oxygen.	Common oxygen β oxygen.	Ozone γ oxygen.
$[H]$	$[HH]$	$[HH]H$
Common nascent or α hydrogen.	Common gaseous or β hydrogen.	Hydrogenium γ hydrogen.

ART. XV.—*The Brachiopoda, a Division of Annelida*;* by EDWARD S. MORSE.

AT a meeting of the Boston Society of Natural History, June 1st, 1870, Mr. Edward S. Morse made a verbal communication on the position of the Brachiopoda in the animal kingdom. He referred to the branch of Mollusca as it was understood forty years ago, when misled by external characters, many worms, like *Serpula* and *Spirorbis*, and a group of Crustaceans, the Cirripedia, were included with mollusks, and that from a proper recognition of their characters these diverse forms had been eliminated from time to time, and referred to their proper branches. After long and careful study Mr. Morse was prepared to state that the Brachiopods were true Articulates, and not Mollusks, and that their proper place was among the worms, forming a group near the tubicolous Annelids.

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* Abstract from Proceedings Boston Society Natural History.

He stated that for the past year he had been deeply engaged in the study of the Brachiopoda, and more particularly their early stages. Beside material from the coast of New England, he had had, through the kindness of Prof. A. E. Verrill, a large lot of *Discina* from Callao, Peru, belonging to the Yale College Museum. From these he had studied their early stages, but as he had in preparation a memoir upon the subject, he would now confine himself to the considerations that follow.

He first spoke of the structure and composition of the Brachiopod shell, and pointed out the relations between the cœcal prolongations of the mantle in *Terebratula* and a similar structure in the test of Crustacea. He had also noticed a marked resemblance between the polygonal cells in the shell of a young *Discina*, and a similar feature in certain lower Crustaceans. The scale-like structure of the test of *Idotaea*, resembled the scale-like structure of *Lingula*. The skin of *Nereis* had similar punctures or dots, as seen in *Terebratula* and also in the peduncle of *Lingula*. He had submitted the shell of *Discina* to chemical tests and believed it to be chitinous. Gratiolet had already given the chemical analysis of *Lingula anatina* and found forty two per cent of phosphate of lime, and only six per cent of carbonate of lime. The position of the valves of all Brachiopoda were dorsal and ventral, and this was a strong articulate character to be compared to the dorsal and ventral plates of the Articulates. The horny setæ that fringe the mantle of Brachiopods was a feature entirely absent in the Mollusca, and peculiar to the worms.

The bristles of worms differ from those of other articulate animals in having sheaths containing muscular fiber, while in other Articulates, the hairs were simply tubular prolongations of the epidermal layer. In Brachiopods the setæ or bristles were secreted by follicles imbedded, or surrounded by muscular fibers, and were moved freely by the animal. In the structure of the setæ he found an identity with that of the worms. He then called attention to the resemblance between the lophophore of the Brachiopods and a similar structure in the tubicolous worms. In *Sabella* the cephalic collar was split laterally, and a portion of it reflected. Let this collar be developed so as to cover the fringed arms, and a representation of the mantle of Brachiopoda would be attained. The thin and muscular visceral walls suggest similar parts in the worms. The circulating system he had not sufficiently studied, though Dr. Gratiolet had stated that in this respect there was a strong resemblance to the Crustacea.

In regard to the respiratory system, Burmeister had shown that there was a resemblance between the soft folds or lamellæ, developed on the internal surface of the mantle of *Balanidæ*, and similar features in *Lingula;* though the existence of these folds in *Lingula* had been questioned, he would presently show that Vogt was right in his observations. In regard to the reproductive system, he called attention to the fact that in one group of Cirripeds the ovaries were lodged in the upper surface of the peduncle, while in another group the same parts were lodged in the mantle. A similar condition existed in the Brachiopods where in one group the mantle holds the ovaries, while in another group they are found in the visceral cavity.

Through Polyzoa also he showed that, in their winter eggs or statoblasts, a relation was seen to the ephippia of *Daphniæ*, and the winter eggs of Rotifers.

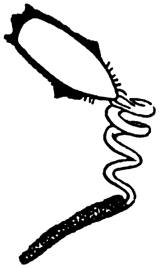
Of great importance also, and upon which he laid particular weight, were the peculiar oviducts with their trumpet-shaped openings so unlike the oviducts of mollusks, and as he believed, bearing the closest affinity to the oviducts in many of the worms, namely: a pair of tubes, and in one case two pairs, having their inner apertures with flaring mouths, suspended in the visceral cavity, thus opening a direct communication between the visceral fluids and the surrounding media. He then called attention to what little information we had regarding the embryology of the Brachiopods. Lacaze Duthiers had shown that in Thecidium the embryo was composed of four segments with eye-spots and other strong articulate features. Fritz Müller had given a description, with figures, of the early stage of *Discina*, in which we have not only little cirri projecting from the shell, but a little appendage recalling the plug or operculum in some of the tubiculous worms.

Of great importance also was the fact that in the early stage of *Discina*, Müller observed large bristles, and these were moved freely by the animal. Smitt had shown that in certain Polyzoa (*Lepralia*) the embryo, besides being furnished with cilia, also supported several bristles or setæ which were locomotive, and finally in the worms, Claparède and Mecznikow had figured an embryo of *Nerine* in which barbed bristles were also developed. Mr. Morse referred to his communication before the American Association for the Advancement of Science on the early stages of the Brachiopods, in which he had shown the intimate connections existing between this group and the Polyzoa. Now Leuckart had already seen reasons for placing the Polyzoa with the Annelids, and he would call attention to *Crepina gracilis* and *Phorronis hippocrepia*, admitted to be worms or early stages of them, and their close resemblance in nearly every point of their structure to the hippocrepian Polyzoa. Mr. Morse then stated that in the evidence already given, he had drawn his conclusions from alcoholic specimens of *Terebratula* and *Discina*, and from the papers of Lacaze-Duthiers, Claparède, Mecznikow, Hancock, Huxley, Vogt, Hyatt, Williams, De Morgan and others. He felt the importance of first examining *Lingula* in a living condition before making these announcements and for this reason he had recently visited the coast of North Carolina for the express purpose of finding if possible the rare *Lingula pyramidata* of Stimpson, first discovered by Prof. Agassiz in South Carolina. After nearly a week's fruitless search he had found it, had studied it alive, and had brought with him living examples, which he has the pleasure of exhibiting before the Society.

He would here express his deep sense of gratitude to Dr. Elliott Coues, Surgeon U. S. A. at Fort Macon, N. C., and the Commandant of the Post, Major Joseph Stewart, U. S. A., for the constant aid and sympathy rendered to Dr. A. S. Packard and himself during their visit there. He would not enter into a description of *Lingula* as he had already in preparation a memoir upon the subject, but would call attention simply to the additional evidence in support of the views advanced.

Lingula was found in a sand shoal at lowwater mark, buried just below the surface of the sand. The peduncle was six times the length of the shell, and was encased in a *sand tube* differing in no respect from the sand tubes of neighboring annelids. In many instances the peduncle was broken in sifting them from the sand, yet the wound was quickly repaired, and another sand case was formed.

1



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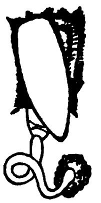

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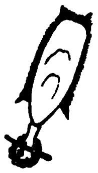


Fig. 1. Peduncle perfect, retaining portion of sand tube.
Fig. 2. Showing valves in motion; peduncle broken and forming new sand-case.
Fig. 3. Peduncle broken close to the body, and forming new sand-case.

He observed that *Lingula* had the power of moving over the sand by the sliding motion of the two valves, using at the same time the fringes of setæ which swung promptly back and forth like a galley of oars, leaving a peculiar track in the sand. In the motion of the setæ he noticed the impulse commencing from behind and running forward.

Within the mantle he found a series of rows of prominent lamellæ in which the blood rapidly circulated, thus confirming the correctness of Vogt's observations. These lamellæ were contractile, however.

The peduncle was hollow, and the blood could be seen coursing back and forth in its channel. It was distinctly and regularly constricted or ringed, and presented a remarkably worm-like appearance. It had layers of circular and longitudinal muscular fiber, and coiled itself in numerous folds or unwound at full length. It was contractile also, and would quickly jerk the body beneath the sand. But the most startling observation in connection with this interesting animal, was the fact that its blood was *red*. This was strongly marked in the gills and various ramifications of the mantle and in the peduncle. At times the peduncle would become congested and then a deep rose blush was markedly distinct. Mr. Morse expressed his gratification in having come to the conclusions in regard to the annelidan characters of Brachiopods a long time previous to his observations on *Lingula*.

He then concluded by stating that the Brachiopods, with the Polyzoa, should be removed from the Mollusca, and placed with the Articulates among the Annelids. That the Brachiopods came near the tubiculous worms, though they were much more highly cephalized. That they exhibit certain crustacean characters, but were widely removed from the Mollusca unless a relation could be traced through the homologues of the Polyzoa to that aberrant group the Tunicates, as pointed out by Allman. He believed the Brachiopods to be a comprehensive type, exhibiting general articulate features, and forming another example of those groups belonging to the last that exhibit the characters of two or more classes combined.

It was interesting in this connection to remark that Lingula, one of the earliest forms created, had yet remained the same through all ages of the earth's history.

A New Comet.—Winnecke discovered a new telescopic comet at Carlsruhe on the night of the 29–30 of May. The position obtained by him for May 30, is as follows: M. T. at C. $14^h\ 13^m\ 34^s$. R. A. $0^h\ 50^m\ 9{\cdot}55^s$. Decl. $+28°\ 52'\ 18''$. Vogel found for the same comet at the Leipsic observatory, M. T. at Leipsic $13^h\ 2^m\ 28{\cdot}5^s$, R. A. $0^h\ 50^m\ 4{\cdot}09^s$. Decl. $+28°\ 53'\ 17{\cdot}4^s$. We would call the attention of American astronomers to the prizes for the discovery of comets offered by the Austrian Academy. (See this Jour., vol. xlix, p. 442).

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the emission, absorption and reflexion of varieties of heat radiated at low temperatures.*—Magnus has communicated an interesting and valuable paper on the heat radiated at low temperatures, and on the absorption and reflexion of such rays. The results of this investigation,—the last we suppose made by the lamented author,—are so far as published, in his own words as follows:

(1.) Different bodies at 150° C. radiate different kinds of heat. These kinds of heat are more absorbed by a substance of the same kind, as the radiating body, than by others, and this absorption increases with the thickness of the absorbent.

(2.) There are substances which emit only one or a few kinds of heat, others, which emit many kinds.

(3.) To the first of these belongs rocksalt when quite pure. Just as its ignited vapor, or that of one of its constituents, sodium, radiates but one color, so rocksalt, even at a low temperature, emits but one kind of heat. It is monothermic, as its vapor is monochromatic.

(4.) Rocksalt even when quite clear emits, together with its peculiar rocksalt-heat, heat which is not more absorbed by a plate of rocksalt 80^{mm} in thickness than by one 20^{mm} in thickness.

(5.) Rocksalt absorbs very powerfully the heat which it radiates. It therefore does not, as Melloni supposed, allow all kinds of heat to pass through it with equal facility.

(6.) The great diathermancy of rocksalt does not depend upon its less power of absorption for different kinds of heat, but upon the fact that it radiates only one kind of heat, and consequently absorbs only this one, and that almost all other substances send out heat containing only a small fraction or none of the rays which rocksalt emits. But all rays which differ from those radiated by any substance, are not absorbed by it, but pass through with undiminished intensity.

From this we may infer that every substance is diathermanous, only because it radiates but few waves of quite definite length, and consequently absorbs only these, allowing all the others to pass through.

(7.) Sylvin behaves like rocksalt, but is not monothermic to the same extent. In the case of this substance also an analogy exists with its ignited vapors, or those of potassium, which as is well known yield a nearly continuous spectrum.

(8.) Fluor spar completely absorbs pure rocksalt heat. We ought therefore to expect that the heat which it emits will be equally absorbed by rocksalt. Nevertheless 70 per cent of this heat pass through a rocksalt plate 20^{mm} in thickness. This may doubtless be easily explained with reference to the proportion of the quantity of heat which fluorspar emits in comparison with that of the rocksalt; still it is possible that fluorspar at 150° emits rays other than those which it absorbs at ordinary temperatures. This behavior is however probably connected with the great reflecting power of fluorspar for rocksalt-heat, of which I shall speak in the second part of this memoir.

(9.) If it were possible to produce a spectrum of the heat radiated at 150° C., the spectrum would, if rocksalt were the radiating body, exhibit only one luminous band. If sylvin were used as a radiator, the spectrum would be much more extended, but would still occupy but a small portion of the spectrum which the heat radiated from lamp-black would form.

In concluding this part of his memoir Magnus makes some remarks on transparency which seem to us very suggestive. If we assume that there is a constant interchange of heat even between bodies having the same temperature, we may fairly assume also that there is such an exchange in the case of light. We cannot observe the light which bodies emit at ordinary temperatures, but they do not absorb light, since this absorption produces their colors. If such an exchange of light takes place at ordinary temperatures, it would follow that transparent bodies either radiate only such rays as are not contained in the light emitted by ignited bodies, and then they absorb none of these rays, or that they emit only one or a few of the wave-lengths of the light which is visible to us; since then they absorb only these, and allow all others to pass through, so that the intensity of the transmitted light is but little less than that of the incident light. We may therefore infer that the transparency of bodies depends upon the fact that they only radiate a few of the wave-lengths, which are contained in the light known to us.—*Pogg. Ann.*, Band cxxxix, p. 431. W. G.

2. *On the breadth of the spectral lines.*—LIPPICH has given an explanation of the breadth of the spectral lines of luminous gases, which depends upon the now generally received dynamical theory of gases and vapors. The explanation amounts to a mathematical theory of the broadening of the bands, the diminutions in their intensity, the addition of new bands, and the appearance of parts of continuous spectra, all of which appear to depend upon changes of temperature. In the present paper the author treats only of the application of the theory to the widening of the bands. The fundamental assumption is this; if it be necessary to consider a molecule of a gas as a system capable of vibration, the spectrum of an ideal gas in which the molecules would be perfectly free elastic systems, could consist only of a number of different colored bands of absolutely homogeneous light, if the vibratory motions of the molecules alone are taken into account. According to the theory of Krönig and Clausius, the molecules of a gas have progressive motions, with very great velocities, and this circumstance in connection with the well known influence of the motion of a luminous point upon the refrangibility of the emitted rays, makes it possible to explain the widening of the spectral

bands, and to show the dependence of the breadth of the brands upon the temperature and density of the ignited gas. Setting out with these principles, the author arrives at the following law:

The ratio of the difference of the wave-lengths which correspond to the borders of a spectral band, to the mean wave-length of this band, is for one and the same gas constant for all the spectral bands, and in different gases is directly proportional to the square root of the absolute temperature, but inversely proportional to the square root of the density.

The breadths of the bands, as they appear in the spectroscope to the eye, will increase with the refrangibility of the rays somewhat more rapidly than $\frac{1}{\lambda^2}$. The author in the next place shows that in the case of dark bands in a spectrum, produced by inversion, the distribution of the darkness, so to speak, and the breadths of the band will follow the same law as that above given for bright bands. The mere comparison of the relative breadths of the bands may lead to important conclusions. If in any gas-spectrum bands of different breadths lie near together, we might infer the presence of a mixture of gases of different densities, or of different allotropic states of the same gas. Thus the author thinks that the presence of some fine lines in the blue of the oxygen-spectrum may indicate the presence of the denser ozone. Furthermore, the observation of the breadths of these lines may decide whether the appearance of new lines is due to the more intense vibrations of the same molecules, or to their allotropic conditions. For the same gas the breadth of the spectral bands permits a conclusion as to the temperature. This is of special interest in the case of the heavenly bodies, in the spectra of which a widening especially of the hydrogen lines has been observed. The author does not agree with Frankland and Lockyer in ascribing this result solely to the higher pressure of the gas, but thinks that it must also have a much higher temperature. As Huggins and Lockyer have observed a widening of certain dark lines in the spectrum of the umbra of a solar spot, the author infers that the gases and vapors forming the umbra must have a higher temperature than that of the part of the sun's atmosphere, in which Fraunhofer's lines originate. Finally, Lippich suggests that measurements of the breadths of the spectral bands will finally lead not merely to relative but to approximate absolute values of temperature and density. Among other results, it may be possible in this way to furnish an experimental proof of the correctness of the dynamical theory of gases. In conclusion, the author remarks that, strictly speaking, the results which he has obtained apply only to perfect gases. Changes in the spectrum will indicate changes from gases to vapors. New periods of vibration will occur, which will exhibit themselves in the spectrum with the less intensity, the greater their deviation from the periods of vibration of the molecules of the perfect gas. In this manner a spectral band will fade out on both sides, and with increased pressure the illuminated portion will become broader the more the gas deviates from the law of

Gay Lussac and Mariotte. This explanation agrees with Wüllner's experiments, according to which the "washing out" of the bands occurs in the cases of oxygen and nitrogen, under much lower pressures than in the case of hydrogen.—*Pogg. Ann.*, cxxxix, p. 465. W. G.

3. *On the position of thallium among the elements.*—Rammelsberg has contributed some interesting facts in relation to the compounds of thallium, without however furnishing data for determining the degree of atomicity of the metal. The author, in the first place, calls attention to the fact that the isomorphism of thallium in its compounds with potassium and sodium is not sufficient to decide the question. When thallic sesquioxyd is heated with a solution of iodic acid, a normal dithallic salt is formed, which has the formula $\text{Ŧl}(\text{IΘ}_3)_6+3\text{aq}$. The sesquioxyd dissolves easily and completely in chlorhydric acid, forming a colorless solution, which after addition of potassic or ammonic chlorid yields beautiful crystals. The new salts, respectively $\text{Ŧl}\text{Cl}_6,6\text{KCl}+4\text{aq}$. and $\text{Ŧl}\text{Cl}_6,6\text{NH}_4\text{Cl}+4\text{aq}$., form large colorless transparent crystals, which resemble combinations of the cube, octahedron and dodecahedron, but which really belong to the square prismatic system. They are not decomposed by water, even on boiling. With bromine and potassic bromid, and iodine and potassic iodid, thallium forms the salts, $\text{Ŧl}\text{Br}_6,3\text{KBr}+3\text{aq}$. and $\text{Ŧl}\text{I}_6,3\text{KI}+3\text{aq}$., which crystallize in regular octahedra. Rammelsberg remarks that the thallium atom, $\text{Tl}=204$, can hardly be regarded as other than monatomic, but as the double atom of the molecule in the dithallic compounds is hexatomic, the single atom would have to be considered as tetratomic. The specific heat of thallium is an evidence that the metal is $\text{Tl}=204$, but the isomorphism of two monatomic with one diatomic atom has been established in so many cases that no certain conclusion can be drawn from the isomorphism of thallium with potassium and sodium. It seems probable that the question can only be settled by determinations of vapor density.—*Berichte der Deutschen Chemischen Gesellschaft, Jahrgang*, 3, No. 7, p. 360. W. G.

4. *On a new method for the volumetric estimation of copper.*—Weil has given a new method for the determination of copper which appears deserving of attention. It is based upon the following facts. At a boiling heat and in presence of an excess of free chlorhydric acid the least trace of cupric chlorid communicates a very distinct greenish yellow tint to the solution. This tint is the more intense the greater the quantity of acid present. Stannous chlorid instantly reduces under these circumstances cupric chlorid to colorless soluble cuprous chlorid.

$$2\text{Ȼu}\text{Cl}_2+\text{SnCl}_2=\text{Ȼu}_2\text{Cl}_2+\text{SnCl}_4.$$

The reaction is finished when the green solution of cupric chlorid is completely decolorized. A single drop of stannous chlorid in excess is easily detected by a drop of mercuric chlorid, which gives a precipitate of calomel. When the solution contains iron in the form of sesquioxyd as well as copper, the volume of the

solution of tin employed will indicate the sum of the copper and iron. In this case the author precipitates the copper in another portion of the assay by means of zinc coupled with platinum, and then determines the iron by means of potassic hypermanganate. The copper is then easily found by difference. The author determines the litre of his solution of tin by means of pure metallic copper, and preserves it from oxydation under a layer of petroleum. It is of course necessary that the solution of copper should be perfectly free from nitric acid.—*Comptes Rendus*, lxx, p. 997.
W. G.

5. *On the utilization of the secondary products obtained in the manufacture of chloral.*—Dr. A. W. Hofmann has examined a mixture of secondary products obtained during the manufacture of chloral, and condensed during cold weather. The liquid began to boil at 17°–18°, rising slowly to 30°–31°, where the temperature remained constant a short time, and then rising again to 50°, when nearly all distilled over. The most volatile portions were mixed with three times their volume of alcohol saturated at 0° with ammonia and heated in a water bath for an hour. The liquid was then filtered to separate crystals of sal-ammoniac and the alcohol ammonia and chlorinated ethylic chlorids distilled off. The mass of chlorhydrates of ethyl-ammonias remaining were decomposed with caustic soda, and the separated liquid alkalies dehydrated by caustic soda, and finally distilled. In this manner 5 litres of the secondary products operated on gave 1½ liters of a mixture of anhydrous ethylamines. These could be separated from each other by means of oxalic ether, in the manner already pointed out by Hofmann. The results of this investigation are interesting, from the prospect which they afford of obtaining the ethyl-ammonias as an article of commerce, at a reasonable price, and in comparative abundance.—*Comptes Rendus*, lxx, p. 906. W. G.

6. *On the nature of the secondary products obtained in the manufacture of chloral.*—Krämer has studied the other products of the action of chlorine upon alcohol, the existence of a large quantity of ethylic chlorid having been shown by Hofmann. As the ethylic chlorid was in contact with an excess of chlorine, it was natural to expect to find in the less volatile oily products the whole series of chlorinated ethylic chlorids described by Regnault, and experiment showed that several of these substances were present. The most volatile product boiling at 60°, proved to be chlorinated chlorethyl or chlorethyliden, $\text{Є}_2\text{H}_4\text{Cl}_2$, identical with the chlorethyliden prepared from aldehyd. A liquid boiling at 85°, proved to be ethylen-dichlorid, the formation of which by the action of chlorine upon ethylic chlorid had not before been observed. The next product was chlorinated ethylen dichlorid $\text{Є}_2\text{H}_3\text{Cl}\,.\,\text{Cl}_2$ boiling at 115°, and the last bichlorinated ethylen, $\text{Є}_2\text{H}_2\text{Cl}_2$, boiling at 37°. Other chlorinated products were also observed, but not yet studied. To prove the identity of the chlorethyliden obtained in this manner with that obtained from aldehyd by the action of phosphoric pentachlorid, Krämer heated a portion of it

with alcoholic ammonia to 160° for 12 hours. In this manner an oily base boiling at 180°–182°, and having the characteristic odor of collidine, $C_8H_{11}N$, was obtained. This base had already been formed from aldehyd-ammonia by Bæyer, and found to be identical with that obtained by Anderson from animal oil.—*Berichte der Deutschen Chemischen Gesellschaft, Jahrgang*, 3, p. 257–262.
W. G.

7. *On some properties of iron precipitated by the galvanic current.*—Lenz has given some interesting particulars in relation to the composition and properties of iron as precipitated in the metallic form by the battery. The iron examined was deposited by Klein's process from a solution of the mixed sulphates of iron and magnesium. Weak currents were employed, and the solution was kept neutral by carbonate of magnesia. Iron so thrown down has a beautiful fine-granular structure, showing no traces of crystals under the microscope. Its color is a soft bright gray. Its hardness is very remarkable—not less than 5·5 of the ordinary mineral scale, and it is excessively brittle, so that it may be rubbed to powder between the fingers. When the iron is slowly reduced upon a polished surface, it is free from flaws and has a velvety look. As it becomes thicker, bubbles or pits are formed as small oval depressions. When heated over a fire the iron loses many of these properties in a remarkable degree. Its hardness diminishes and becomes 4·5; its brittleness entirely disappears, and it becomes so flexible and tenacious that it cannot be broken by repeated bending or even by folding and strongly smoothing down the folds. When heated in vacuo the iron changes color and becomes almost as white as worked platinum. The ignited iron rusts very quickly both in air and in previously boiled water; this is not the case with the metal before ignition. In the electric series unignited iron stands nearer to copper than the ignited metal. On analysis by means of Sprengel's pump, the precipitated iron was found to contain various gases—vapor of water, nitrogen, carbonic oxyd, carbonic acid and hydrogen. Levy thinks that the carbonic acid came from the solution from which the iron was precipitated, and that the carbonic oxyd was formed during the ignition of the tube containing the iron in the process of analysis; also that the vapor of water was formed by the union of hydrogen with the oxygen of a small amount of rust in the iron, since it was only given off at 1,600° C. The hydrogen in the iron was always in largest quantity; the whole quantity of gas varied greatly, and sometimes amounted to 185 times the volume of the iron. The absorption of the gases was found to take place mainly in the first layers formed. On warming the reduced iron, the evolution of gas began at temperatures below 100°, but at this temperature chiefly hydrogen was evolved. Ignited galvanically reduced iron decomposes water and absorbs the free hydrogen either wholly or partially.—*Bulletin de l' Académie de Pétersbourg*, xiv, p. 337, *cited in Dingler's Polytechnisches Journal*, cxcvi, p. 44. W. G.

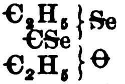
8. *On the preparation of barium chlorate.*—BRANDAU has proposed the following simple method for the preparation of barium chlorate: Commercial crystallized aluminum sulphate, sulphuric acid, and potassium chlorate, in the ratio of one molecule of each of the two former to two of the latter, are mixed with water to the consistence of a thin paste, warmed for half an hour on the water-bath, allowed to cool completely, and treated with alcohol in excess. Upon filtering, and neutralizing with barium hydrate, barium sulphate and some aluminum hydrate are precipitated, and barium chlorate remains in solution. The alcohol is distilled off, and the filtrate on evaporation yields crystals of the pure barium chlorate. The only precaution necessary is to have the aluminum sulphate and the sulphuric acid in slight excess.—*Ann. Ch. Pharm.*, cli, 361, Sept. 1869. G. F. B.

9. *On the properties of Selenium.*—RATHKE has investigated, in the Halle laboratory, the various modifications which selenium, like sulphur, is capable of assuming. Notwithstanding the difference in the behavior of the various forms of each, respectively, to solvents, especially $\mathrm{ꞒS_2}$, Rathke places the insoluble black selenium with the rhombic variety of sulphur, and the red amorphous selenium with the insoluble amorphous form of sulphur. As in the case of sulphur, the latter variety of selenium is produced by the decomposition of selendithionates (solutions of selenium in alkaline sulphites) by acids; by the action of water on selenium chlorid; and by the sudden cooling of fused selenium. So on the other hand, by the slow decomposition of a solution of potassium selenid, distinct crystals of black selenium are produced, precisely as when by the similar decomposition of alkaline sulphids, large rhombic crystals of sulphur separate. Though red selenium is more stable than the corresponding form of sulphur, yet, like this, it passes into the other variety on raising the temperature to 100° C., with a distinct evolution of heat. The specific gravity of selenium in these forms, is as follows: For the black variety, 4·80 to 4·81; the red, crystallized from carbon disulphid, (and corresponding to monoclinic sulphur,) 4·46 to 4·51, the red, amorphous, 4·26. Agreeing here, also, with the corresponding forms of sulphur, as already given. Regarding the behavior of selenium toward carbon disulphid as exceptional, Rathke tried the action of other solvents. Sulphur chlorid saturated with selenium with the aid of heat, deposits on cooling, crystals of sulphur, leaving selenium chlorid, in which selenium dissolves freely, forming when cold a syrupy liquid, from which, on standing, the black variety of selenium separated in small nodules. The solubility of rhombic (octahedral) sulphur in carbon disulphid, led to the supposition that selenium might be equally soluble in carbon diselenid. Various methods for preparing this substance were tried, but, though small quantities were obtained by acting on selenium phosphid with moist vapor of carbon tetrachlorid, heated in a tube, sufficient for the above purpose could not be obtained. Carbon tetrachlorid itself, does not dissolve the black variety of selenium, selenium-ethyl dissolves both,

but in very small quantity. Of the solvents tried, only those which were selenium-compounds dissolved black selenium.

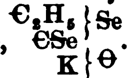
Rathke also examined the compounds which sulphur forms with selenium, hoping to throw some light upon their mutual iso-dimorphism. He first melted the substances together in the ratio of one atom of selenium to two of sulphur, repeatedly extracted the resulting mass with carbon disulphid, and crystallized out the portion dissolved in fractions. The crystals, however, increased progressively in sulphur and diminished in selenium. The precipitate obtained when sulphydric gas and selenium dioxyd gas act upon each other in solution, was then examined. By solution in carbon disulphid and evaporation, well-formed rhombic prisms, in color like potassium dichromate, were obtained, which gave on analysis 63·86 per cent selenium and 35·50 per cent sulphur. These crystals, then, though by no means a mixture of the constituents, do not correspond in composition to the formula SeS_2, which requires 55·42 per cent Se and 44·58 per cent S. The process was then reversed, and the precipitate produced when selenhydric gas reacts upon sulphur dioxyd collected and treated as above. It contained considerable black selenium, not taken up by the solvent. On fractionally crystallizing, three products were obtained, one (*c*) soluble in 57 parts CS_2, the second (*b*) in 263 parts, and the third (*a*) considerably more so than selenium itself. (*a*) consisted of minute dark ruby-red, rounded crystals; (*b*) of brilliant-red prisms; and (*c*) of orange-red tabular prisms. On analysis, (*a*) was found to agree nearly with the formula Se_2S; but (*b*) and (*c*) were intermediate between Se_2S and SeS_2. Rathke regards them as isomorphous mixtures.

Carbon diselenid CSe_2 was prepared but only in small quantity, not even sufficient for analysis. It appeared as a thin, brilliant-brown liquid, of peculiar and disagreeable odor, recalling at first, when dilute, that of carbon disulphid, but being when concentrated, very pungent and irritating to the eyes. It burned with the blue selenium flame, was not very volatile, and was insoluble in all solvents tried. At the same time there was formed ethyl selenxanthate,



$$\left.\begin{array}{l} C_2H_5 \\ CSe \\ C_2H_5 \end{array}\right\} \begin{array}{l} Se \\ \\ \Theta \end{array}$$

and potassium selenxanthate,

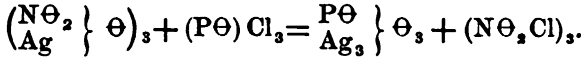


$$\left.\begin{array}{l} C_2H_5 \\ CSe \\ K \end{array}\right\} \begin{array}{l} Se \\ \\ \Theta \end{array}.$$

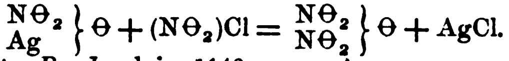
Rathke also also attempted the production of selenium tetra-ethyd but succeeded in getting only tri-ethylselenin chlorid $Se(C_2H_5)_3Cl$, in combination with zinc chlorid, and also with platinum chlorid.—*Ann. Ch. Pharm.*, clii, 181, Nov. 1869. G. F. B.

10. *On the preparation of nitrogen pentoxyd* (*nitric anhydrid.*)—ODET and VIGNON employ for this purpose a double U tube, in each bend of which 140 to 150 grams of silver nitrate is placed, the whole being heated to a temperature of 60° C. Phosphoryl chlorid ($P\Theta Cl_3$) is allowed to fall drop by drop, into the first leg of the tube. The nitryl chlorid ($N\Theta_2Cl$) thus produced, reacts upon the silver nitrate in the second bend, to which is attached, by melting, a receiver, in which the product of the reaction collects.

This receiver is immersed in a freezing mixture. No disengagement of oxygen takes place, silver phosphate and chlorid being the only secondary products. The reactions are as follows:—

(1) 

$$\left(\left.\begin{matrix}N\Theta_2\\Ag\end{matrix}\right\}\Theta\right)_3+(P\Theta)Cl_3=\left.\begin{matrix}P\Theta\\Ag_3\end{matrix}\right\}\Theta_3+(N\Theta_2Cl)_3.$$

(2) 

$$\left.\begin{matrix}N\Theta_2\\Ag\end{matrix}\right\}\Theta+(N\Theta_2)Cl=\left.\begin{matrix}N\Theta_2\\N\Theta_2\end{matrix}\right\}\Theta+AgCl.$$

—*Comptes Rendus*, lxix, 1142. G. F. B.

11. *On a new method for preparing bromhydric acid.*—Champion and Pellet describe a new and simple mode of preparing bromhydric acid by the action of bromine upon paraffin at a moderately elevated temperature. Two retorts are employed, one for the bromine, the other for the paraffin. The neck of the first retort is prolonged and bent at right angles so as to enter the tubulure of the second—being fixed there by a ground joint—terminating near the bottom. By means of a sand or oil-bath, the paraffin is kept at a temperature of 180° C. and by a saline bath the bromine is maintained at 65° C. As the bromine gradually distils over into the paraffin, the bromhydric acid gas is evolved, and after passing through a bulb tube containing water, and a U tube filled with broken glass and bits of moistened phosphorus, to remove any traces of unchanged bromine, it is collected in the liquid to be saturated with it, placed in a vessel surrounded with ice. The aqueous solution thus obtained saturated at 0° C. has a density of 1·78, and corresponds to the formula $HBr, H_2\Theta$. Each c. c. contains 1·46 grms. HBr.—*Bull. Soc. Ch.*, II, xiii, 197, March, 1870.

G. F. B.

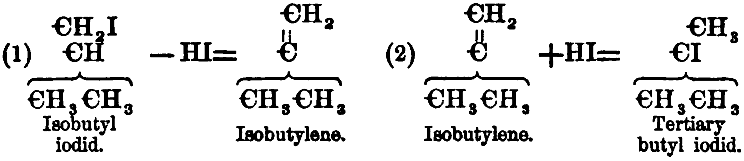
12. *On the recovery of Uranium from the Phosphate.*—In using a solution of uranium for determining volumetrically phosphoric acid, residues of uranium phosphate are obtained, from which it is desirable to recover the uranium. Two methods for doing this have been lately proposed. The first by Heintz, obtains the uranium as nitrate. The phosphate, previously washed, dried and weighed, is dissolved in nitric acid. Half as much pure tin is weighed out, and nine-tenths of it added to the nitric solution, which is then heated until the tin is entirely converted into stannic hydrate; if, in this solution, ammonia gives a precipitate not entirely soluble in acetic acid, the rest of the tin is added, and the process repeated. All the phosphoric acid is contained in the precipitate. The filtrate is diluted, treated with sulphydric gas to precipitate the last traces of tin, again filtered, and evaporated to crystallization.

The second method, proposed by Reichardt, consists in dissolving the uranium residues in either nitric or chlorhydric acid, adding an excess of ferric chlorid, making the solution acid with acetic acid by adding sodium acetate, and after considerable dilution, heating to boiling. All the phosphoric acid, together with the excess of iron, is thus precipitated. Instead of boiling the solution after the addition of the ferric chlorid, it may be treated with an excess of sodium carbonate, and filtered. In presence of free carbonic acid, the sodium carbonate holds the uranium in solution, the phosphoric acid being precipitated with the iron. The filtrate from either of these methods is acidulated with chlorhydric acid, and after boiling to expel the carbonic gas, the uranium oxyd is precipitated with ammonia.—*Ann. Ch. Pharm.*, cli, 216, Aug. 1869. *Zeitschr. analyt. Chem.*, 1869, 116. G. F. B.

13. *On the modifications of sulphur trioxyd* (*sulphuric anhydrid.*)—SCHULTZ-SELLACK has examined the isomeric modifications of sulphur trioxyd. It was prepared by distillation from di-sulphuric acid (fuming,) the vapors being rendered anhydrous by passing them over phosphoric anhydrid. On cooling the liquid thus obtained, the thermometer immersed in it is observed to become stationary at 16° C., the fluid solidifying in long transparent prisms. These melt again at the same temperature, the liquid being obtained again, frequently unaltered. Sometimes, however, white flocks remain in the melted mass, which gradually collect in warty masses of fine white needles, until the entire liquid has become a matted mass of them. The same change takes place when the liquid is kept for some time at a temperature below 25° C.; above 27° C., however, it does not take place. The solid anhydrid thus produced becomes gradually fluid again when the temperature rises to above 50° C.; but this solidification and liquefaction is not a freezing and melting, in the ordinary sense of these terms, since both take place gradually, within certain definite ranges of temperature. The fluid anhydrid has a remarkably high coefficient of expansion by heat; being for temperatures between 25° and 45°, 0·0027 for each degree, more than two-thirds that of the gases. It boils at 46° C. under a pressure of $760^{mm.}$; at 20° its vapor-tension is 200^{mm}. In a vacuum the first modification shows no vapor-tension; but gradually vapor is evolved, so that after several days, a tension of 30 or $40^{mm.}$ at 20° is obtained. The vapor of the solid as well as that of the fluid trioxyd has a normal density, found to be 2·74 to 2·76; $\text{S}\!\!\!-\!\Theta_3$ requires 2·76. From the above facts, the author concludes that sulphur trioxyd exists in two states: 1st, α Sulphuric anhydrid solidifying at +16° in long colorless prisms, which melt at the same temperature; boiling at 46° C. 2d. β Sulphuric anhydrid, produced from the former, at temperatures below 25°, in very fine needles; becoming at temperatures above 50° gradually fluid again, being transformed into α; yielding vapor very slowly at ordinary temperatures, which is like that given by α, but has a less tension. He regards β as a polymer of α, since many polymeric organic compounds, as, for example, cyanuric acid, show similar variations in physical properties, as their molecules are more condensed.—*Ber. Berl. Chem. Ges.*, iii, 215, March, 1870. G. F. B.

14. *On the conversion of isobutyl alcohol into tertiary pseudo-butyl alcohol.*—From purely theoretical considerations, MARKOWNIKOFF was led to believe that isobutyl alcohol (that produced during fermentation) by the loss of water, or its iodid by the loss of

hydriodic acid, would give a butylene identical with the isobutylene of Butlerow. By heating isobutyl iodid with alcoholic potash, a butylene was obtained, which united directly with hydriodic acid, giving a butyl iodid having the boiling point of tertiary pseudo-butyl iodid. On treating this with moist silver oxyd, tri-methyl-carbinol (tertiary pseudo-butyl alcohol) was obtained with its very characteristic properties. The reaction which takes place is as follows:



$$(1)\ \underset{\text{ЄH}_3\,\text{ЄH}_3}{\underbrace{\overset{\text{ЄH}_2\text{I}}{\text{ЄH}}}} - \text{HI} = \underset{\text{ЄH}_3\text{ЄH}_3}{\underbrace{\overset{\text{ЄH}_2}{\overset{\|}{\text{Є}}}}} \qquad (2)\ \underset{\text{ЄH}_3\text{ЄH}_3}{\underbrace{\overset{\text{ЄH}_2}{\overset{\|}{\text{Є}}}}} + \text{HI} = \underset{\text{ЄH}_3\text{ЄH}_3}{\underbrace{\overset{\text{ЄH}_3}{\text{ЄI}}}}$$

Isobutyl iodid. Isobutylene. Isobutylene. Tertiary butyl iodid.

Morkownikoff believes that these facts sustain his general law of the formation of unsaturated compounds; a law which for the homologues of ethylene may thus be formulated: *When water is removed from an alcohol-molecule, that carbon-atom which is directlg united with the carbon-atom holding the hydroxyl, furnish the hydrogen.* A similar law holds in the case of the haloid anhydrids of the monatomic alcohols. *When combination takes place between an unsymmetrically constituted homologue of ethylene* (for example, propylene $\text{ЄH}_3\text{ЄHЄH}_2$) *and water* (*or an haloid acid*), *the residues into which these latter bodies are separated, are divided between the two carbon-atoms so that the hydroxyl* (*or the corresponding halogen*), *is united to the least hydrogenized carbon-atom in the compound.—Zeitschr. Chem.*, II, vi, 29, Dec., 1869. G. F. B.

15. *On the Synthesis of Aromatic Acids.*—Starting from the well known facts that a monobasic acid may be viewed as a hydrocarbon in which an atom of hydrogen is replaced by carboxyl ($\text{Є}\Theta\Theta\text{H}$), and further, that, in general, the basicity of acids depends upon the number of carboxyl groups which they contain, Wurtz proposes to make practical use of them in synthesis. By effecting one or more such replacements in a hydrocarbon, either by the carboxyl group itself or its ethyl-derivative ($\text{Є}\Theta\Theta(\text{Є}_2\text{H}_5)$),—which can be effected by heating the corresponding bromine derivative of the next lower homologous hydrocarbon with ethyl chlorocarbonate and sodium-amalgam,—any desired aromatic acid may be produced. In this way, for example, Wurtz prepared benzoic acid; 90 grams monobromo-benzol (phenyl bromid, $\text{Є}_6\text{H}_5\text{Br}$) being heated with 60 grams ethyl chlorocarbonate ($\text{Є}\Theta\Theta(\text{Є}_2\text{H}_5)\text{Cl}$) and 3·5 kilograms sodium-amalgam containing one per cent of sodium. The action proceeded slowly, requiring many days for completion, even at 110° C. Carbonic and carbonous gases were evolved, sometimes mixed with a gas which burned with a green flame, probably ethyl chlorid. When the fluid had entirely disappeared and the mercury had again recovered its mobility, the saline mass was extracted with ether, and the ether distilled off till the temperature rose to 150° C. The residue, which

contained ethyl benzoate, on being treated with potassium hydrate, evaporating to dryness to remove the alcohol, re-solution in water, and precipitation with hydrochloric acid, afforded benzoic acid abundantly. The reaction, therefore, is as follows:

$$Є_6H_5Br + ЄΘ\left\{\begin{matrix}ΘЄ_2H_5\\ Cl\end{matrix}\right. + Na_2 = NaCl + NaBr + Є_6H_5(ЄΘΘЄ_2H_5)$$

By using mono-bromo-toluol $\left(Є_6H_4\left\{\begin{matrix}ЄH_3\\ Br\end{matrix}\right.\right)$, toluic acid $\left(Є_6H_4\left\{\begin{matrix}ЄH_3\\ ЄΘΘЄ_2H_5\end{matrix}\right.\right)$ with a trace of another acid perhaps isomeric with it, was obtained. With the isomeric benzyl bromid $(C_6H_5(CH_2Br))$ the reaction yields a more complicated product.

In a more recent paper, Wurtz shows that the acid mentioned above as obtained simultaneously with the toluic, is iso-toluic acid; and that its presence is due to the fact that the bromo-toluol used, contained an isomeric body. The substance obtained by acting upon benzyl bromid or chlorid, he finds to have the composition $Є_{15}H_{14}Θ_2$, and he gives it the name di-benzyl-carboxylic acid. In a second operation, 252 grams benzyl chlorid, 108 grams ethyl chlorocarbonate and 8,000 grams one per cent sodium-amalgam, were heated on a saline bath, with an upward condenser, till the whole mass was solid. This residue was extracted with ether, the ether distilled off till the temperature rose to 180°, the fluid remaining in the retort decomposed with alcoholic potash, the the alcohol evaporated, the residue dissolved in water, precipitated by hydrochloric acid and recrystallized from water. It separated in drops which solidified to a mass of fine needles. It is almost insoluble in cold water, and but little soluble in hot; alcohol and ether dissolve it readily. At 84° it melts and at a higher temperature distils. Its vapors are aromatic and irritating. To produce it, Wurtz assumes that under the influence of the sodium, the chlorid, by the loss of hydrochloric acid, becomes chloro-di-benzyl $\begin{matrix}Є_6H_5ЄH_2\\ |\\ Є_6H_5ЄHCl\end{matrix}$ and that this by the simultaneous action of the sodium and the ethyl chlorocarbonate becomes ethyl di-benzyl-carboxylate $\begin{matrix}Є_6H_5ЄH_2\\ |\\ Є_6H_5ЄHЄΘΘ(Є_2H_5).\end{matrix}$—*Comptes Rendus*, lxviii, 1298; lxx, 350. G. F. B.

16. *On the volatile acids of Croton oil.*—Froelich having observed in the Jena laboratory, that by the action of phosphorus pentachlorid upon ethyl-diacetic acid, two metameric chlor-acids were obtained, yielding when treated with sodium-amalgam, two metameric acids of the composition $Є_4H_6Θ_2$, of which one was solid and identical with that prepared from allyl cyanid, the other was fluid, and supposed to be the same as that described by Schlippe as occurring in Croton oil, and called Crotonic acid, GEUTHER undertook a confirmation of this supposition. Having prepared the volatile acids from four pounds of Croton oil, he finds that no volatile acid of the composition $Є_4H_6Θ_2$ exists in this oil, and that the solid acid contained in it is not angelic acid; and there-

fore, that Schlippe's statements are entirely erroneous. The volatile fluid acids are essentially acetic, butyric and valeric, mixed perhaps with traces of oenanthic acid and higher members of the oleic series. The solid acid supposed by Schlippe to be angelic acid, is a metamer of it, to which Genther gives the name Tiglic acid. It constitutes more than a third of the volatile acids of Croton oil, and forms a barium salt readily soluble in water, crystallizing in pearly plates, and having the composition $Є_5H_7Ba'\Theta_2+5H_2\Theta$. It has a remarkably close correspondence in properties with the methyl-crotonic acid of Frankland and Duppa. It is therefore obvious that the name "crotonic" given to the acid $Є_4H_6\Theta_2$, is a misnomer, since croton oil contains no acid of this composition. Geuther therefore proposes to call the chlor-acid of Froelich, mentioned above, which fuses at 59·5° and boils at 194·8°, monochlor-quartenic acid, and the acid derived from it by the action of sodium-amalgam, which is fluid at 15° and boils at 171·9°, quartenic acid. For the metamer of the chlor-acid, melting at 94° and boiling between 206°–211° with partial decomposition, he proposes the name mono-chlor-tetracrylic acid; and for its derivative $Є_4H_6\Theta_2$, first prepared from allyl cyanid and till now called crotonic acid, the name tetracrylic acid. Its aldehyd called croton-aldehyd by Kekulé, would therefore be tetracryl-aldehyd.—*Zeitschr. Chem.*, II, vi, 26, Dec. 1869. G. F. B.

17. *On the Rhenish creosote from beech-wood tar.*—Under the direction of Baeyer and Graebe in Berlin, Marasse has made an investigation of beech-wood creosote, with results far more satisfactory and conclusive than had been previously obtained. The material on which he worked came from the manufactory of Dietze & Company in Mayence; it was colorless, a little thick, heavier than water, in which it was scarcely soluble, and dissolved completely in potassium hydrate solution. On subjecting it to fractional distillation, three separate products were obtained: one boiling below 199°, one between 200° and 203° (by far the larger portion) and one between 216° and 220°. After drying the lightest product, and subjecting it to sixteen fractional distillations, a body was obtained, which boiled between 183° and 184°, solidified on cooling, and had the properties of phenol, which an analysis proved it to be. On distilling the second and largest fraction with zinc-dust and purifying and fractioning the distillate, two products were obtained; the one, boiling between 110° and 112°, proved on analysis to be toluol, $Є_7H_8$, or $Є_6H_4\left\{\begin{matrix}ЄH_3\\H\end{matrix}\right.$. Since the zinc-dust acts by reducing hydroxyl to hydrogen, the body yielding this toluol must have been $Є_6H_4\left\{\begin{matrix}ЄH_3\\\Theta H\end{matrix}\right.$ or cresol. The other portion boiling at 150° to 155° afforded the properties and composition of anisol. As this anisol $Є_6H_4\left\{\begin{matrix}H\\\Theta ЄH_3\end{matrix}\right.$ does not exist in the creosote as such, it must have been produced by a similar action of the zinc-dust, from the body $Є_6H_4\left\{\begin{matrix}\Theta H\\\Theta CH_3\end{matrix}\right.$ which is guaiacol, the acid methyl

ether of pyrocatechin $\text{Є}_6\text{H}_4\begin{cases}\Theta\text{H}\\\Theta\text{H}\end{cases}$. That the fraction boiling between 200° and 203° was thus composed, Marasse further proved by fusing it with potassium hydrate. Two liquids were thus obtained which on examination proved to be cresol itself $\text{Є}_6\text{H}_4\begin{cases}\text{ЄH}_3\\\Theta\text{H}\end{cases}$, and pyro-catechin $\text{Є}_6\text{H}_4\begin{cases}\Theta\text{H}\\\Theta\text{H}\end{cases}$, the latter produced by the saponification of its ether, guaiacol, in the experiment. The same result was reached by acting upon this fraction with hydriodic acid; cresol and pyro-catechin being produced as before. And finally, by acting upon this fraction with methyl iodid and potassium hydrate in sealed tubes, the methyl ethers of both cresol (cresyl-anisol $\text{Є}_6\text{H}_4\begin{cases}\text{ЄH}_3\\\Theta\text{ЄH}_3\end{cases}$) and guaiacol ($\text{Є}_6\text{H}_4\begin{cases}\Theta\text{ЄH}_3\\\Theta\text{ЄH}_3\end{cases}$) were obtained. The last fraction, boiling between 217° and 220°, afforded, after reduction with hydriodic acid and fractioning, phlorol, $\text{Є}_8\text{H}_{10}\Theta$ or $\text{Є}_6\text{H}_3\begin{cases}\text{ЄH}_3\\\text{ЄH}_3\\\Theta\text{H}\end{cases}$ and homo-pyro-catechin $\text{Є}_6\text{H}_3\begin{cases}\text{ЄH}_3\\\Theta\text{H}\\\Theta\text{H}\end{cases}$, which last substance was derived from creosol, its acid methyl ether $\text{Є}_6\text{H}_3\begin{cases}\text{ЄH}_3\\\Theta\text{ЄH}_3\\\Theta\text{H}\end{cases}$ precisely as pyro-catechin was in the previous fraction, from guaiacol. Marasse hence concludes that Rhenish beech-wood creosote is a mixture of compounds belonging to two parallel series, the phenols and the acid methyl ethers of pyro-catechin and its homologues. And since the first members of the series do not coincide in boiling point, the first member of the guaiacol series agreeing with the second member of the phenol series, it is obvious that that portion of creosote which boils at the lowest temperature will consist of the first member of the phenol series, i. e., phenol itself.

Phenol Series.	Boiling point.	Guaiacol Series.	Boiling point.
Phenol, $\text{Є}_6\text{H}_5(\Theta\text{H})$	184° C.	....	....
Cresol, $\text{Є}_6\text{H}_4\begin{cases}\text{ЄH}_3\\\Theta\text{H}\end{cases}$	203°	Guaiacol, $\text{Є}_6\text{H}_4\cdot\begin{cases}\Theta\text{H}\\\Theta\text{ЄH}_3\end{cases}$	200° C.
Phlorol, $\text{Є}_6\text{H}_3\begin{cases}\text{ЄH}_3\\\text{ЄH}_3\\\Theta\text{H}\end{cases}$	220°	Creosol, $\text{Є}_6\text{H}_3\begin{cases}\text{ЄH}_3\\\Theta\text{ЄH}_3\\\Theta\text{H}\end{cases}$	219°

All the different kinds of beech-wood creosote appear to be identical in composition; those specimens having the highest boiling point, which contain the higher members of these parallel series.—*Ann. Ch. Pharm.*, clii, 59, Oct., 1869. G. F. B.

16. *On Ocean Currents, in relation to the Distribution of Heat over the Globe*, by JAMES CROLL of the Geological Survey of Scotland. (Phil. Mag., Feb. 1870.)—I. *The absolute Heating-power of Ocean-currents.*—* * * From an examination of the published sections [of the Gulf Stream] some years ago,* I came to the conclu-

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* Philosophical Magazine for February, 1867, p. 127.

sion that the total quantity of water conveyed by the stream is probably equal to that of a stream 50 miles broad and 1000 feet deep,* flowing at the rate of four miles an hour, and that the mean temperature of the entire mass of moving water is not under 65° at the moment of leaving the Gulf. I think we are warranted to conclude that the stream, before it returns from its northern journey, is on an average cooled down to at least 40°; consequently it loses 25° of heat. Each cubic foot of water, therefore, in this case carries from the tropics for distribution upwards of 1500 units of heat, or 1,158,000 foot-pounds. According to the above estimate of the size and velocity of the stream, 5,575,680,000,000 cubic feet of water are conveyed from the Gulf per hour, or 133,816,320,000,000 cubic feet daily. Consequently the total quantity of heat transferred from the equatorial regions per day by the stream amounts to 154,959,300,000,000,000,000 foot-pounds.

From observations made by Sir John Herschel and by M. Pouillet on the direct heat of the sun, it is found that, were no heat absorbed by the atmosphere, about 83 foot-pounds per second would fall upon a square foot of surface placed at right angles to the sun's rays.† Mr. Meech estimates that the quantity of heat cut off by the atmosphere is equal to about 22 per cent of the total amount received from the sun. M. Pouillet estimates the loss at 24 per cent. Taking the former estimate, 64·74 foot-pounds per second will therefore be the quantity of heat falling on a square foot of the earth's surface when the sun is in the zenith. And were the sun to remain stationary in the zenith for twelve hours, 2,796,768 foot-pounds would fall upon the surface.

It can be shown that the total amount of heat received upon a unit surface on the equator during the twelve hours from sunrise till sunset at the time of the equinoxes is to the total amount which would be received upon that surface, were the sun to remain in the zenith during those twelve hours, as the diameter of a circle to half its circumference, or as 1 to 1·5708. It follows, therefore, that a square foot of surface on the equator receives from the sun at the time of the equinoxes 1,780,474 foot-pounds daily, and a square mile 49,636,750,000,000, foot-pounds daily. But this amounts to only $\frac{1}{3121870}$ part of the quantity of heat daily conveyed from the tropics by the Gulf-stream. In other words, the Gulf-stream conveys as much heat as is received from the sun by 3,121,870 square miles at the equator. The amount thus conveyed is equal to all the heat which falls upon the globe within 63 miles on each side of the equator. According to calculations made by Mr. Meech,‡ the annual quantity of heat received by a unit surface on the frigid zone, taking the mean of the whole zone, is $\frac{5\cdot45}{12}$ of that received at the equator; consequently the quantity of heat conveyed by the Gulf-stream in one year is equal to the heat which falls on an average on 6,873,800 square miles of the arctic regions. The frigid zone or arctic regions contain 8,130,000 square miles. There is actually, therefore, nearly as much heat transferred from tropical regions by the Gulf-stream as is received from the sun by the entire arctic regions, the quantity conveyed by the stream to that received from the sun by those regions being as 15 to 18.

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* The Gulf-stream at the narrowest place examined by the Coast Survey, and the place where its velocity was greatest, was found to be over 30 statute miles broad and 1950 feet deep. But we must not suppose this represents all the warm water which is received by the Atlantic from the equator; a great mass of water flows into the Atlantic without passing through the Straits of Florida.

† Trans. of Roy. Soc. of Edinb., vol. xxi, p. 57. Phil. Mag., S. 4, vol. ix, p. 36.

‡ Smithsonian Contributions to Knowledge, vol. ix.

But we have been assuming in our calculations that the percentage of heat absorbed by the atmosphere is no greater in polar regions than it is at the equator, which is not the case. If we make due allowance for the extra amount absorbed in polar regions in consequence of the obliqueness of the sun's rays, the total quantity of heat conveyed by the Gulf-stream will probably nearly equal the amount received from the sun by the entire arctic regions.

If we compare the quantity of heat conveyed by the Gulf-stream with that conveyed by means of aërial currents, the result is equally startling. The density of air to that of water is as 1 to 770, and its specific heat to that of water is as 1 to 4·2; consequently the same amount of heat that would raise 1 cubic foot of water 1° would raise 770 cubic feet of air 4°·2, or 3234 cubic feet 1°. The quantity of heat conveyed by the Gulf-stream is therefore equal to that which would be conveyed by a current of air 3234 times the volume of the Gulf-stream, at the same temperature and moving with the same velocity. Taking, as before, the width of the stream at 50 miles, and its depth at 1000 feet, and its velocity at 4 miles an hour, it follows that, in order to convey an equal amount of heat from the tropics by means of an aërial current, it would be necessary to have a current about 1¼ mile deep, and at the temperature of 65°, blowing at the rate of four miles an hour from every part of the equator over the northern hemisphere towards the pole. If its velocity were equal to that of a good sailing-breeze, which Sir John Herschel states to be about twenty-one miles an hour, the current would require to be above 1200 feet deep. A greater quantity of heat is probably conveyed by the Gulf-stream alone from the tropical to the temperate and arctic regions than by all the aërial currents which flow from the equator.

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The anti-trades or upper return-currents, as we have seen, bring no heat from the tropical regions. After traversing some 2000 miles in a region of extreme cold they descend on the Atlantic as a cold current, and there absorb the heat and moisture which they carry to northeastern Europe. Those aërial currents derive their heat from the Gulf-stream, or if it is preferred, from the warm water poured into the Atlantic by the Gulf-stream. How, then, are these winds heated by the warm water? The air is heated in two ways, viz: by direct *radiation* from the water, and by *contact* with the water. Now, if the Gulf-stream continued a narrow and deep current during its entire course similar to what it is at the Straits of Florida, it could have little or no opportunity of communicating its heat to the air either by radiation or by contact. If the stream was only about 40 or 50 miles in breadth, the aërial particles in their passage across it would not be in contact with warm water more than an hour or two. Also the number of the particles in contact with the water, owing to the narrowness of the stream, would be small, and there would therefore be little opportunity for the air becoming heated by contact. The same also holds true in regard to radiation. The more we widen the stream and increase its area, the more we increase its radiating surface; and the greater the radiating surface, the greater is the quantity of heat thrown off. But this is not all; the number of aërial particles heated by radiation increases in proportion to the area of the radiating surface; consequently the wider the area over which the waters of the Gulf-stream are spread, the more effectual will the stream be as a heating-agent. And, again, in order that a very wide area of the Atlantic may be covered with the warm waters of the stream, slowness of motion is essential. * * *

The quantity of heat conveyed by the Gulf-stream, as we have seen, is equal to all the heat received from the sun by 3,121,870 square miles at the equator. Mr. Findlay, however, as has been stated, thinks that I have doubled the actual volume of the stream. Assuming that I have done so, the amount of heat carried by the stream would still be equal to all the heat received from the sun by 1,560,935 square miles at the equator. The mean annual quantity of heat received from the sun by the temperate regions per unit surface is to that received by the equator as 9·83 to 12.* Consequently the quantity of heat conveyed by the stream, taking Mr. Findlay's estimate of its volume, is equal to all the heat received from the sun by 2,062,960 square miles of the temperate regions. The total area of the Atlantic from the latitude of the Straits of Florida, 200 miles north of the tropic of Cancer, up to the Arctic Circle, including also the German Ocean, is about 8,500,000 square miles. In this case the quantity of heat carried by the Gulf-stream into the Atlantic through the Straits of Florida, to that received by this entire area from the sun, is as 1 to 4·12, or in round numbers as 1 to 4. It therefore follows that one-fifth of all the heat possessed by the waters of the Atlantic over that area, even supposing that they absorb every ray that falls upon them, is derived from the Gulf stream. Would those who call in question the efficiency of the Gulf-stream be willing to admit that a decrease of one-fourth in the total amount of heat received from the sun, over the entire area of the Atlantic from within 200 miles of the tropical zone up to the arctic region, would not sensibly affect the climate of Northern Europe? If they would not willingly admit this, why, then, contend that the Gulf-stream does not affect climate? for the stoppage of the Gulf-stream, taking it at Mr. Findlay's estimate, would deprive the Atlantic of 77,479,650, 000,000,000,000 foot-pounds of energy in the form of heat per day, a quantity equal to one-fourth of all the heat received from the sun by that area.

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* See Smithsonian Contributions to Knowledge, vol. ix.

Were the sun extinguished, the temperature over the whole earth would sink to nearly that of stellar space, which, according to the investigations of Sir John Herschel[*] and of M. Pouillet,[†] is not above —239° F. Were the earth possessed of no atmosphere, the temperature of its surface would sink to exactly that of space, or to that indicated by a thermometer exposed to no other heat-influence than that of radiation from the stars. But the presence of the atmospheric envelope would slightly modify the conditions of things; for the heat from the stars (which of course constitutes what is called the temperature of space) would, like the sun's heat, pass more freely through the atmosphere than the heat radiated back from the earth, and there would in consequence of this be an accumulation of heat on the earth's surface. The temperature would therefore stand a little higher than that of space; or, in other words, it would stand a little higher than it would otherwise do were the earth exposed in space to the direct radiation of the stars without the atmospheric envelope. But, for reasons which will presently be stated, we may in the mean time, till further light is cast upon this matter, take —239° F. as probably not far from what would be the temperature of the earth's surface were the sun extinguished.

Suppose, now, that we take the mean annual temperature of the Atlantic at, say, 56°.[‡] Then 239°+56°=295° represents the number of degrees of rise due to the heat which it receives. In other words, it takes all the heat that the Atlantic receives to maintain its temperature 295° above the temperature of space. Stop the Gulf-stream, and the Atlantic would be deprived of one-fifth of the heat which it possesses. Then, if it takes five parts of heat to maintain a temperature of 295° above that of space, the four parts which would remain after the stream was stopped would only be able to maintain a temperature of four-fifths of 295° or 236° above that of space: the stoppage of the Gulf-stream would therefore deprive the Atlantic of an amount of heat which would be sufficient to maintain its temperature 59° above what it would otherwise be, did it depend alone upon the heat received directly from the sun. It does not, of course, follow that the Gulf-stream actually maintains the temperature 59° above what it would otherwise be were there no ocean-currents, because the actual heating-effect of the stream is neutralized to a very considerable extent by cold currents from the arctic regions. But 59° of rise represent its actual power; consequently 59°, minus the lowering effect of the cold currents, represent the actual rise. What the rise may amount to at any particular place must be determined by other means. * * *

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[*] 'Meteorology,' Section 36.

[†] Comptes Rendus, July 9, 1838. Taylor's Scientific Memoirs, vol. ix, p. 44, (1846).

[‡] The mean temperature of the Atlantic between the tropics and the arctic circle, according to Admiral FitzRoy's chart, is about 60°. But he assigns far too high a temperature for latitudes above 50°. It is probable that 56° is not far from the truth.

At present there is a difference merely of 80° between the mean temperature of the equator and the poles;* but were each part of the globe's surface to depend alone upon the direct heat which it receives from the sun, there ought, according to theory, to be a difference of more than 200°. The annual quantity of heat received at the equator to that received at the poles, supposing the proportionate quantity absorbed by the atmosphere to be the same in both cases, is as 12 to 4·98, or, say, as 12 to 5. Consequently if the temperatures of the equator and the poles be taken as proportionate to the absolute amount of heat received from the sun, then the temperature of the equator above that of space must be to that of the poles above that of space as 12 to 5. What ought, therefore, to be the temperatures of the equator and the poles, did each place depend solely upon the heat which it receives directly from the sun? Were all ocean and aërial currents stopped, so that there could be no transference of heat from one part of the earth's surface to the other, what ought to be the temperatures of the equator and the poles? We can at least arrive at a rough estimate on this point. If we diminish the quantity of warm water conveyed from the equatorial regions to the temperate and arctic regions, the temperature of the equator will begin to rise and the temperature of the poles to sink. It is probable, however, that this process would affect the temperature of the poles more than it would do that of the equator; for as the warm water flows from the equator to the poles, the area over which it is spread becomes less and less. But as the water from the tropics has to raise the temperature of the temperate regions as well as the polar, the difference of effect at the equator and poles might not, on that account, be so very great. Let us take a rough estimate. Say that, as the temperature of the equator rises one degree, the temperature of the poles sinks about one degree and a half. The mean annual temperature of the globe is about 58°. The mean temperature of the equator is 80°, and that of the poles 0°. Let ocean and aërial currents now begin to cease, the temperature of the equator begins to rise and the temperature of the poles to sink. For every degree that the equator rises the poles sink 1½°; and when the currents are all stopped and each place dependent alone upon the direct rays of the sun, the mean annual temperature of the equator above that of space will be to that of the poles, above that of space, as 12 to 5. When this proportion is reached, the equator will be 374° above that of space, and the poles 156°; for 374 is to 156 as 12 is to 5. The temperature of space we have seen to be —239°, consequently the temperature of the equator will in this case be 135°, reckoned from the zero of the Fahrenheit thermometer, and the poles 83° below zero. The equator would therefore be 55° warmer than at present, and the poles 83° colder. The difference between the temperature of the equator and the poles will in this case amount to 218°.

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* The mean temperature of the equator, according to Dove, is 79°·7, and that of the north pole 2°·3. But as there is of course some uncertainty regarding the actual mean temperature of the poles, we may take the difference in round numbers at 80°.

Now, if we take into account the quantity of positive energy in the form of heat carried by warm currents from the equator to the temperate and polar regions, and also the quantity of negative energy (cold) carried by cold currents from the polar regions to the equator, we shall find that they are sufficient to reduce the difference of temperature between the poles and the equator from 218° to 80°.

The quantity of heat received in the latitude of London, for example, to that received at the equator is about as 12 to 8. This, according to theory, should produce a difference of about 125°. The temperature of the equator above that of space, as we have seen, would be 374°. Therefore 249° above that of space would represent the temperature of the latitude of London. This would give 10° as its temperature. The stoppage of all ocean and aërial currents would thus increase the difference between the equator and the latitude of London by about 85°. The stoppage of ocean-currents would not be nearly so much felt, of course, in the latitude of London as at the equator and the poles, because, as has been already noticed, in all latitudes midway between the equator and the poles the two sets of currents to a considerable extent compensate each other; viz. the warm currents from the equator raise the temperate, while the cold ones from the poles lower it; but as the warm currents chiefly keep on the surface and the cold return-currents are principally under-currents, the heating effect very greatly exceeds the cooling effect. Now, as we have seen, the stoppage of all currents would raise the temperature of the equator 55°; that is to say, the rise at the equator alone would increase the difference of temperature between the equator and that of London by 55°. But the actual difference, as we have seen, ought to be 85°; consequently the temperature of London would be lowered 30° by the stoppage of the currents. For if we raise the temperature of the equator 55° and lower the temperature of London 30°, we then increase the difference by 85°. The normal temperature of the latitude of London being 40°, the stoppage of all ocean and aërial currents would thus reduce it to 10°. But the Gulf-stream raises the actual mean temperature of London 10° above the normal. Consequently 30°+10°=40° represents the actual rise at London due to the influence of the Gulf-stream over and above all the lowering effects resulting from arctic currents. On some parts of the American shores on the latitude of London, the temperature is 10° below the normal. The stoppage of all ocean and aërial currents would therefore lower the temperature there only 20°.

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[The author next shows that the climate of the arctic must be affected by the Gulf-stream; and also that the low temperature of the southern hemisphere is owing to the Ocean-currents].

Without Ocean-currents the Globe would not be habitable.—All these foregoing considerations show to what an extent the climatic condition of our globe is due to the thermal influences of ocean-currents.

As regards the northern hemisphere, we have two immense oceans, the Pacific and the Atlantic, extending from the equator to near the north pole, or perhaps to the pole altogether. Between these two oceans lie two great continents, the eastern and the western. Owing to the earth's spherical form, by far too much heat is received at the equator and by far too little at high latitudes to make the earth a suitable habitation for sentient beings. The function of these two great oceans is to remove the heat from the equator and carry it to temperate and polar regions. Aërial currents could not do this. They might remove the heat from the equator, but they could not, as we have already seen, carry it to the temperate and polar regions; for the greater portion of the heat which aërial currents remove from the equator is dissipated into stellar space: the ocean can alone convey the heat to distant shores. But aërial currents have a most important function; for of what avail would it be, though ocean-currents should carry heat to high latitudes, if there were no means of spreading the heat thus conveyed over the land? The function of aërial currents is to do this. Upon this twofold arrangement depends the thermal condition of the globe. Exclude the waters of the Pacific and the Atlantic from temperate and polar regions and place them at the equator, and nothing now existing on the globe could live in high latitudes.

Were these two great oceans placed beside each other on one side of the globe, and the two great continents placed beside each other on the other side, the northern hemisphere would not then be suitable for the present order of things: the land on the central and on the eastern side of the united continent would be by far too cold. * * *

II. GEOLOGY.

1. *Sun-Pictures of Rocky Mountain Scenery;* by Dr. F. V. HAYDEN, Prof. of Mineralogy and Geology in the University of Pennsylvania.—We have been permitted to make the following extracts from a work of the above title, by Dr. Hayden, which is soon to be published by Julius Bien of New York.* The volume will contain thirty photographic views along the line of the Pacific railroad from Omaha to Sacramento, and besides a description of the geographical and geological features of the country, by Dr. Hayden, whose researches over the Rocky Mountains have often been chronicled in this Journal. We understand that the views selected for publication are generally those that illustrate the geographical and geological features of the region, and are interesting to the student of science as well as to the artist and the lovers of the picturesque in nature.

Communications on the Fossil Plants, Mammals, Fishes, Insects, &c., have been contributed by Messrs. Newberry, Leidy, Cope, and

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* The work will be issued to subscribers only, at $25 per copy, and subscriptions should be addressed to Julius Bien, lithographer, 16 and 18 Park Place, New York City.

Scudder, and our extracts are from these papers, and from some of the notes of Dr. Hayden, furnished us by him.

Photograph No. 9, illustrates a peculiar feature of the surface near the junction of Hilter Creek with Green river. The formations are composed of thin layers of fine sand, clay and sandstones or chalky limestones readily disintegrating on the surface, and, as it were, breaking down, so that the water has worn an almost unlimited series of furrows or small ridges with considerable uniformity; sometimes on the summits of the hills or ridges harder portions are left, which wear into castellated forms. This peculiar appearance of the surface is not uncommon all over the west where the Cretaceous and Tertiary formations prevail, especially where they are composed of rather soft clays and sands. The black plastic clays of the Cretaceous beds on the Upper Missouri are subject to this style of weathering, as well as much of the country usually termed the "Bad Lands."

This appearance of the surface carries with it, also, the aspect of desolation. There is little vegetation but the wild sage, Chenopodiaceous shrubs, and other plants which love the alkaline soils. Little depressions occur here and there in which the surface waters accumulate in wet weather; but in the dry season the water evaporates and the surface is left with a thick incrustation of salts of soda, magnesia, &c. I have called the formation along Green river, the Green river shales, from the fact that the sediments are arranged in regular layers, mostly thin like shale, varying, however, in thickness, from that of a knife blade to several feet. This laminated character with the slight variations in color give to the hills the peculiar branded appearance as shown in Photographs 11 and 12 of "Citadel Rock" and "Castle Rock."

Photograph No. 10, illustrates a cut along the railroad through thin layers of a sort of cream-colored chalky limestone. Some of the layers are of a dark brown color and so saturated with petroleum that they burn with a good deal of freedom. This cut is usually called the "Burning Rock cut" from the fact that during the progress of the work, the men built a fire by the side of one of the walls, and the rocks ignited, burning for some days, illuminating the labors of the workmen by night, and filling the valley with a dense smoke by day. In the distance may be seen the banks of Green river formed of similar rocks, which are made up for the most part of silica, lime, and some clay, excellent material for the preservation of organic remains. Besides, all the rocks are more or less impregnated with the oily substance which no doubt originated from the vast quantities of animals which existed in this lake, the remains of which are found in the greatest abundance every where. One of these excavations along the railroad bears the name of the "Petrified Fish Cut," on account of the thousands of beautiful and perfect impressions of fishes which are shown on the surface of the thin slabs or layers of rock. Sometimes a dozen or two of these little herring are found on an area of a square foot. Insects, water plants, and a remarkable specimen of a *feather of a bird* has been found here. The feather Prof. Marsh regards as a unique specimen, forming a most interesting addition to the bird remains of North America. It is the distal portion of a large feather, with the shaft and vane in such excellent preservation that it may perhaps indicate approximately the nature of the bird to which it belonged.

My collection of *fossil fishes* from these shales was very large, and my success was mostly due to the kind aid of Mr. A. W. Hilliard, a gentleman of intelligence, who superintended the excavations on the line of the railroad, and preserved from time to time such specimens of value as came to his observation.

Prof. E. D. Cope, a distinguished comparative anatomist of Philadelphia, has kindly prepared the following *account of the petrefied fish remains* which were submitted to him for examination, especially for this work. Prof. Cope says:

"The fishes placed in my hands for determination by you, consist of four species, viz: one Acanthopterygian, *Asineops squamifrons* Cope, and three Malacopterygians, *Clupea humilis* Leidy, *Clupea pusilla* Cope, *Cyprinodon levatus* Cope. Those named by the writer were not previously known, and the Asineops represents also a genus not before brought to the notice of scientific men. In ordinary language the last mentioned fish is a perch, but in no degree similar to the white and yellow brought to our markets. The nearest resemblance in general structure is to be found in the black bass of the Ohio and Tennessee rivers, but a closer similarity in form exists in the Red Eye or Goggled Eyed perch of western and southern rivers, Amblopiltes of Naturalists. Zoologically it is not very nearly related to either, for it combines with some of their characters, others now existing only in marine fishes of other families. It is an aberrant form of the family of Chætodons, which embraces marine fishes only, and which chiefly abound at the present time in the Indian and Pacific Oceans. But the form and proportions of its fins and scales remind one very much of the swamp and the tide-water sun perch of New Jersey, (*Acantharchius pomotis* Baird, *Bryttus* sp. etc.), and suggest a similarity of habit. The teeth are fine and the dentition brush-like as in the fresh water and many marine perch, and its food was probably much like theirs. As far as zoological evidence goes, there is nothing to indicate whether this species belonged to fresh or salt water; its unarmed character constitutes a pecularity much more prevalent among fresh water than marine fishes, while its zoological affinities, so far as known, are altogether with marine forms. In size this fish exceed the red eye, and was less than the black bass, averaging about as the yellow perch.

"The Clupeas are herring of small species, considerably less than the herring of our coasts. One of the blocks contains the remains of two small shoals of the fry, probably of *Clupea humilis* which were caught suddenly by a slide or fall of calcareous mud, and entombed for the observation of future students. They must have been taken unawares, since they lie with their heads all in one direction as they swam in close bodies. One or two may have had a moment's warning of the catastrophe, as they have turned a little aside, but they are the exceptions. The fry are from one-half to three quarters of an inch long and upwards.

"The herring, or those without teeth, are chiefly marine, but they run into fresh water to deposit their spawn in the spring of the year, and then return to salt waters. The young run down to the sea in autumn and remain there till old enough to spawn. The size of the fry of the Rocky Mountain herring indicates that they had not long left their spawning ground, while the abundance of adults suggests that they were not far from salt water, their native element. To believe then that the locality from which the specimens were taken, was neither far from fresh nor far from salt water, is reasonable, and this points to a tide or brackish inlet or river. Lastly, the species of Cyprinodon inhabit also tide and brackish waters. Most of the species of the family, as well as of the genus, are inhabitants of fresh water, but they generally, especially the Cyprinodons proper, prefer still and muddy localities, and often occur in water really salt. This habitat distinguishes them especially from Cyprinidæ (minnows and suckers) and Pike. The materials which compose the shales indicate quiet water, and not such as is usually selected by herring for spawning in, while the abundance of adult Clupeas indicate the proximity of salt water. This is far from a satisfactory demonstration of the nature of the water which deposited this mass of shales, but it is the best that can be attained with such a meagre representation of species.

"As to geological age, the indications are rather more satisfactory. The genus Clupea ranges from the Upper Eocene upward, being abundant in the slates of Lebanon and Monte Bolca, while Cyprinodon has been found in neither, but first appears in the middle or lower Miocene in Europe. The Asineops resemble very closely, and I believe essentially, the Pygaeus of Agassiz, of Eocene age, from Monte Bolca. The peculiarities presented by the genus found by Dr. Hayden are of such small significance as to lead me to doubt the beds in question being of later than Eocene in age, though the evidence rests chiefly on this single new and peculiar genus.

"The position of these fishes, 7,000 feet above the level of the sea, furnishes another illustration of the extent of the elevations of regions once connected with the ocean, and the comparatively late period of geological time at which, in this case, this elevation took place."

The *fossil Insects* were examined by Mr. Samuel H. Scudder, of Boston, and he has kindly prepared the following short but very interesting note. The insects were found in the same locality with the fishes.

"The fossil insects found by Dr. Hayden in the tertiary shales of Green river, belong to three species, one being an ant, the others flies. The ant is rather poorly preserved and must be examined with great care before its precise character can be determined.

The larger fly, of which we can distinguish almost the whole of the body, though but little of the wings, evidently belonged to some species of *Syrphidæ*; it is nearly three quarters of an inch in length and seems to have had a bright colored abdomen, banded with black. In their perfect state, all of this family are very fond of flowers, but the typical species are particularly interesting from the peculiar habits of the larvæ; these are footless grubs which feed on plant lice, piercing them one by one, and sucking out their juice; it is more likely, however, that our species belonged to one of the genera whose larvæ live in the water, or about decaying vegetable substances; larvæ, which from their size and general form might well produce such a fly as this, were found abundantly by Professor Denton; they evidently inhabited the water.

The other fly, judging from the neuration of the wings, which is pretty well preserved, seems to belong to the great family of *Muscidæ*, of which the common house fly is an example; it may probably be referred to a section in which they are nearly or quite wanting, and where the larvæ ordinarily feed upon dead animal matter or upon decomposing plants. Belonging to the same group and perhaps already allied, are the species of *Ephydra* which live in salt marshes, and frequent salt pans to such an extent as to be very troublesome; they have been discovered in the saline waters of some Nevadan lakes, and Professor Denton states that dipterous larvæ, probably of a similar kind, are found in great numbers in lakes impregnated with petroleum. It is probable that the shales, in which these remains occur, were deposited in such a lake."

About a mile west of the "Petrified Fish Bed," is a cut along the railroad which passes through a moderate thickness of buff, chalky limestone filled with impressions of leaves of deciduous trees. These rocks hold a position about one hundred feet above the petroleum shales which contain the fish remains, and therefore the date of their existence may be regarded as somewhat subsequent, though belonging to the same basin. Prof. J. S. Newberry has given these plants a partial examination, and communicated the following interesting notes in the form of a letter.

"I have examined the plants from the Green River beds with as much care as the limited time at my command would permit, and am surprised in not finding among them a single species contained in any of your other great collections at the far west. They, thus far, afford no certain criteria for collating the Green River Tertiaries with those of other localities where you have studied them. The plants from the rocks enclosing the coal at Marshall's mine are more significant, as they include species (*Platanus Haydeni*, which is certainly different from *P. aceroides*), such as were found by you at Carbon Station, and at the mouth of the Yellow Stone. Every collection of fossil plants received from the Tertiary of the west brings to light many new species, and the great diversity which they exhibit proves either a number of plant-bearing horizons, or great localization of the species in the Tertiary flora.

"Among your Green River plants, are only some half dozen species, so well preserved as to be capable of satisfactory identification or comparison, but they form a very interesting group. Among them I find two palms, both quite unlike anything before found on this continent. One is a new *Phenicites*, resembling Heer's *Manicaria formosa.* The other but an imperfect fragment, is yet altogether new and strange to me. The most abundant species contained in the collection is a *Magnolia* allied to *M. tenninervis* Lesqx., but more elongate and acute—also an oak resembling *Quercus Saffordi* Lesqx. There is another oak in the collection, a laurel (probably), fragments of two ferns too imperfect for determination. On the whole, these plants resemble most those described by Lesquereux from Mississippi, and I am inclined to suspect that they are of the same age. This would make the Green River beds older than you have thought them, and I should want more material before venturing anything more than a suggestion to that effect. I trust you will be able to make other collections from these plant-beds during the present season.

"The specimens contained in the bluff marly limestones of the Green River series are generally not well preserved, and yet I think careful search at the locality where these plants sent me were obtained would result in the discovery of some fine things. I would especially urge a search for fruits.

"The aspect of the small group of plants now before me from Green River, is more tropical than any you have brought from the west, and, as we have reason to believe that our Eocene climate was warmer than the Miocene, and that from the Eocene epoch to the Glacial period a progressive depression of temperature took place, the Green River beds would seem to be of early rather than of late Miocene."

2. *On the Graphite of the Laurentian of Canada;* by J. W. Dawson. (Jour. Geol. Soc., 1869, 112).—In this paper Dr. Dawson sustains the view that the graphite of the Canada Laurentian is of organic origin, and shows that the amount of "graphite in the Lower Laurentian Series is enormous." A limestone in the township of Buckingham on the Ottawa, which is 600 feet or more thick, with some three intercalated bands of gneiss, is in some parts *one-fourth* graphite, and the whole is not less than 20 or 30 per cent graphite. In the adjoining township of Lochaber, a band of limestone 25 to 30 feet thick is so reticulated with graphite, that it is mined for it; and another bed in the same district, 10 to 12 feet thick, yielding 20 per cent of the pure material, is worked. It occurs in equal abundance at other horizons through beds of limestone which have, according to Logan, an aggregate thickness of 3500 feet. In view of the facts Dr. Dawson adds "it is scarcely an exaggeration to maintain that the quantity of carbon in the Laurentian is equal to that in similar areas of the Carboniferous system."

On the mode of occurrence and origin the author observes:

"The beds of graphite near St. John, some of those in the gneiss at Ticonderoga in New York, and at Lochaber and Buckingham and elsewhere in Canada are so pure and regular that one might fairly compare them with the graphitic coal of Rhode Island. These instances, however, are exceptional, and the greater part of the disseminated and vein graphite might rather be compared in its mode of occurrence to the bituminous matter in bituminous shales and limestones. We may compare the disseminated graphite to that which we find in those districts of Canada in which Silurian and Devonian bituminous shales and limestones have been metamorphosed and converted into graphitic rocks not dissimilar to those in the less altered portions of the Laurentian.* In like manner it seems probable that the numerous reticulating veins of graphite may have been formed by the segregation of bituminous matter into fissures and planes of least resistance, in the manner in which such veins occur in modern bituminous limestones and shales. Such bituminous veins occur in the Lower Carboniferous limestone and shale of Dorchester and Hillsborough, New Brunswich, with an arrangement very similar to that of the veins of graphite; and in the Quebec rocks of Point Levi, veins attaining to a thickness of more than a foot are filled with a coaly matter having a transverse columnar structure and regarded by Logan and Hunt as an altered bitumen. These paleozoic analogies would lead us to infer that the larger part of the Laurentian graphite falls under the second class of deposits above mentioned, and that, if of vegetable origin, the organic matter must have been thoroughly disintegrated and bituminized before it was changed into graphite. This would also give a probability that the vegetation implied was aquatic, or at least that it was accumulated under water.

"Dr. Hunt has, however, observed an indication of terrestrial vegetation, or at least subaërial decay, in the great beds of Laurentian iron-ore. These, if formed in the same manner as more modern deposits of this kind, would imply the reducing and solvent action of substances produced in the decay of plants. In this case such great ore beds as that of Hull, on the Ottawa, 70 feet thick, or that near Newborough, 200 feet thick,† must represent a corresponding quantity of vegetable matter which has totally disappeared. It may be added that similar demands on vegetable matter as a deoxydizing agent are made by the beds and veins of metallic sulphids of the Laurentian, though some of the latter are no doubt of later date than the Laurentian rocks themselves."

He concludes as follows:

"We may sum up these facts and considerations in the following statements:—First, that somewhat obscure traces of organic structure can be detected in the Laurentian graphite; secondly, that the general arrangement and microscopic structure of the substance corresponds with that of the carbonaceous and bituminous matters in marine formations of more modern date; thirdly, that if the Laurentian graphite has been derived from vegetable matter, it has only undergone a metamorphosis similar in kind to that which organic matter in metamorphosed sediment of later age has experienced; fourthly, that the association of the graphitic matter with organic limestone, beds of iron ore, and metallic sulphids greatly strengthens the probability of its vegetable origin; fifthly, that when we consider the immense thickness and extent of the Eozoonal and graphitic limestones and iron-ore deposits of the Laurentian, if we admit the organic origin of the limestone and graphite, we must be prepared to believe that the life of that early period, though it may have existed under low forms, was most copiously developed, and that it equalled, perhaps surpassed, in its results, in the way of geological accumulation, that of any subsequent period.

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* Granby, Melbourne, Owl's Head, &c., 'Geology of Canada,' 1863, p. 599.

† Geology of Canada, 1863.

"In conclusion, this subject opens up several interesting fields of chemical, physiological and geological inquiry. One of these relates to the conclusions stated by Dr. Hunt as to the probable existence of a large amount of carbonic acid in the Laurentian atmosphere, and of much carbonate of lime in the seas of that period, and the possible relation of this to the abundance of certain low forms of plants and animals. Another is the comparison, already instituted by Professor Huxley and Dr. Carpenter, between the conditions of the Laurentian and those of the deeper parts of the modern ocean. Another is the possible occurrence of other forms of animal life than *Eozoon* and Annelids, which I have stated in my paper of 1864, after extensive microscopic study of the Laurentian limestones, to be indicated by the occurrence of calcareous fragments, differing in structure from *Eozoon*, but at present of unknown nature. Another is the effort to bridge over, by further discoveries similar to that of the *Eozoon bavaricum* of Gümbel, the gap now existing between the life of the Lower-Laurentian and that of the Primordial Silurian or Cambrian period. It is scarcely too much to say that these inquries open up a new world of thought and investigation, and hold out the hope of bringing us into the presence of the actual origin of organic life on our planet, though this may perhaps be found to have been Prelaurentian. I would here take the opportunity of stating that in proposing the name *Eozoon* for the first fossil of the Laurentian, and in suggesting for the period the name 'Eozoic,' I have by no means desired to exclude the possibility of forms of life which may have been precursors of what is now to us the dawn of organic existence. Should remains of still older organisms be found in those rocks now known to us only by pebbles in the Laurentian, these names will at least serve to mark an important stage in geological investigation.

3. *On Laurentian Rocks in Nova Scotia;* by T. Sterry Hunt, F.R.S.—The American Journal of Science for May, 1870, contains a paper by Prof. H. Y. Hind on the Geology of Nova Scotia, in which it is said that Dr. Honeyman, in the autumn of 1868, discovered in the Arisaig district, "syenites, diorites, and crystalline limestones, with serpentine, which he then supposed to be of Lau-

rentian age, as he informed me subsequently to the publication of my preliminary report on the Nova Scotia Laurentian. Specimens were sent to Montreal for examination, and instructions were given by Dr. Hunt, who also shared Dr. Honeyman's opinion, to the lapidary, to prepare sections of the serpentinous rock for microscopic examination. By some mischance this was neglected, and the specimens remained unexamined, and indeed forgotten until quite recently, as Dr. Hunt informs me, under date of Feb. 3, 1870. When submitted to the microscopic test the *Eozoon Canadense* was distinctly seen, and Dr. Dawson has confirmed the observations." Prof. Hind further says that the discovery of this fossil "enables geologists to recognize the truth of Dr. Honeyman's opinions, although, by accident, these opinions were not made known or confirmed until after the publication of my report." He again refers to "the just though tardy recognition of the correctness of Dr. Honeyman's views with reference to the age of the limestones and diorites of Arisaig." Pages 354, 355.

Prof. Hind has been deceived in this whole matter, and as he is now, and has been for some months absent in England, I feel called upon, in the interest of truth, to state the facts in the case. In the spring of 1869, Dr. Honeyman, previously employed as an explorer by the Geological Survey of Canada, showed me, in Montreal, a series of specimens collected by him the autumn previous, in the Arisaig district, and including besides syenites and diorites, crystalline limestones, sometimes mixed with a pale green serpentine. These were at once noticed by Mr. Murray of the Geological Survey of Newfoundland, who was present, and myself, as having a close lithological resemblance to the Laurentian rocks, and we mentioned the fact to Dr. Honeyman; while I at once suggested to the lapidary of the Survey, who was in the room, that they were so like the Eozoon-limestones of the Ottawa that it would be well to prepare slices for examination. Meanwhile Dr. Honeyman never made to myself, Sir William Logan, or Dr. Dawson, any suggestions as to the geological age or relations of the rocks in question, and in his official report, handed to Sir William some days later, not only neglects to mention the name of Laurentian, but forgets to make any allusion whatever to the diorites, limestones and serpentines in question. Farther, in summing up his report, he concludes that the district examined by him includes all the rocks between the coal-measures and the gold-bearing slates, thus, by implication, excluding anything lower in the geological series.

I see no reason to believe that the name of Laurentian, first applied by me to these rocks from Arisaig, in Dr. Honeyman's presence, conveyed to his mind, at the time, any notion of geological age, position, or succession, or that he attached any importance to the specimens in question, except as ornamental stones, until the appearance of Prof. Hind's first notice called his attention to the meaning of the term, and to the published descriptions of Laurentian rocks. Were it otherwise, his total silence on the subject from the autumn of 1868 to some time last winter, is simply incomprehensible.

To Prof. Hind belongs the credit of having declared that the old granitoid rocks of the region are clearly stratified gneisses, and at the base of the series in that region; as they are well known to be on the north side of the Bay of Fundy, where they have been described as Laurentian, and examined by Dr. Dawson, Matthew and Bailey. I had, however, sought in vain for Eozoon in the serpentine-limestones of New Brunswick, but on the receipt of Prof. Hind's preliminary notice, recalled the Arisaig specimens, and recognized in them a form of Eozoon, which, however, according to Dr. Dawson, is specifically distinct from *Eozoon Canadense.*

Montreal, June 7, 1870.

4. *The Ornithosauria: an elementary study of the bones of Pterodactyles;* by H. G. Seeley of St. John's College, Cambridge: with twelve plates. 132 pp. 8vo, 1870. (Deighton, Bell & Co., Cambridge; Bell & Dalby, London). *Index to the Fossil Remains of Aves, Ornithosauria, and Reptilia, from the Secondary system of strata, arranged in the Woodwardian Museum of the University of Cambridge.* By H. G. Seeley, of St. John's College, Cambridge; with a prefatory notice by the Rev. Adam Sedgwick, LL.D., F.R.S., Woodwardian Professor and Senior Fellow of Trinity College.—The author of these works, the assistant of Prof. Sedgwick at Cambridge, states in his preface that they are portions of the Catalogue of the Woodwardian Museum. This Museum, through the labors mainly of Prof. Sedgwick, is rich in remains of Reptilians from the lower Cretaceous, and is unsurpassed in those of the Cambridge upper Greensand, which has afforded large numbers of bones of Ornithosaurs (Pterosaurs). In the first of these works the able author, Mr. Seeley, reviews briefly what had before been done in connection with the subject of Pterosaurs, discusses their classification and relations, gives the details with regard to the various bones in the collections, and describes and names the several species to which they belong. The author finds that the pneumatic perforations in the bones (seen in the lower jaw, the whole vertebral column, the bones of the fore limb, the scapula and coracoid, the femur and tibia, etc.) are situated as in birds, and indicate a similar system of air-circulation from the lungs, and he argues that this implies the existence of a double heart as in birds. He also points out a relation to birds in the form of the brain, this organ having a very large cerebrum, and, as seen from above, a very small cerebellum abutting against it and pressing to either side the optic lobes (instead of having, as in ordinary Reptiles, the cerebellum behind separated from the cerebrum by the part called the optic lobes). Hence he concludes that the Ornithosaurs were hot-blooded, and makes of them a class of Vertebrates distinct from both Reptiles and Birds. The subject is illustrated by twelve plates.

5. *First Annual Report of the Geological Survey of Indiana, made during the year* 1869; by E. T. Cox, State Geologist, assisted by Prof. Frank H. Bradley, Dr. Rufus Hammond, and Dr. G. M. Levette. 240 pp. 8vo. Indianapolis, 1870.—This first annual Report of the Geological Survey of Indiana, under Prof. Cox, consists of a General Report by the head of the survey, and special Reports by Prof. Bradley on Vermillion Co., and Dr. Hammond on Franklin Co. Prof. Cox has begun a good work with reference to the coal formation, and has already proved that the number of coal beds, and the subdivisions of the formation, as laid down by Owen and others in Indiana and Kentucky, are wholly erroneous. As one example he shows that the "Mahoning Sandstone" and the "Anvil Rock Sandstone" are actually the same rock. Prof. Cox states that he is not yet prepared to present his own conclusions, but he shall make it a special object, in the course of the Survey, to ascertain the precise facts on this important subject. Much valuable information is presented by him respecting the coal formation and other rocks and products of Clay county. Prof. Bradley describes with considerable detail the Carboniferous rocks of Vermillion county, mentioning many of their fossils, and giving some account of the beds of iron ore and fire clay which they contain. He says that in this county the "boulder clay" has a depth near Perrysville of about 100 feet before reaching the underlying quicksand; and that in some places it is 125 feet thick. It contains boulders of limestone and of metamorphic rocks, which are sometimes striated, besides occasional rolled masses of galena and native copper. There is much in the volume we might quote, with interest to our readers, if space allowed. The report closes with a list of the Mammals and Birds of Franklin Co. It is illustrated by three maps, and a large plate of sections.

6. *Meek on Crinoids.*—The views of Mr. Meek on the anatomy of of the Paleozoic Crinoids, presented in a paper reprinted from the Proceedings of the Academy of Nat. Sciences of Philadelphia, in vol. xlviii of this Journal (at p. 23), are fully endorsed in a letter written by Prof. Sars of Norway, the profound investigator of species of recent Crinoids, a few days only before his death. Prof. Sars observes, judging from the photographs of the specimens which had reached him, that Mr. Meek's conclusions seem to him to be perfectly founded, and to result with logical necessity from the investigations;" and that "they spread unexpected light upon these curious extinct Crinoids." Letters from Prof. Wyville Thomson, of Belfast, and Dr. Lütken, of Copenhagen, also approve entirely of his conclusions.

7. *The lifted and subsided Rocks of America, with their influences on the oceanic, atmospheric, and land currents and the distribution of Races;* by Geo. Catlin. 228 pp., 12mo. London, 1870. (Trübner & Co.)—The writer of this work, well known for his travels among the American Indians, here treats of mountain drainage, upheavals, metamorphism, making of mountain chains, sinking of mountains, and of the Indian races of America. He

presents his geological views and criticisms with great positiveness, which is consistent with the fact of his limited knowledge of the subject.

8. *Geological Survey of Iowa.*—The State of Iowa has ordered the printing of three thousand copies of the Report of the Geology of the State by Prof. White. The work will consist of two royal octavo volumes, in the style of Hall's Geology of Iowa and of the Illinois Geological Report, and will be well illustrated. We regret to have to add that the Legislature has discontinued the Survey.

III. ZOOLOGY.

1. *Die bis jetzt bekannten Schildkröten, u. d. bei Kelheim u. Hannover neu aufgefunden altesten Arten derselben;* von Dr. G. A. Maack. Cambridge, Mass. (Cassel, 1869,—from H. von Meyer's Palæontographica).—This useful work, a 4to of 146 pp., is contributed by Dr. Maack, whose arrival in the United States and occupation in the Museum of Comparative Zoology, we take pleasure in noticing. It embraces a synopsis of the species of extinct Testudinata, arranged in the order of geological succession, rather than according to structural affinity. The number thus enumerated is 192, of which 26 are assigned to the Testudinidæ, 114 to the Emydidæ, including the Pleurodira; 27 to the Trionychidæ, and 25 to the Cheloniidæ. The species are not nearly all described, but their enumeration forms an invaluable hand-book to the student of the subject. The stratigraphical table given, adds to its value. From it we perceive that the Tertiaries embrace the majority of the species, and the Cretaceous and Jurassic periods successively fewer. The upper Jurassic of Switzerland, Bavaria and Hanover have furnished the oldest known Testidinata, unless the Chelytherium of the Würtembergian Trias belong to the order, a point still doubtful. The number of Jurassic species known was 15, to which Dr. Maack adds 3, based on remains mostly from Hanover, from a stratum of prior deposit to those of Switzerland and Bavaria.

Before noticing the types of Cretaceous and Jurassic Tortoises, it must be observed that the system of Strach which Dr. Maack adopts, is a very defective one, and far behind the requirements of modern zoology and paleontology. The structural features defining the suborders and families are overlooked in this. For example, one of the primary divisions of the order, the Pleurodira, is included among the Emydidæ, whereas it embraces a series of families distinguished by features quite similar to those defining the remaining families from each other.* In consequence several conclusions are reached which require modification. The genus *Platemys* as adopted may be cited. It embraces 9 species according to the present work, the genus Pleurosternum of Owen being referred to it. This is done because the additional pair of thoracic bones which characterizes it is found in a rudimental condition in *Platemys Bowerbankii*, and *Platemys Bullockii* of Owen presents the intermarginal scuta of *Pleurosternum*, and because of the general resemblance in specific characters between the latter and the *Pl. concinnum*. To us, however, the genus *Pleurosternum* appears to be Cryptodire not Pleurodire, as it lacks the integular scutum of the latter suborder, and to represent a peculiar family of that group characterized by the possession of ten instead of eight sternal bones. *Platemys Bullockii*, *P. Bowerbankii* and *Emys lævis* Ow. and Bell appear on the other hand to be Pleurodira, and to be referable to two families of that suborder. The *Pl. Bullockii*, on account of its five pairs of sternal bones, to the Sternothæridæ, and on account of its intermarginal scuta, to a new genus which I have called *Digerrhum*. The last two, in their rudimental fifth pair of sternals resemble many Pleurodira, and cannot be distinguished from the genus *Podocnemis* now living in the Amazon. The *P. sulcatus* Leidy is near to Podocnemis also, but represents a distinct genus, characteristic of the Cretaceous, which I call *Taphrosphys;* there are six species in North America. After these deductions the only Platemydes that remain are *P. Mantellii* and *P. Dixonii* of Owen.

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* See Proc. Acad. Nat. Sci., 1868, p. 282.

The new forms described by Maack are of much interest. His *Chelonides Wittei* is one of the group found in both Jurassic and Cretaceous strata in Europe and North America, which combines *Chelydroid* and *Chelonioid* characters so as to render it difficult to be assured as to which group they truly represent. The characters of the carapace in most, and of the plastron in many, are those of the latter, while those of the limbs, the crucial test in this case, are those of the former. Two of the North American genera add one or two costal bones, a character of importance and one not hitherto met with in the order; these may be regarded as the type of a peculiar family with the name of the *Propleuridæ*, including the genera *Osteopygis* and *Propleura*. The family with eight costals includes *Chelonides* Maack, which seems to be near *Chelonemys* Jourd., as he has placed it,—with *Platychelys*, *Hydropelta*, *Idiochelys* and some other European forms which, with *Catapleura* and *Lytoloma* from North America, are nearer *Chelydra*, and I cannot at present find characters which distinguish them as a family from the existing forms. In *Stylemys* Maack, the second new genus introduced into the present work, the sternum is without fontanelles, and resembles entirely that of *Adocus* from the American Crètaceous, while the carapace and femur are of the type of *Osteopygis*. Until further investigated it should remain as an *Emydoid*, as placed by Dr. Maack. Two species are described, *S. Lindenensis* and *S. Hannoveranus*. The use of the name *Stylemys* is a *faux pas*, since it must probably be used for a genus of *Emydidæ* described by Leidy from the Miocene and Pliocene of Nebraska and Dakota. True, it was originally established on untenable characters, and reunited by its proposer with *Testudo*. But I have been able to point out (Trans. Amer. Phil. Soc., 1869, 123) that the species so originally named are really *Emydidæ* having Testudinine characters, and requiring a distinct generic name, for which *Stylemys* should perhaps be adopted. Dr. Maack will therefore be necessitated to find another for that which he describes.

This and other rectifications relating to North American species not having been published prior to Dr. Maack's work, he was unable to take advantage of them. I therefore append the following list as supplementary to it.

POSTPLIOCENE SPECIES.

Emydidæ.

Cistudo eurypygia Cope.
Emys petrolei Leidy.

PLIOCENE AND MIOCENE SPECIES.

Emydidæ.

Stylemys Nebrascensis Leidy.
S. Niobrarensis Leidy.

Trionychidæ.

Trionyx lima Cope.
T. Buiei Cope.

Cheloniidæ.

Puppigerus grandævus Leidy.

EOCENE SPECIES.

Trionychidæ.

? *Trionyx pennatus* Cope.

? *Cheloniidæ.*

Lembonax polemicus Cope.
Puppigerus parviscutatus Cope.

CRETACEOUS SPECIES.

Emydidæ.

Emys turgidus Cope.
E. petrosus Cope.
E. firmus Leidy.
E. Stevensoni Leidy.
Adocus beatus Leidy.
Adocus agilis Cope.
A. pravus Leidy.
A. Vyomingensis Leidy.
A. pectoralis Cope. (*Pleurosternum* olim.)

Chelydridæ.

Lytoloma Jeanesii Cope.
L. angusta Cope.
Catapleura repanda Cope.
Pentresius ornatus Leidy.

Propleuridæ.

Euclastes platyops Cope.
Propleura sopita Leidy.
Osteopygis emarginatus Cope.
Osteopygis platylomus Cope.
O. chelydrinus Cope.

Trionychidæ.

Trionyx halophilus Cope.
T. priscus Leidy.
Trionyx guttatus Leidy.

Podocnemididæ.

Bothremys Cookii Leidy.
Taphrosphys molops Cope.
T. sulcatus Leidy.
T. longinuchus Cope.
Taphrosphys Leslianus Cope.
T. strenuus Cope.
T. nodosus Cope.

Thirty-eight species. *Puppigerus* embraces all of the Chelones described by Owen and Bell from Sheppey. To *Lytoloma* perhaps belong the two Chelones described and figured by Faujâs and Cuvier from the Cretaceous of Maestricht named *Ch. cretacea* by Keferstein and *Ch. Faujasii* by Maack. E. D. C.

2. *On Discosaurus and its allies;* by Dr. J. LEIDY. (Proc. Acad. Nat. Sci. Philad., 1870, p. 18).—On the 5th of April, Dr. Leidy made remarks on Discosaurus and its allies, additional to those published at p. 392 of the last volume of this Journal; and in the course of it discusses the relations of *Cimoliasaurus*, as originally established, to *Discosaurus.* On this point he observes:

"In the first place, by comparison with the skeleton of the Kansas saurian, we observe that the position in the column assigned to the vertebral bodies of *Cimoliasaurus* was incorrect, and this probably contributed to mislead Prof. Cope in his examination of the skeleton of the Kansas saurian.

"The vertebral specimens referred to *Cimoliasaurus* consisted of two sets of specimens, from two different individuals, both from the greensand of Burlington Co., N. J. They are described in 'Cretaceous Reptiles,' page 25, and characteristic ones presented in plates v and vi.

"The eleven vertebræ considered as lumbar, and represented by figs. 17–19, pl. v, and 16–18, pl. vi, are evidently cervicals. Those considered as dorsals on page 26, and represented in figs. 13–16, pl. v, are at least in part posterior cervicals. Of the fourteen vertebræ referred to on page 27 as dorsals and lumbars, those described and represented in figs. 1–5, pl. vi, are alone dorsals, while the others described and represented in figs. 6–9 are posterior, and those of figs. 10–18 more anterior cervicals.

"The cervicals of *Cimoliasaurus* are so different in their proportions from those of the Kansas saurian that there can be no question as to the distinction of the two animals, at least as species.

"Do all the remains originally referred to *Discosaurus* belong to this genus as distinct from *Cimoliasaurus?* I suspect that those from New Jersey belong to the latter. The animals indicated by all the fossils which have been under consideration are Plesiosauroid, and, as in recognized species of *Plesiosaurus*, there is much variability in the number, proportions, and other characters of the cervicals without a corresponding extent of variation in other parts of the vertebral column, we would be prepared to find in *Cimoliasaurus* nearly the same kind of caudals as in *Discosaurus.*

"Prof. Cope, in his 'Synopsis of the Extinct Batrachia and Reptilia,' pt. i, 1869, p. 56, describes two vertebral specimens from the lower bed of the Cretaceous green sand of Gloucester and Monmouth counties, which he attributes to a species with the name of *Elasmosaurus orientalis.* The specimens described as caudals are seen, by comparison with the Kansas skeleton, to be cervicals."

The species referred to in this paper—all Cretaceous—are finally as follows: 1. *Discosaurus vetustus* Leidy (Cimoliasaurus magnus, and C. vetustus, of *Cope*), from Alabama. 2. *Disc. grandis* (Brimosaurus grandis *Leidy*, Cim. grandis *Cope*), from Arkansas. 3. *Disc. carinatus* (Elasm. platyurus, and Disc. carinatus of *Cope*) from Kansas. 4. *Disc. magnus* (Cim. magnus *Leidy*, Disc. vetustus in part? *Leidy*), from New Jersey. 5. *Disc. planior* (Disc. vetustus in part, *Leidy*), from Mississippi. 6. *Disc. orientalis* (Elasm. orientalis *Cope*), from New Jersey.

3. *On Elasmosaurus platyurus* Cope; by Prof. E. D. COPE. (Communicated by the author).—I observe, in the last number of this Journal, that Prof. J. Leidy criticizes my determination of the structural characters and generic relationships of the above reptile, stating that I have reversed the direction of the vertebral column, describing the cervical as the caudal series and vice versa, and that it is the same as his previously described genus *Discosaurus*. On these points I would make the following observations.

First, as to the direction of the vertebral column, I have little doubt that Prof. Leidy is correct in his determination, especially since I have already pointed out, that, assuming the direction I gave it to be true, the vertebral articulations, and the scapular and pelvic arches, appeared to be the reverse of those of Plesiosaurus. Prof. Leidy does not, however, allude to the principal cause of this error, which was the similar reversal of the vertebral column in his descriptions of his genus *Cimoliasaurus*, first published in 1851, and re-published with 2 plates in 1864. Having my mind pre-occupied with this determination and not suspecting the error, I arranged *Elasmosaurus* in accordance with it. The great size of the clavicle, and lack of special characters of the scapular arch, as mesosternal, etc., and consequent close resemblance to many reptilian fishes, rendered the error more easy, while the coincident discovery of several reptilian forms with zygosphene articulation, attracted my atention to that character.

It might be added that the description and restoration are correct in the "Synopsis Extinct Batr. Reptilia, etc., North America," the error having appeared in a few extra copies only. Also that the accounts already given will require scarcely any modification, the caudal region like the cervical being very long, and less depressed than in *Cimoliasaurus*, etc.

Second, from the identification of *Elasmosaurus* with *Discosaurus*, I entirely dissent. Dr. Leidy, having assumed the cervicals of *Cimoliasaurus* to be lumbars, and stating it as "probable that part of the series described as lumbars may be regarded as representing sacrals and caudals,"[*] referred the true caudals of the same genus, to another supposed genus, under the name of *Discosaurus*. Anterior caudals of *Cimoliasaurus magnus* he regarded as cervicals of the new genus. But, entertaining a suspicion that the two genera might be one, he says that in this case, they "represent cervicals, dorsals and lumbars of *Discosaurus*," (i. e. *Cimoliasaurus*, the name earliest given). Having shown the identity of the two forms in accordance with the structure of *Elasmosaurus*, I failed to reverse the arrangement adopted by Prof. Leidy. Had I done so, *Cimoliasaurus* and *Discosaurus* would have become synonyms of *Plesiosaurus*, since no characters are known by which to distinguish them. As it is, I preserve the first, on the supposition that its scapular arch, will be found to present the peculiarities belonging to *Elasmosaurus.*

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[*] Cretaceous Reptiles, N. America, 29.

There are, however, several species included in Leidy's last description of *Discosaurus vetustus*, as he suggests, but believing that "the material was not sufficient to justify a separation," he allowed them to remain together. A portion of this material from New Jersey belongs undoubtedly to *Cimoliasaurus magnus*, and the other specimens (two vertebræ), which present a few peculiarities, are recorded in my Synopsis Extinct Batrachia, Reptilia, etc., as *Cimoliasaurus vetustus.* I presume that it is on these two vertebræ that Leidy bases his reference of *Elasmosaurus* to *Discosaurus.* If the evidence furnished by these was "insufficient to justify their separation" from *C. magnus*, it is certainly insufficient to justify their reference to another genus. The proximal caudals of *Elasmosaurus* and *Cimoliasaurus* are identical, but the median and distal caudals of the two are quite distinct. In *E. platyurus* they present a deep median groove beneath and a rib-like elevation on each side. No such vertebræ have been described as referable to *Cimoliasaurus*, and there is no evidence to prove that the slightly angulate caudals among those referred to *C. vetustus* by Prof. Leidy did not belong to the medial caudal region of a *Cimoliasaurus.** In a little notice furnished to LeConte's report on the Geology of the Union Pacific R. R., southern division, written almost as soon as I received the fossil, I temporarily referred the caudals to *Discosaurus*, not being generally willing to establish a new genus on caudal vetebræ or other distal portions.

In conclusion, it may be summarily stated that: 1, *Discosaurus* was erroneously constituted; 2, that characters separating it from *Plesiosaurus* were not adduced; 3, that it was not distinguished from *Cimoliasaurus;* 4, that *Discosaurus vetustus* embraces at least two species, one of which is *Cimoliasaurus magnus ;* and, 5, the other cannot be proven to be an *Elasmosaurus*, but scarcely differs from corresponding parts of *Cimoliasaurus magnus.*

4. *A new species of Tapir, from Guatemala ;* by Prof. THEODORE GILL. (Extract from a letter to one of the Editors).—The Smithsonian Institution has now the skulls of four adults and one young of the Tapir of Guatemala, and strange as it may appear, they seem conclusively to prove that the Tapir of that region is a different species from that of Panama, belonging, however, to the same genus—*Elasmognathus.* The most obvious differences are in the development of the nasal and frontal bones, but those are confirmed by the differences in the dentition, especially in the form of the first premolar of each jaw. The nasal bones of the young, compared with those of the corresponding age of *E. Bairdii*,

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* It can hardly be doubted that the median and distal caudals of Cimoliasaurus are angulate beneath, to produce terminal planes for the chevrons, in accordance with the structure of Plesiosaurus.

are wider, especially in front of the "pits," and exhibit basilar processes recurrent forward along the frontal bones, like those of *Tapirus*, but less developed, and the grooves for the nasal cartilages are deeper. As the animal advances in age, however, the frontals would appear to grow forward and force apart the nasals, which apparently do not increase, or even diminish in size, and sooner or later are fused with the frontals! This strange outgrowth of the frontal bones has been verified on four adults, and consequently the natural suggestion, that it was a monstrous individual feature, is rendered improbable. The first premolars of the Guatemalan form, in comparison with those of *E. Bairdii*, are in the upper jaw much narrower and divided into halves, the anterior of which is compressed and of almost uniform width, while the inner face of the posterior half bulges abruptly inward; in the lower jaw, they are also narrower, and the anterior cusp is separated by a vertical groove on the inner face of the tooth. All the specimens were obtained by Capt. John M. Dow, who "was told by the party who gave [him] the skulls that the young *are not* marked on the body with longitudinal light colored stripes, like *E. Bairdii*," and he believes that "this want of marking is evidently constant in the young of the species found in Guatemala." This, if confirmed by Capt. Dow himself from autopsy, would furnish a remarkable exception in the family; but without attaching undue importance to the statement, the differences already enumerated seem sufficient to establish its specific rank, and I shall describe it at length under the name *Elasmognathus Dowii.* I may add that Prof. Baird from the first has insisted on its distinctness.

5. *Contributions to the Theory of Natural Selection: A Series of Essays;* by Alfred Russell Wallace, author of the Malay Archipelago, etc. 384 pp. 8vo. London, 1870. (Macmillan & Co). —Mr. Wallace, the author of this work, has the credit of being an independent originator of the doctrine of "Natural Selection." His first memoir, published in 1855, led to the publication by Darwin of his first work (in 1859) on the Origin of Species, as Darwin states in his preface. Darwin's views had been partly in manuscript for more than ten years, and had been made known to Lyell in 1844. Wallace's work will be read with interest by all who would acquaint themselves with the arguments and facts upon which the hypothesis of Natural Selection rests, and also for its original and curious observations on the geographical distribution and habits of some animals. The author differs widely on some points from Darwin and Huxley. He has a chapter, evincing profound and earnest thought, on the Limits of Natural Selection as applied to Man, in which he shows that Man could not have been made through Natural Selection, appealing for proof to the size of his brain; his naked and soft skin; his feet and hands; man's mental faculties, and his moral sense. He speaks of man as an ever-advancing, spiritual being, the ultimate aim of all organized existence; of his will, as not a product of nature's chemistry, but above

nature; and he says that the inference which he draws from the facts reviewed is that "a superior intelligence has guided the development of man in a definite direction, and for a special purpose, just as man guides the development of many animal and vegetable forms." He also argues that "all force is probably will-force;" and thus, that "the whole universe is not merely dependent on, but actually *is*, the WILL of higher intelligences, or of one Supreme Intelligence." We cite a few paragraphs from the part of his argument based on a comparison of the brains of Man and the Man-apes.

"The collections of Dr. J. B. Davis and Dr. Morton give the following as the average internal capacity of the cranium in the chief races:—Teutonic family, 94 cubic inches; Esquimaux, 91 cubic inches; Negroes, 85 cubic inches; Australians and Tasmanians, 82 cubic inches; Bushmen, 77 cubic inches. These last numbers, however, are deduced from comparatively few specimens, and may be below the average, just as a small number of Finns and Cossacks give 98 cubic inches, or considerably more than that of the German races. It is evident, therefore, that the absolute bulk of the brain is not necessarily much less in savage than in civilized man, for Esquimaux skulls are known with a capacity of 113 inches, or hardly less than the largest among Europeans.* But what is still more extraordinary, the few remains yet known of pre-historic man do not indicate any material diminution in the size of the brain case. A Swiss skull of the stone age, found in the lake dwelling of Meilen, corresponded exactly to that of a Swiss youth of the present day. The celebrated Neanderthal skull had a larger circumference than the average, and its capacity, indicating actual mass of brain, is estimated to have been not less than 75 cubic inches, or nearly the average of existing Australian crania. The Engis skull, perhaps the oldest known, and which, according to Sir John Lubbock, "there seems no doubt was really contemporary with the mammoth and the cave bear," is yet according to Professor Huxley, "a fair average skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage." Of the cave men of Les Eyzies, who were undoubtedly contemporary with the reindeer in the South of France, Professor Paul Broca says (in a paper read before the Congress of Pre-historic Archæology in 1868)—"The great capacity of the brain, the development of the frontal region, the fine elliptical form of the anterior part of the profile of the skull, are incontestible characteristics of superiority, such as we are accustomed to meet with in civilized races." * * * We cannot fail to be struck with the apparant anomaly, that many of the lowest savages should have as much brains as average Europeans. The idea is suggested of a surplusage of power; of an instrument beyond the need of its possessor. In order to discover if there is any foundation for this notion, let us compare the brain of man with that of animals. The adult male

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* While the largest Teutonic skull in Dr. Davis's collection is 112·4 cubic inches, there is an Araucanian of 115·5, an Esquimaux of 113·1, a Marquesan of 110·6, a Negro of 105·8, and even an Australian of 104·5 cubic inches.

Orang-utan is quite as bulky as a small sized man, while the Gorilla is considerably above the average size of man, as estimated by bulk and weight; yet the former has a brain of only 28 cubic inches, the latter, one of 30, or, in the largest specimen yet known, of 34½ cubic inches. * * *

"We see, then, that whether we compare the savage with the higher developments of man, or with the brutes around him, we are alike driven to the conclusion that in his large and well-developed brain he possesses an organ quite disproportionate to his actual requirements—an organ that seems prepared in advance, only to be fully utilized as he progresses in civilization. A brain slightly larger than that of the gorilla would, according to the evidence before us, fully have sufficed for the limited mental development of the savage; and we must therefore admit, that the large brain he actually possesses could never have been solely developed by any of those laws of evolution, whose essence is, that they lead to a degree of organization exactly proportionate to the wants of each species, never beyond those wants—that no preparation can be made for the future development of the race—that one part of the body can never increase in size or complexity, except in strict co-ordination to the pressing wants of the whole. The brain of pre-historic and of savage man seems to me to prove the existence of some power, distinct from that which has guided the development of the lower animals through their ever-varying forms of being."

IV. ASTRONOMY.

1. *Cordova Observatory under the direction of* Dr. B. A. Gould.—Dr. Gould has, at our request, furnished us with the following statement with regard to the Cordova Observatory which has been placed under his charge. He sailed with his family for South America in the latter part of May.

The Argentine Congress voted to establish a national observatory at Cordova, at the instance of President Sarmiento, and through the exertions of the present Minister of Public Instruction, Dr. Avelleneda,—who invited me to organize and take charge of it, knowing my desire to extend the catalogue of the southern heavens beyond the limit of 30° to which the zones of Argelander extend. Bessel went through the region from 45° N. to 15° S. with systematic zone-observations at Königsberg, which have since been reduced and published in two catalogues by Weisse of Cracow. Argelander carried the same systematic scrutiny with the Meridian Circle, from Bessel's Northern limit to the pole, and afterwards from Bessel's southern limit to 30° S.

Since then Gilliss has observed a series of zones for 30° around the south pole; but the reduction of these, although very far advanced, was not completed at the time of his death, and the Ms. is now stored somewhere in Washington. Let us hope that it may at some time be recovered, the work completed and given to the world.

My hope and aim is to begin a few degrees North of Argelander's southern limit, say at 26° or 27°, and to carry southward a system of zone observations to some declination beyond Gilliss's northern limit, thus rendering comparisons easy with both these other labors, and permitting the easy determination of the corrections needful for reducing positions of any one of the three series to corresponding ones for the other. It is of course impossible to arrange in advance the details of such an undertaking, but my expectation is to go over the region in question in zones 2° wide, (except in the vicinity of the Milky Way where the width would be but one-half as great,) up to a declination of about 55°, after which the width would be gradually increased as the declinations became greater. Within these zones all stars seen as bright as the 9th magnitude would be observed, so far as possible, moving the telescope in altitude when no bright star is in the field until some one becomes visible, according to the well known method of zone-observations.

For reducing the observations, differential methods will probably be employed, inasmuch as the time now assigned for my absence from home would be inadequate for proper discussion of the corrections required for nice determinations of an absolute character. Still it is my present purpose, so far as possible, to make such subsidiary determinations as might hereafter be needed in any attempt at computing the observations absolutely. But as I hardly venture to anticipate any opportunity of making a thorough determination of the constants of refraction, or of the errors of graduation, it seems best to arrange for a differential computation at least at first.

It is improbable that a sufficient number of well determined stars will be found available even for this differential reduction, and the necessity may thus be entailed of determining the comparison-stars myself, this determination, however, itself depending upon standard star places. So far as possible I propose employing those heretofore determined by me, and published by the coast survey, which form the basis of the star places of the American Nautical Almanac.

With these observations of position it is my hope to combine others of a physical character to some extent; but in the presence of a plan implying so much labor and effort, it would be unwise to rely upon the possibility of accomplishing much more than the zone-work.

The Meteorological relations of the place are very peculiar, but I dare not undertake any connected series of observations bearing upon these, without self-registering apparatus, which is beyond my means.

Cordova is one of the oldest cities, and contains the oldest university, of the Western hemisphere. It is situated in 31°½ S. latitude, on the boundary of the Pampa, where the land begins to rise toward the group of mountains known as the Sierra de Cordova. It is connected with Rosario, on the Parana, by the Central Argentine railway, which has probably been already opened to travel through its entire length of about 250 miles, although information to that effect has not yet reached this country.

The two largest instruments will be a Repsold meridian-circle of 54 inches focal length and 4½ inches aperture, and an equatorial by Alvan Clark & Sons, provided with the 11 inch object-glass, by Fitz, lately in the posession of W. Rutherfurd who has supplied its place by one of 13 inches. A photometer by Ausfeld of Gotha, according to Zöllner's latest form, has been constructed under the supervision of Prof. Zöllner himself; a spectroscope will be furnished by Merz of Munich, and a clock by Tiede of Berlin.

The Scientific institutions of the U. S. have afforded the expedition every possible assistance. The Coast Survey lends a circuit-breaking clock, a chronograph, and a portable transit; the Smithsonian Institution lends a zenith telescope; the American Academy of Arts and Sciences of Boston [probably] a photometer and spectroscope; the Washington Observatory and the Nautical Almanac have greatly aided the undertaking by gifts of books and by a manuscript copy of Gilliss's catalogue of Standard Stars; and from the astronomers of England, Germany and Russia important assistance has been freely and effectively contributed, in the order and supervision of instruments and apparatus, and by the gift of books, as well as by important and valuable suggestions.

Four assistants will accompany me, Messrs. Miles Rock, John M. Thome, Clarence L. Hathaway and William M. Davis, Jr. We hope to reach Buenos Ayres not later than the middle of August.

The building is now under construction in Boston. The means available proved inadequate for its construction according to the original plan, which was in the form of a cross, with four square rooms about its center, and turrets at its four extremities. One half of it will be first erected; and it is hoped that the remaining portion will speedily be added.

2. *Recent Auroral displays in the United States.*—We are now near the period of a maximum of the solar spots, and if auroral displays observe a similar periodicity, we must also be near an auroral maximum. The reports made by the observers of the Smithsonian Institution as published with the monthly Reports of the Department of Agriculture have been examined to ascertain the number of auroras recorded upon successive years. Only an occasional observation of the aurora appears in these Reports until the year 1869. It is presumed that the Department of Agriculture did not regard the auroral observations of the preceding years as possessing sufficient general interest to authorize their publication. The following are the dates upon which auroras were reported by at least one of the observers of the Smithsonian Institution for the year 1869, and for three months of the year 1870.

1869,	Jan.	3, 5, 6, 7, 10, 13, 17, 18, 19, 20, 21, 22, 30,	Total	13	days
	Feb.	2, 3, 4, 5, 6, 8, 9, 10, 11, 15, 17, 18, 19,	"	13	"
	March	1, 2, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 16, 17, 18, 30, 31,	"	18	"
	April	1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 19, 22, 23, 28, 29, 30,	"	23	"

1869,	May 3, 4, 5, 6. 7, 8. 9, 10. 12, 13. 24, 27, 28, 29,	Total, 14 days.
	June 4, 5, 6, 7, 8, 12. 15, 16, 19. 20, 23, 25, 26, 27. 28, 29. 30,	" 17 "
	July 2, 3, 5. 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 25, 26, 28, 29,	" 18 "
	Aug. 1, 3, 5, 6, 7, 10, 14, 15, 16, 22, 24. 27, 28, 29, 30,	" 15 "
	Sept. 1, 2, 3, 4, 5, 6, 8, 9, 10, 11, 12, 13, 14, 15, 16, 19, 24, 26, 27, 28, 29, 30,	" 22 "
	Oct. 1, 2, 4, 5, 6, 8, 9, 12. 21, 22, 24, 25, 26, 27, 28, 30, 31,	" 17 "
	Nov. 1, 6, 8, 9, 10, 12, 25, 26, 29,	" 9 "
	Dec. 4, 5, 6, 7, 13, 14, 15, 18, 22, 25, 26, 29, 30,	" 13 "
	Total for the year 1869,	192 days.
1870,	Jan. 1, 2, 3, 4, 5, 6, 7, 8, 9, 16, 18, 24. 25, 26. 27, 28, 29, 30, 31,	Total 19 days.
	Feb. 1, 2, 3, 4, 5, 6, 10. 11, 12. 13, 20, 21, 23, 24, 25, 26, 27, 28,	" 18 "
	March 1, 2, 3, 4, 6, 8, 9, 10, 11, 13, 19, 20, 21, 22. 23, 24, 25, 26, 27, 28, 29, 30, 31,	" 23 "
	Total for three months of 1870,	60 "

These observations indicate a very remarkable number of auroras, but it should be remembered that they are derived from the reports of 350 observers spread over a territory embracing 56 degrees of longitude and 20 degrees of latitude. In order that we may be able to make a comparison with former years, it is desirable to have a similar summary of the reports of all the Smithsonian observers; and it is to be hoped that the Secretary of that Institution will furnish us with such a summary for each year since the Smithsonian system of meteorological observations was commenced.

The following summary of the auroral observations made for five years at Depauville, N. Y. (lat. 44° 7′ N. long. 76° 3′ W.) has been furnished by Mr. Henry Haas.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1865	2	3	3	5	1	5	5	5	4	5	2	2	42
1866	1	1	4	3	7	2	3	4	6	5	4	2	42
1867	2	1	4	3	5	2	3	6	10	5	2	1	44
1868	3	3	10	4	6	4	3	3	2	1	0	1	40
1869	4	1	4	6	5	2	4	4	8	1	2	3	44

The number of auroras reported for the different years is remarkably uniform, and similar results have been found at other stations where the annual number of auroras rises as high as fifty or upwards; but in lower latitudes where auroral displays are less frequent, the indications of periodicity are unmistakable.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Variations in the Eccentricity of the Earth's Orbit.*—In vol. xlvi of this Journal (1868), Prof. J. N. Stockwell has a paper, with a chart, illustrating the variations in the eccentricity of the Earth's orbit for the past one million of years, starting from a point 175,000 years back of the present time; and in an earlier memoir "On the Secular Equation of the Moon's mean motion" published at Cambridge in 1867, he has given a similar chart for the million of years following. His calculations were made for intervals of 10,000 years, and hence they are much more exact than the earlier chart

of Croll, whose intervals were, for the major part of it, 50,000 years. The main point put forward by Prof. Stockwell in his Cambridge memoir has not been accepted by astronomers, but this does not affect his calculations of the eccentricity. According to his results for past time, a low maximum occurred about 100,000 years back; a higher, 200,000 years; a lower, 300,000; a rather low minimum 410,000; a low maximum, 475,000; a very low minimum, 520,000; a maximum, 570,000; two maxima, the second 750,000; a very low minimum 800,000; an extreme maximum, 850,000; another very low minimum, 900,000; a high maximum, 950,000; etc.—In future time, there will be a very low minimum, 24,000 years on; a low maximum, 150,000 years; another low maximum, 250,000; a very low minimum, 300,000; a low maximum, 400,000; a very high maximum, 515,000; a minimum, 560,000; an extreme maximum, 610,000 years; and so on. Some of the minor undulations, and most of the minima, are not here noted.

2. *Note on an Electrification of an Island;* by F. JENKIN.—A curious discovery has been made by Mr. Gott, the superintendent of the French company's telegraph station at the little island of St. Pierre Miquelon. There are two telegraph stations on the island. One, worked in connection with the Anglo-American company's lines by an American company, receives messages from Newfoundland and sends them on to Sydney, using for the latter purpose a powerful battery and the ordinary Morse signals.

The second station is worked by the French Trans-atlantic Company, and is furnished with exceedingly delicate receiving instruments, the invention of Sir William Thomson, and used to receive messages from Brest and Duxbury. These very sensitive instruments were found to be seriously affected by earth-currents; i.e., currents depending on some rapid changes in the electrical condition of the island; these numerous changes caused currents to flow in and out of the French company's cables, interfering very much with the currents indicating true signals. This phenomenon is not an uncommon one, and the inconvenience was removed by laying an insulated wire about three miles long back from the station to the sea, in which a large metal plate was immersed; this plate is used in practice as the earth of the St. Pierre station, the changes in the electrical condition or potential of the sea being small and slow, in comparison with those of the dry rocky soil of St. Pierre. After this had been done, it was found that part of the so-called earth-currents had been due to the signals sent by the American company into their own lines, for when the delicate receiving instrument was placed between the earth at the French station and the earth at the sea, so as to be in circuit with the three miles of insulated wire, the messages sent by the rival company were clearly indicated, so clearly indeed, that they have been automatically recorded by Sir William Thomson's syphon recorder. Annexed is a facsimile of a small part of the message concerning the loss of the steamship Oneida, stolen in this manner [here omitted.]

It must be clearly understood that the American lines come nowhere into contact, or even into the neighborhood of the French line. The two stations are several hundred yards apart, and yet messages sent at one station are distinctly read at the other station; the only connection between the two being through the earth; and it is quite clear that they would be so received and read at fifty stations in the neighborhood all at once. The explanation is obvious enough: the potential of the ground in the neighborhood of the stations is alternately raised and lowered by the powerful battery used to send the American signals. The potential of the sea at the other end of the short insulated line remains almost if not wholly unaffected by these, and thus the island acts like a sort of great Leyden jar, continually charged by the American battery, and discharged in part through the short insulated French line. Each time the American operator depresses his sending key, he not only sends a current through his lines, but electrifies the whole island, and this electrification is detected and recorded by the rival company's instruments. * * *

All owners of important isolated stations should use earth-plates at sea, and at sea only. This plan was devised by Mr. C. Varley many years ago to eliminate what we may term natural earth-currents, and now it should be used to avoid the production of artificial earth-currents which may be improperly made use of.—*Nature*, May 5th, 1870.

3. *Baron von Richthofen's Explorations in China.*—The Baron v. RICHTHOFEN, formerly of the Geological Survey of Austria, accompanied Mr. J. Ross Brown to China two years ago, and since then has occupied his time in making geological explorations in China. His investigations have been encouraged by the generous subscription of 16,000 dollars, made by the American merchants of China to aid him in prosecuting such exploration. An interesting feature of this subcription is that he is at liberty to use it as he deems best for the interests of science, without regard to immediate commercial or economic results, and in addition to his geological work, he purposes to institute meteorological observations at several points.

Already many important additions have been made to the knowledge of the geology of Northern China and Manchuria. Some of his observations made between Shanghai, and Han-kau were published in the Proceedings of the American Academy of Boston, vol. viii. Since these were written other letters have been received by Prof. J. D. Whitney, from which we hope to make extracts for a future number of this Journal.

4. *Thermal Units.*—Prof. T. MUIR proposes in "Nature" of April 14th, the introduction of the word *therm* for a thermal unit, making that unit *the quantity of heat necessary to raise the temperature of* 1 *gram of water from* 0° C. *to* 1° C. *Therm, hectotherm, kilotherm* would be consequently the expressions for respectively 1,100, 1000 therms, suggestively corresponding to gram, hectogram, kilogram in name as well as in nature."

5. *Astronomer at the Cape of Good Hope.*—Mr. E. J. Stone, F. R.S., of the Greenwich Observatory, has received an appointment to this position.—*Nature*, June 23.

6. *American Association.*—The American Association for the Advancement of Science will hold its next meeting in Troy, commencing on the first Wednesday in August. Prof. William Chauvenet is the President for the year.

7. *British Association.*—The next meeting of the British Association will be held at Liverpool on Wednesday, the 14th of September, under the Presidency of Prof. Huxley.

8. ADMIRAL RUSSELL HENRY MANNERS, President of the Astronomical Society of London, died recently, aged 70 years.

IV. MISCELLANEOUS BIBLIOGRAPHY.

1. *A concise analytical and logical Development of the Atmospheric System as God made it; and the Elements of prognostication by which the weather may be forecasted; designed as a Weather-book for the practical mind of the Country;* by THOMAS B. BUTLER. Hartford, 1870. 404 pp. 8vo.—This work on Meteorology will be read with profit by all interested in the subject. It presents a large number of facts of the Author's own observations, together with others from various sources, and is illustrated with many woodcuts. The title is calculated to excite a prejudice in scientific minds against the volume, and many would at once substitute for "The Atmospheric System as God made it," "The Atmospheric System as Thomas B. Butler makes it." The subject is too large a one to be discussed in a book notice; and we refer, therefore, to the volume for the views, which, according to the motto on the title page, is, in the author's opinion, "the truth [that] is mighty and will prevail."

2. *Synopsis of the Extinct Batrachia and Reptilia of North America*: Part 2d. By EDWARD D. COPE.—This second part of Prof. Cope's important work on the Extinct Batrachia and Reptilia of North America includes the synopsis of the remainder of the Dinosauria, the Testudinata, the Pterosauria, the Pythonomorpha, and the Ophidia. The Testudinata are divided into two groups: the Cryptodira, represented by thirty-four species which are referred to fourteen genera, and the Pleurodira, represented by ten species of four genera. *Rhabdofelix longispinis* Cope is the only known North American representative of the Pterosauria. Of the Pythonomorpha, the author mentions twenty-seven species. Three species of the Ophidia are given. Several additional Reptilian species are remarked upon in the Appendix, and one new genus of the Cheloniidæ is defined. Prof. Cope, through his labors, is throwing great light on the ancient vertebrate life of this continent.

3. *First Principles of Chemical Philosophy;* by JOSIAH P. COOKE, JR., Erving Professor of Chemistry and Mineralogy in Harvard College. 534 pp. text and 22 tables and index, 12mo.—The first part of Prof. Cooke's Chemical Philosophy was noticed in a former volume of this Journal. That portion of the work, it will be remembered, was devoted to the development of the fundamental principles of chemical science. In the second part of his treatise, the author gives a succinct, but very comprehensive, summary of the more important elements and compounds, exhibiting

their constitution and relation by means of formulæ and reactions, illustrated by problems calculated to direct the attention of the student to the more important facts and principles of the science.

By his method of developing the subject, Prof. Cooke succeeds in giving to chemistry a high degree of logical sequence and mathematical exactness, and his work, taken in connection with a good book of laboratory practice, will be found a most efficient aid to both teacher and pupil in the study of chemistry.

4. *Neue Untersuchungen über den elektrisirten Sauerstoff;* von Dr. G. Meissner. Mit zwei lithographirten tafeln. Göttingen, 1869. 4to, pp. 110.—This brochure, re-published from the Transactions of the Royal Academy of Sciences in Göttingen, contains the experiments made by the author, with the aid of his previous experience, in support of his opinion that the so-called atmizone or antozone fog consists only of electrified oxygen and water; an opinion stated in his previous research published in 1863. An abstract of this supplementary memoir of Meissner will appear in our next number.

5. *Great Outline of Geography: a Text-Book to accompany the Universal Atlas;* by Theodore S. Fay. 238 pp. 12mo. New York, 1869. (G. P. Putnam & Son).—Mr. Fay has made an important contribution to our means of instruction in geography in the Atlas and its accompanying text, lately published by Putnam & Son. The work has been long in preparation, the map having been prepared by Hassenstein of Berlin, during the author's long residence as American Minister at European courts. A fac-simile of a letter received by the author from Humboldt expressing his approval of it, is given in the volume. The number of plates constituting the atlas is eight, the most of them double and all well executed. They give, as far as the limited space allows, the chief astronomical, political and historical features of geography, together with some of the physical, and are presented in a clear and striking manner. The text consists partly of brief explanations of terms and principles, and of historical, geographical and other observations, but mainly of lists of places to be looked out on the maps. The plan of instruction proposed in the work is for the teacher to read the lesson from the book while the pupil before him "follows every word upon the plates," looking for the cities, rivers, &c., as the names are mentioned; and this exercise, which is not to exceed, at any one time, an hour, is study enough. The pupil who goes through with the work under the guidance of a patient teacher cannot fail to become well acquainted with the subject.

6. *Geology and Revelation; or the ancient history of the earth considered in the light of Geological facts and revealed religion, with Illustrations;* by the Rev. Gerald Molloy, D.D., Professor of Theology in the Royal College of St. Patrick Meynooth. 418 pp. 12mo. London, 1870. (Longmans, Green, Reader and Dyer). —No book on this subject has appeared from the theological side that is more worthy of respect than this by Dr. Molloy. The author's discussion of the principles of geology, with reference to

their bearing on the Mosaic history of Creation evinces great familiarity with the science and the necessities of the unscientific reader, and thorough knowledge of the sacred record. In treating of the antiquity of the earth he enforces his argument by copious quotations from the Christian Fathers, showing that long before geology had any existence as a science, and of course, when the discussions and doubts it has excited were unknown, the essential points respecting *time* and the order of Creation had received careful attention from devout thinkers, and that the conclusions, at which they arrived on purely theological grounds, were in most cases much the same as those which the best writers of our time deduce from geological evidence. Dr. Molloy closes his arguments as follows:—"We have then two distinct systems of interpretation according to which the vast antiquity of the earth asserted by geology may be fairly brought into harmony with the history of creation recorded in Scripture. One allows an interval of incalculable duration between the Creation of the Heavens and the Earth, and the work of the six days; the other supposes each one of the six days to have been itself an indefinite period of time." The questions touching the antiquity of the human race the author proposes to discuss separately at a future time.

We understand that this work will soon be issued from the publishing house of G. P. Putnam & Son of New York.

7. *Reliquiæ Aquitanicæ, being contributions to the Archæology and Palæontology of Périgord and the adjoining Provinces of Southern France;* by E LARTET and HENRY CHRISTY, edited by THOMAS RUPERT JONES, Prof. Geol., etc., Roy. Mil. College, Sandhurst. Part X, for February, 1870, carries the work to pages 140 and 132 in its two parts, the plates to A XXXII, and B XVIII.

Report of the Commissioner of Agriculture for the year 1868. 671 pp. Washington, 1869.

Annual Report of the Trustees of the Museum of Comparative Zoology, at Harvard College. 42 pp. 8vo. Boston. 1870.

First Annual Report of the American Museum of Natural History. [Museum in the New York Park.] 30 pp. 8vo. Jan. 1870.

Transactions of the Edinburgh Geological Society, Vol. I, Parts I, II, III, 372 pp. 8vo. with many plates. 1868–1869. Edinburgh.

Tables for the determination of Minerals; by Thomas Egleston, Jr., Prof. Min. and Met., in the School of Mines of Columbia College. 26 pp. 8vo. New York, 1870.

A Treatise on Elementary Geometry, with appendices containing a Collection of Exercises for students and an introduction to Modern Geometry; by William Chauvenet, L.L.D. Prof. of Math. and Astr. in Washington Univ. 368 pp. 8vo. Philadelphia, 1870. (J. B. Lippincott.)

On Eozoon Canadense, by Professors William King and T. H. Rowney, of Queen's Univ., Ireland. 48 pp. 8vo.. with 4 colored plates. From the Proc. R. Irish Acad.. July, 1869. [Facts and arguments against the animal nature of the Eozoon, from which we may cite at another time].

Report of the British Association for 1869. cv, 434 and 260 pp. London, 1870.

Grundzüge einer allgemeinen Theorie der Oberflächen in synthetischer Behandlung, von D. Ludwig Cremona, Prof. Berlin. 1870.

Annali di Mathematica pura ed applicata, diretta da F. Brioschi e L. Cremona, Ser. II, Milano.

The American Colleges and the American Public; by Noah Porter, D.D., Professor in Yale College. 282 pp. 12mo. New Haven, Conn., 1870. (C. C. Chatfield & Co.)

THE

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[SECOND SERIES.]

ART. XVI.—*Comparison of the mean daily range of the Magnetic Declination, with the number of Auroras observed each year, and the extent of the black Spots on the surface of the Sun;* by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College.

IN 1826, M. Schwabe of Dessau in Germany commenced a series of observations of the solar spots which he has continued to the present time. For each year he has kept a record of the number of days of observation, the number of groups of spots observed, and the days on which the sun was free from spots. These observations decidedly indicate a periodicity in the number of the solar spots, a maximum recurring at an interval of from 7 to 13 years. In 1849, Dr. Rudolf Wolf, of Zurich, Switzerland, commenced a series of observations for the same object as those of Schwabe, but upon a plan somewhat more precise and thorough. For each day of observation he recorded two numbers; the first showed how many groups or isolated spots were seen with a four-feet Fraunhofer telescope and a magnifying power of 64; the second showed the total number of visible spots for that day. In order to deduce from these observations a series of numbers which shall be proportional to the amount of spotted surface of the sun, he multiplies the number of groups for each day by ten, and adds to this product the total number of spots. Thus, if on a certain day he counts 9 groups and 31 single spots, he obtains 121 ($9\times10+31$) which he calls the relative number. The mean of all the numbers thus obtained for a month, is the relative number for that month; and the mean of the number thus obtained for a year is the relative number for that year. Dr. Wolf has continued these observations to the present time, and they show clearly a minimum in 1856 and another in 1867, the interval being 11 years.

Dr. Wolf has made a most thorough examination of the records of Astronomical observers, since the invention of the telescope in 1608, and has aimed to deduce a similar series of numbers which shall be proportional to the amount of spotted surface of the sun for a period of 260 years. In this attempt he has been remarkably successful, and he claims to have determined the date of every maximum and every minimum of the solar spots since 1608. He has also published a table of relative numbers for each year since 1700, in which, however, we find 36 years marked with an interrogation point, indicating that the numbers for those years are specially unreliable, or were derived by interpolation. Since 1749 only 12 years are thus marked with an interrogation point. These years are 1774, 1792, 1793, 1801 to 1807, and 1814–5. From 1749 to 1825, the numbers for 37 years are claimed to be specially reliable.

The following is a list of all the important observations since 1700 from which these numbers are derived. All the references are to "Vierteljahrschrift der Naturforschenden Gesellschaft, in Zurich."

Obs. from 1700–1748 by Kirch, vol. 12, p. 142.
" 1705–1726 by Plantade, vol. 5, p. 258.
" 1718–1726 by Rost and Alischez, vol. 5, p. 261.
" 1742–1751 by Hagen, vol. 4, p. 250.
" 1749–1799 by Staudacher, vol. 2, p. 277.
" 1754–1760 by Zucconi, vol. 2, p. 285.
" 1769 by Horrebow, vol. 10, p. 281.
" 1773–1777 by Mallet, vol. 3, p. 394.

Obs. from 1794–1830 by Flaugergues, vol. 6, p. 433.
" 1800–1818 by Heinrich, vol. 4, p. 85.
" 1813–1836 by Stark, vol. 3, p. 373.
" 1816–1825 by Tevel, vol. 4, p. 239.
" 1819–1823 by Adams, vol. 6, p. 449.
" 1826–1868 by Schwabe, vol. 1, p. 266; vol. 5, p. 1, etc.
" 1849–1868 by Wolf, vols. 1, to 14.
" 1854–1860 by Carrington, vol. 9, p. 248.

I have no reason to doubt that the relative numbers which Dr. Wolf has derived from these observations are generally as accurate as can be deduced from the materials employed, but I think his numbers need some correction for the years 1793 and 1794 and probably also for 1795. His numbers from 1787 to 1795 are

1787	92·8*	1790	75·2	1793	20·7?
1788	90·6*	1791	46·1	1794	23·9
1789	85·4	1792	52·7?	1795	16·5

I propose to examine into the accuracy of these numbers for 1793 and 1794.

The relative numbers from 1787 to 1793 depend almost entirely upon the observations of Staudacher. On p. 283, vol. 2, Wolf, by comparing the average number of spots on each day of observation, deduces the following numbers:

1787.	1788.	1789.	1790.	1791.	1792.	1793.
46·4	45·3	39·4	37·6	22·8	21·7	17·0

The relative numbers which he gives for 1787 and 1788 are obtained by doubling the corresponding numbers here given for those years; and applying the same rule to 1793 we obtain 34 as the relative number for that year. If we combine with Staudacher's observations, the six observations made by Huber, Hahn, and Bode we shall obtain almost exactly the same result. I, therefore, adopt 34 as the most probable relative number for 1793.

The relative number for 1794 depends chiefly upon the observations of Flaugergues, and I will compare his observations for 1794 with those for 1816, for which year Wolf considers the relative number as well determined; and I will also include 15 days (Sept. 12 to 27, 1794) as being without spots. The following is a summary of the results:

	Groups.	Spots.	Days.	Average daily No. of Groups.	Average daily No. of Spots.	Relative No.
1794,	41	148	35	1·17	4·23	15·9
1816,	62	112	51	1·22	2·20	14·4

The relative number which Wolf adopts for 1816 is 45·5. The above result for 1794, reduced to the same scale, becomes 50·2. If we combine with the observations of Flaugergues, observations on 15 days made by Ende, Herschel, Bode and Staudacher, this relative number will be somewhat increased. I therefore adopt 50 as a number not too great for the relative number of 1794, in place of 23·9 given by Wolf. A similar analysis applied to the observations of 1795 would give 35 as the correct relative number instead of 16·5. The maximum for 1804, Dr. Wolf estimates at 70; but this number is altogether conjectural, since observations are reported for only two days of that year. The observations of the magnetic declination indicate that this number is too great, and I have accordingly reduced it to 50, and have reduced the numbers for the three preceding and following years in the same ratio. The entire series of relative numbers from 1740 to 1868 will then be as follows. An asterisk (*) denotes a result considered specially trustworthy; an interrogation point (?) denotes a result considered specially untrustworthy.

Table of Relative Number of Solar Spots Each Year.

Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number of Spots.
1740	60·0 ?	1766	17·5	1792	52·7 ?	1818	34 1*	1844	13·0*
1741	35·0 ?	1767	33·6	1793	34 0 ?	1819	22·5*	1845	33·0*
1742	18·3	1768	52·2	1794	50·0	1820	8·9*	1846	47·0*
1743	14·6	1769	85 7*	1795	35.0	1821	4·3*	1847	79·4*
1744	5·0 ?	1770	79·4*	1796	9·4*	1822	2·9*	1848	100·4*
1745	10·0 ?	1771	73·2*	1797	5·6*	1823	1·3*	1849	95·6*
1746	20·0 ?	1772	49·2	1798	2·8*	1824	6·7	1850	64·5*
1747	35·0 ?	1773	39·8	1799	5.9*	1825	17·4	1851	61·9*
1748	50·0 ?	1774	47·6 ?	1800	10·1*	1826	29·4*	1852	52·2*
1749	63·8*	1775	27·5	1801	22·1 ?	1827	39·9*	1853	37·7*
1750	68·2*	1776	35·2	1802	27·4 ?	1828	52·5	1854	19·2*
1751	40·9*	1777	63·0	1803	35·7 ?	1829	53·5*	1855	6·9*
1752	33·2*	1778	94·8	1804	50·0 ?	1830	59·1*	1856	4·2*
1753	23·1	1779	90 2	1805	35·7 ?	1831	38·8*	1857	21 6*
1754	13·8*	1780	72·6	1806	21·4 ?	1832	22·5*	1858	50·9*
1755	6·0*	1781	67·7	1807	7·1 ?	1833	7·5*	1859	96·4*
1756	8·8*	1782	33·2	1808	2·2	1834	11·4*	1860	98·6*
1757	30·4*	1783	22·5	1809	0·8	1835	45.5*	1861	77·4*
1758	38·3	1784	4·4	1810	0·0*	1836	96·7*	1862	59·4*
1759	48·6	1785	18·3	1811	0·9*	1837	111·0*	1863	44·4*
1760	48·9*	1786	60·8*	1812	5·4*	1838	82·6*	1864	47·1*
1761	75·0*	1787	92·8*	1813	13·7*	1839	68·5*	1865	32·5*
1762	50·6*	1788	90·6*	1814	20·0	1840	51·8*	1866	17·5*
1763	37·4*	1789	85·4	1815	35·0	1841	29·7*	1867	8·0*
1764	84·5*	1790	75·2	1816	45·5*	1842	19·5*	1868	40·2*
1765	23·0*	1791	46.1	1817	43·5*	1843	8·6*	1869	84·1*

The curve line at the bottom of the accompanying Plate, shows the fluctuations in this series of numbers, and exhibits a succession of maxima and minima which occur at the following dates:

Maxima.

Year.	Interval.	Year.	Interval.
1750			10 years.
	11 years.	1804	
1761			12·5 “
	8 “	1816·5	
1769			13·5 “
	9 “	1830	
1778			7 “
	9 “	1837	
1787			11 “
	7 “	1848	
1794			12 “
		1860	

Minima.

Year.	Interval.	Year.	Interval.
1744			11·5 years.
	11·5 years.	1810	
1755·5			13 “
	10·5 “	1823	
1766			10 “
	9 “	1833	
1775			10 “
	9 “	1843	
1784			13 “
	9 “	1856	
1793			11 “
	5·5 “	1867	
1798·5			

The maxima occur at intervals which vary from 7 to 13·5 years, the average interval being exactly ten years. The minima occur at intervals which vary from 5·5 to 13 years, the average interval being 10½ years. Dr. Wolf overlooks the maximum of 1794 and counts but one period from 1787 to 1804. This interval amounts to 17 years, which is the same as the interval from the maximum of 1761 to that of 1778 (including two periods); and the interval from the maximum of 1830 to that of 1848 (including also two periods) is only 18 years. I think, therefore, we should count 1794 as a year of maximum, equal in amount to the following maxima of 1804 and 1816, but almost blended with the preceding maximum of 1787, in consequence of an unusual prevalence of spots during the intermediate interval. From 1750 to 1860, Dr. Wolf reckons only 10 periods, which would make the average length of one period 11 years; but if we reckon 11 periods (as I think we ought) we shall find the average length of one period to be 10 years.

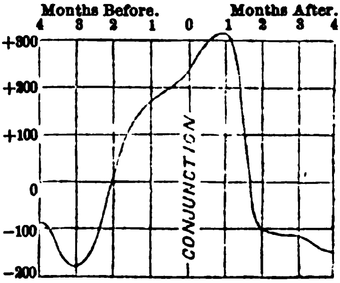
If now we attempt to explain the fluctuations in the sun's surface by ascribing them to the influence of the planets, we find that the planet whose period approaches nearest to the period of 10 or 11 years, is Jupiter, whose time of revolution is 11·86 years, which is nine months greater than Wolf's period, and almost two years greater than the period above found. But the interval between two successive heliocentric conjunctions of Jupiter and Saturn is 19·86 years. Once, therefore, in 9·93 years, Jupiter and Saturn are either in conjunction or opposition; and if we suppose that the action of these planets upon the sun has some analogy to that of the moon upon the earth in raising a tide, then we shall have a cause whose period corresponds quite accurately with the mean period of the maxima of the solar spots. It remains, however, to assign a cause why these periods are alternately increased and diminished by 3 or 4 years, and why the successive maxima are so variable in amount. This cause may, perhaps, be found in part in the position of Jupiter and Saturn in their orbits at the time of conjunction and opposition; but to render the explanation complete we seem obliged to admit that the magnetic condition of the sun has undergone a decided change within the past century; for whether we compare the times of maximum and minimum of the solar spots with the heliocentric longitudes of Jupiter and Saturn, or with their conjunctions and oppositions, we find large differences in the apparent influence of these planets in successive revolutions, and these differences go on steadily increasing for several revolutions of Jupiter; in other words the action of Jupiter and Saturn does not appear to be always the same in the same part of their orbits, nor does the maximum of the spots always occur at the same interval from conjunction or opposition of the planets.

That the mean period of the solar spots is determined mainly by the conjunctions and oppositions of Jupiter and Saturn, is rendered probable by a similar action of Venus and the earth. The observations of Mr. Carrington from 1854 to 1860, have been carefully reduced by Messrs. De La Rue and Stewart (Researches on Solar Physics, second series, 1866) so as to show the actual area of the sun's spotted surface for each day of observation for seven years. In order to eliminate the effect produced upon the amount of spotted surface by the ten yearly period, Messrs. D. and S. have taken the difference between the actual amount of spotted surface for each month, and the amount indicated by the normal ten yearly curve. These differences are mainly the effect of causes whose period is less than 10 years. If now we arrange these numbers in appropriate columns regulated by the times of conjunction of Venus and the earth, and take the averages of the different columns, we shall find that the results follow (in the main) an obvious law, as will be seen in the following table, showing the

Spotted surface of the sun dependent upon the conjunctions of Venus and the Earth.

	Months Before.				Con-junc.	Months After.			
	4	3	2	1		1	2	3	4
1854 Feb. 28. Inf. conj.									
" Dec. 12. Sup. conj.				−198	− 34	− 47	+ 10	−147	−107
1855 Sept. 30. Inf. conj.	− 88	+ 5	− 73	+311	− 75	+369	+114	− 50	− 93
1856 July 20. Sup. conj.	− 63	− 48	− 50	− 38	− 35	+ 35	− 2	+ 40	0
1857 May 9. Inf. conj.	0	+ 15	0	+ 9	+ 20	+138	− 30	− 48	− 55
1858 Feb. 28. Sup. conj.	− 5	− 30	−165	+ 16	+240	− 83	+ 20	−160	+280
" Dec. 13. Inf. conj.	+ 45	−177	− 90	+419	+ 62	+494	−364	−214	− 58
1859 Sep. 27. Sup. conj.	−167	+183	+443	+488	+285	+960	−235	− 65	−670
1860 July 18. Inf. conj.	−115	−460	+625	+640	+900	+750	+135	−300	−670
	−305	−900	−500	−180	+720	+235	−565	− 25	+100
Mean	− 87	−177	+ 24	+163	+231	+317	−102	−108	−141

The curve of the accompanying figure represents these average results, and shows that the principal part of these differences may be represented by a simple curve whose period is 9·7 months; being the period between the successive conjunctions of Venus and the sun, or the heliocentric conjunctions and oppositions of Venus and the earth.



The preceding facts are thought to warrant the following propositions:

1. The mean interval between the successive maxima of the solar spots is almost exactly ten years.

2. The value of the maximum of the solar spots is variable, there being generally two or three successive maxima of unusual magnitude, followed by two or three maxima which are smaller than the average magnitude.

3. The ten yearly period of the solar spots is determined by the heliocentric conjunctions and oppositions of Jupiter and Saturn.

4. The principal fluctuations in the amount of the sun's spotted surface from the normal ten-yearly curve, are determined by the heliocentric conjunctions and oppositions of Venus and the earth.

5. The fluctuations in the period of the solar spots from 7 to 13 years, and the great fluctuations in the amount of the suc-

cessive maxima cannot be explained simply by the configuration of the planets, without also admitting a secular change in the magnetism of the sun analogous to the secular change which has been observed in the magnetism of the earth.

Diurnal inequality of the magnetic Declination.—In comparing the diurnal inequality of the magnetic Declination with the amount of spotted surface of the sun, I have taken the observations at Prague as the standard, because these observations are at least as suitable for this purpose as those of any other observatory, and because they are the observations which Professor Wolf has chiefly employed in his comparisons. During the eighteenth century, observations of the magnetic declination were made at Montmorency, France; at Mannheim, Baden; at Paris and London. Those of Montmorency extend from 1777 to '79, and were published in the Connaissance des temps for 1780, etc., and are copied in the Zürich Vierteljahrsschrift, v. 5, p. 241; those of Mannheim extend from 1781 to 1790, and were published in the Palatine Ephemerides, and are copied in the Vierteljahrsschrift, v. 6, p. 427; those of Paris were made by Cassini, and extend from 1784 to '88, and are copied in the Vierteljahrsschrift, v. 2, p. 291; and those of London extend from 1786 to 1805, were made by Gilpin, and were published in the London Philosophical Transactions for 1806, p. 416. During the present century, before the commencement of the Prague observations, we have Beaufoy's observations at London from 1813 to '20; Arago's observations at Paris from 1821 to '31; and the Göttingen observations from 1834 to 1840.

In order that all these observations may be properly represented by a continuous curve, a correction must be applied, not only for difference of locality, but also for a difference in the hours of observation, and for a difference in the mode of deducing from the observations the mean diurnal change. At Prague, the mean diurnal change for the year is obtained by taking the difference between the mean declinations at 8 A. M. and 2 P. M. But the greatest declination usually occurs at 1 P. M., so that the diurnal change appears greatest at those observatories which select the hours of 8 A. M. and 1 P. M. Also the critical hours vary somewhat with the season of the year, so that we shall obtain a still greater value of the diurnal change if we combine the hours of maximum and minimum for each month separately. Moreover, at Greenwich it is customary, in making these determinations, to reject those observations which were made in times of great magnetic disturbance. If such observations are retained, their effect will be to increase considerably the amount of the mean diurnal change. It is difficult to decide what correction should be applied to the different series of observations above enumerated, in order to reduce them to the Prague standard. Fortunately, however, this correction scarcely at all affects the times of maximum and minimum, but simply removes what would otherwise appear as anomalies in the amount of the diurnal fluctuation for different years. The corrections which I have actually applied are the following:

The observations at Montmorency have been increased by one-fifteenth part.
" " " Mannheim diminished by one-fifteenth.
" " " Paris, 1784–88, diminished by one-fifth.
" " " London, 1786–1820, diminished by one-tenth.
" " " Paris, 1821–31, diminished by one-fourth.
" " " Göttingen, 1834–40, diminished by one-fifteenth.

The following table presents a summary of all these observations: [See page 161.]

These observations, after being reduced to the Prague standard in the manner already stated, are represented by the middle curve traced on the accompanying Plate. This curve bears an obvious resemblance to the curve of solar spots, and in general may be said to be parallel with it. The differences between the two curves prior to 1834, may be suspected to arise in part from errors in the observations of one or both of the phenomena represented. Since 1834 the general correspondence of the two curves is remarkably close, but real differences are indicated in the years 1836–8, in 1842–4, and in 1864.

This comparison seems to warrant the following propositions:

1. A diurnal inequality of the magnetic declination, amounting at Prague to about six minutes, is independent of the changes in the sun's surface from year to year.

2. The excess of the diurnal inequality above six minutes as observed at Prague, is almost exactly proportional to the amount of spotted surface upon the sun, and may therefore be inferred to be produced by this disturbance of the sun's surface, or both disturbances may be ascribed to a common cause.

Periodical recurrence of great auroral displays.—In attempting to decide whether auroral displays exhibit the character of a true periodicity, a careful discrimination is necessary in selecting our data for comparison. Travelers who have wintered in some parts of the Arctic regions, have reported that auroral displays were witnessed almost every clear night when the light of the sun did not interfere, even on those years when in the middle latitudes auroras were least frequent. If then we construct a complete catalogue of all recorded auroras, including the 142 auroras observed at Fort Enterprise in 1820–1, the 143 auroras observed at Bossekop in 1838–9; the 141 auroras observed at

Moose Factory in 1850–1; and the 131 auroras observed at Point Barrow in 1852–3, etc., we introduce into our series of numbers an anomaly which results simply from a change of the station of observation, and our numbers do not represent the relative frequency of auroras on different years for any single station or region of the earth. All observations then from the polar regions which are limited to occasional years, and do not form a continuous series, should be entirely discarded in the present comparison. We desire to know

Diurnal inequality of the Magnetic Declination.

Year.	Montmorency.	Mannheim.	Paris.	London.	Reduc'd to Prague.	Year.	Paris.	Gottingen.	Reduced to Prague.	Prague.
1777	11·'2	------	------	------	12·'0	1828	11·'52	------	8·'64	------
1778	10·0	------	------	------	10·7	1829	13·74	------	10·31	------
1779	8·5	------	------	------	9·1	1830	12·40	------	9·30	------
1781	------	9·'12	------	------	8·51	1831	11·75	------	8·81	------
1782	------	8·11	------	------	7·57	1832	------	------	------	------
1783	------	8·77	------	------	8·19	1833	------	------	------	------
1784	------	6·98	9·'65	------	7·12	1834	------	8·'47	7·91	------
1785	------	8·56	10·80	------	8·32	1835	------	9·57	8·93	------
1786	------	12·01	14·00	14·'40	11·79	1836	------	12·34	11·52	------
1787	------	------	15·14	14·98	12·80	1837	------	12.27	11·45	------
1788	------	------	13·48	13·81	11·61	1838	------	12·74	11·89	------
1789	------	8·75	------	11·87	9·43	1839	------	11·03	10·29	------
1790	------	8·33	------	11·95	9·27	1840	------	9·91	9·25	8·'84
1791	------	------	------	11·42	10·28	1841	------	------	------	7·43
1792	------	------	------	9·12	8·21	1842	------	------	------	6·34
1793	------	------	------	8·43	7·59	1843	------	------	------	6·57
1794	------	------	------	7·25	6·53	1844	------	------	------	6·05
1795	------	------	------	6·94	6·25	1845	------	------	------	6·99
1796	------	------	------	7·20	6·48	1846	------	------	------	7·65
1797	------	------	------	7·52	6·77	1847	------	------	------	8·78
1798	------	------	------	7·28	6·55	1848	------	------	------	10·75
1799	------	------	------	7·16	6·44	1849	------	------	------	10·27
1800	------	------	------	6·74	6·07	1850	------	------	------	9·97
1801	------	------	------	7·68	6·91	1851	------	------	------	8·32
1802	------	------	------	8·22	7·40	1852	------	------	------	8·09
1803	------	------	------	9·18	8·26	1853	------	------	------	7·09
1804	------	------	------	8·12	7·31	1854	------	------	------	6·81
1805	------	------	------	8·16	7·34	1855	------	------	------	6 41
1813	------	------	------	6·83	6·15	1856	------	------	------	5·98
1814	------	------	------	7·62	6·86	1857	------	------	------	6·95
1815	------	------	------	7·66	6·89	1858	------	------	------	7 40
1817	------	------	------	8·55	7·70	1859	------	------	------	10·44
1818	------	------	------	8·81	7·93	1860	------	------	------	10·05
1819	------	------	------	7·77	6·99	1861	------	------	------	8·46
1820	------	------	------	7·79	7·01	1862	------	------	------	7·48
1821	------	------	9·10	------	6·82	1863	------	------	------	7·33
1822	------	------	8·83	------	6·62	1864	------	------	------	6·72
1823	------	------	8·18	------	6·14	1865	------	------	------	------
1824	------	------	8·20	------	6·15	1866	------	------	------	------
1825	------	------	9·67	------	7·25	1867	------	------	------	6·47
1826	------	------	9·76	------	7·32	1868	------	------	------	7·27
1827	------	------	11·31	------	8·48	1869	------	------	------	------

whether auroral displays exhibit a periodicity in any given locality or region, and for this purpose we desire a long series of faithful observations at a single station or a limited number of stations; or if satisfactory observations of this kind cannot be obtained, then we must employ observations from a limited region of country where the records have been the most complete and continuous.

The observations at New Haven and Boston combined (Smith. Report, 1865, p. 225,) form a tolerably complete series from 1742 to 1854, and these numbers correspond in a remarkable manner with the fluctuations in the solar spots; but since during a considerable portion of this period no systematic watch for auroras was maintained, it is desirable to combine with this series, observations made at other localities. The observations at St. Petersburg (Smith. Rep. 1865, p. 227) are continuous from 1726 to 1811; and the observations at Berlin (Heis Wochenschrift, Jan. 1870, p. 47) are continuous from 1700 to 1800. I have combined these observations with those at New Haven and Boston, and the correspondence of the resulting numbers with the fluctuations of the solar spots is quite satisfactory. Inasmuch, however, as I have not been able to obtain a continuation of the Petersburg and Berlin observations to the present time, I have thought it best to extend the area of observation, and include all published observations from Europe south of the parallel of 55°. By this method of comparison we eliminate to a considerable extent the anomalies introduced into the observations of any single station, by cloudy weather which conceals many auroras from view; and we also partially eliminate the anomalies resulting from the unequal diligence of the many different observers who must necessarily participate in a series of observations extending over more than a century.

In the Vierteljahrsschrift, vol. 10, p. 232, is given a very complete catalogue of European auroras classified by parallels of latitude, and I have used this as my principal basis in the subsequent comparisons. In the table on page 163, column first shows the year of observation; column second the number of auroras recorded in Europe south of the parallel of 55°; column third the auroras at New Haven and Boston; column fourth the sum of the numbers in the two preceding columns. In order to eliminate still further the effects of irregular and non-periodic causes, I have taken the averages of the numbers in column fourth for each successive period of three years, and the numbers thus resulting are given in column fifth.

For a few years preceding and following 1787, column second shows a very remarkable increase in the number of observed auroras sufficient to excite a suspicion that these numbers are relatively too great. The Palatine Meteorological Society, which

Number of Auroras observed on different years.

Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.	Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.	Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.
1739	46		46		1783	73	22	58	47	1827	16	7	23	17
1740	23		23	39	1784	51	4	29	39	1828	12	6	18	21
1741	49		49	39	1785	42	9	30	56	1829	20	2	22	25
1742	42	2	44	43	1786	110	55	110	84	1830	28	6	34	25
1743	33	2	35	31	1787	133	47	113	108	1831	17	2	19	20
1744	14	0	14	20	1788	124	38	100	105	1832	6	2	8	13
1745	10	0	10	16	1789	103	51	102	84	1833	8	3	11	11
1746	17	7	24	22	1790	77	13	51	68	1834	4	9	13	12
1747	23	10	33	27	1791	78	12	51	46	1835	7	6	13	15
1748	18	6	24	27	1792	60	6	36	37	1836	14	5	19	32
1749	13	10	23	33	1793	15	8	23	23	1837	22	41	63	42
1750	34	17	51	31	1794	8	2	10	14	1838	5	39	44	60
1751	14	5	19	30	1795	6	2	8	7	1839	27	47	74	65
1752	19	2	21	17	1796	2	0	2	8	1840	32	44	76	80
1753	11	1	12	15	1797	13	0	13	5	1841	34	56	90	67
1754	12	0	12	11	1798	1	0	1	6	1842	18	18	36	53
1755	10	0	0	10	1799	5	0	5	4	1843	24	10	34	37
1756	7	0	7	9	1800	6	0	6	6	1844	28	13	41	39
1757	4	6	10	7	1801	8	0	8	6	1845	18	24	42	48
1758	1	4	5	14	1802	2	2	4	6	1846	25	35	60	54
1759	22	5	27	16	1803	1	5	6	6	1847	35	25	60	76
1760	11	6	17	23	1804	4	4	8	14	1848	46	62	108	72
1761	20	5	25	22	1805	23	4	27	14	1849	26	23	49	69
1762	16	7	23	19	1806	3	4	7	13	1850	15	36	51	49
1763	2	6	8	16	1807	2	2	4	4	1851	20	26	46	65
1764	4	12	16	11	1808	1	0	1	2	1852	33	64	97	67
1765	1	7	8	8	1809	0	2	2	1	1853	20	39	59	63
1766	0	0	0	5	1810	1	0	1	1	1854	7	27	34	35
1767	4	4	8	9	1811	0	0	0	0	1855	0	12	12	20
1768	12	7	19	30	1812	0	0	0	1	1856	5	8	13	11
1769	46	18	64	40	1813	2	0	2	4	1857	1	7	8	21
1770	22	14	36	41	1814	6	3	9	5	1858	11	30	41	37
1771	7	15	22	24	1815	2	1	3	5	1859	47	16	63	50
1772	6	7	13	26	1816	2	0	2	5	1860	28	17	45	48
1773	26	17	43	33	1817	11	0	11	7	1861	24	12	36	36
1774	24	20	44	33	1818	4	4	8	11	1862	23	5	28	28
1775	7	5	12	22	1819	9	6	15	10	1863	19	2	21	29
1776	5	4	9	26	1820	4	2	6	8	1864	33	6	39	33
1777	41	15	56	38	1821	2	0	2	4	1865	36	2	38	34
1778	30	18	48	69	1822	2	1	3	2	1866	21	5	26	31
1779	98	4	102	70	1823	0	0	0	1	1867	9	20	29	22
1780	34	25	59	67	1824	0	0	0	1	1868	3	7	10	32
1781	68	25	59	57	1825	2	2	4	4	1869	28	28	56	
1782	58	24	53	57	1826	9	0	9	12	1870				

embraced a large number of zealous observers, was organized in 1780 and continued to publish an annual volume of observations till 1792. During this period, auroras were made a special subject for observation, and it cannot be doubted that they were recorded with more fidelity than for most of the preceding and following years. I have therefore reduced the European observations during this interval by one-half, and have combined this result with the New England observations in completing the numbers in the last two columns.

The Boston observations published by Prof. Lovering in the Memoirs of the American Academy, vol. ix, p. 101, and copied in the Smithsonian Report for 1865, p. 227, extend only to 1848. In order to obtain a continuation of this series to the present time, I have carefully consulted the Meteorological Journal kept at Cambridge (Mass.) Observatory, and have obtained the following results :

Number of Auroras recorded each year at the Cambridge Observatory.

Year.	No.	Year.	No.	Year.	No	Year.	No.	Year.	No.
1841	27	1847	9	1853	27	1859	16	1865	1
1842	11	1848	17	1854	25	1860	12	1866	0
1843	4	1849	4	1855	12	1861	6	1867	20
1844	6	1850	15	1856	8	1862	0	1868	6
1845	7	1851	17	1857	7	1863	1	1869	28
1846	15	1852	44	1858	30	1864	1		

When auroras were recorded at New Haven which were not recorded at Cambridge, I have added them to the preceding numbers, and the result is given in column third of the table on page 163. In the years 1860, 1 and 2, no record of auroras was preserved at New Haven, and to supply this omission I have inserted for these years the observations made at Middletown, Conn., by Prof. John Johnston. The European observations published in the Vierteljahrsschrift close with 1861. I have continued the table to the present time by means of the observations published each year in Heis Wochenschrift, confining myself to such observations as were reported south of lat. 55°.

The numbers given in column fifth have been projected in a curve represented at the top of the accompanying Plate. This curve shows an unmistakable resemblance to the curve of the solar spots, and to that of the magnetic declination. To each maximum of the two latter curves there corresponds a maximum of the auroral curve, not always simultaneously, but not differing from it more than a single year except in one or two instances. The following table shows the departures of the auroral maxima from the maxima of the solar spots.

Date.	Diff.	Date.	Diff.	Date.	Diff.
1750	—1 year.	1787	0 year.	1837	+3 years.
1761	0 "	1804	0 "	1848	—1 "
1769	+1 "	1816·5	+1·5 "	1860	—1 "
1778	0	1830	—1 "		

The correspondence in the times of minimum is still more remarkable as is shown in the following table.

Date.	Diff.	Date.	Diff.	Date.	Diff.
1744	+1 year.	1784	0	1833	0
1755·5	+1·5 "	1798·5	unc'n.	1843	0
1766	0 "	1810	0	1856	0
1775	0 "	1823	0	1867	0

There are only two instances in which there is any discrepancy in the times of minima. In one of them (1744) the observations of the solar spots are confessedly very uncertain, and in the other (1755) the change in the number of observed auroras from 1755 to 1757 was only *three*, so that we seem authorized to assert that there is an invariable coincidence in the times of minima of the solar spots and of auroral displays.

The range of the maxima and minima of auroral displays is considerably greater than that of the solar spots, and the observations of magnetic declination seem to indicate a similar peculiarity. There seems then to be no room for doubt that auroral displays exhibit the ten yearly period of solar spots, but the range of the changes on different years is subject to influences which may be independent of the sun.

In order to decide whether this periodicity is exhibited in the higher latitudes, I have selected all those observations with which I am acquainted which show more than a hundred auroras in a year. The following is the table.

Date.	Place.	Lat.	Long.	Numb. Auroras.	Authority.
1820–1	Cumberland House,	53° 56′	102° 16′	142	Force (Smith. Cont.), p. 6.
1820–1	Fort Enterprise,	64 28	113 6	142	Force (Smith. Cont.), p. 24–35.
1833–5	Great Slave Lake.	62 46	109 1	105	Capt. Back.
1838–9	Bossekop,	69 58	23 34E	143	Voyage en Scandinavie.
1848–9	Fort Confidence,	66 54	118 49	122	Athabasca obs. p. 324–350.
1850-1	Moose Factory,	51 10	81 0	141	Am. Jour. Sci., [2], xiv, 156.
1850–1	Athabasca Lake,	58 43	111 18	109	Am. Jour. Sci., [2], xiv, 156.
1852–3	Point Barrow,	71 21	156 15	131	Phil. Trans. 1857, p. 497.

These places are all situated within the zone of greatest auroral frequency, Am. Jour., vol. xxx, p. 89. The observations of 1820–1 and 1833–5 were made near the time when the disturbance of the sun's surface was a minimum; while those of 1848–9 were made when the disturbance of the sun's surface was a maximum; and those of 1838–9, and 1850–1 were made when the disturbance was greater than the average. The average number of auroras recorded at the first two dates is nearly the same as at the last three dates. So far as any conclusion is warranted from so limited a number of observations, the inference is that within the zone of greatest auroral frequency, the number of auroras is about the same for all years, independent of the disturbed condition of the sun's surface. It is presumed that there must be a periodical change in the *brilliancy* of the auroral displays, but the observations are not sufficiently numerous and precise to justify us in pronouncing positively upon this point. As we recede from the zone of greatest auroral frequency, the influence of the sun is shown in a periodical change in the number of auroras; and this periodicity in Europe appears to be the most distinctly marked near the parallel of 50° latitude; and in New England near the parallel of 42° latitude.

It will be observed that the change from a maximum to a minimum of the solar spots is not uniform, and frequently the inequality is so marked as to assume the form of a distinct secondary maximum. Such maxima occurred in 1774, 1792, 1794 (if this is not to be regarded as a primary maximum) and 1864; and in several other cases the inequality in the progress from a maximum to the succeeding minimum is decided. The mean daily range of the magnetic declination exhibits also secondary maxima, and generally the small inequalities in the curve of magnetic declination bear some correspondence to the small inequalities in the curve of the solar spots. The auroral curve shows still greater irregularities, and it is remarkable that an irregularity in the auroral curve often has a decided corresponding irregularity in one of the other two curves. Thus both the auroral and sun-spot curves show a decided maximum in 1774; also in passing from the maximum of 1778 to the following minimum the undulations of the three curves bear a marked resemblance; in the descent from the maximum of 1787, there is a secondary maximum of the magnetic curve corresponding to that of the solar-spot curve; in the descent from the maximum of 1848 there are corresponding undulations of the sun-spot curve and the magnetic curve, while the auroral curve shows at the same time a well-marked secondary maximum; and nearly the same remark is applicable to the descent from the principal maximum of 1860.

The following conclusions are thought to be warranted by the preceding observations.

1. Within the zone of greatest auroral frequency, auroras are of almost daily occurrence in all years, and it is doubtful whether in this region there is any decided periodicity in the number of auroral displays, although there may be periodical changes of brilliancy.

2. At places where the average number of auroras is about 20 or 25 annually, the ten-yearly period of the solar spots can be distinctly traced.

3. The times of maximum and minimum of the solar spots correspond in a remarkable manner with the times of maximum and minimum in the frequency of auroral displays in the middle latitudes.

4. The successive maxima of auroral displays are more variable than those of the solar spots, so that the ten-yearly period might be easily overlooked, and it might be inferred that the maxima only occurred at intervals of about 60 years.

5. The most remarkable irregularities in the auroral curve in the progress from a maximum to a minimum, correspond to similar though generally smaller inequalities in the curve of solar spots or of magnetic declination.

Magnetic storms compared with the prevalence of solar spots.

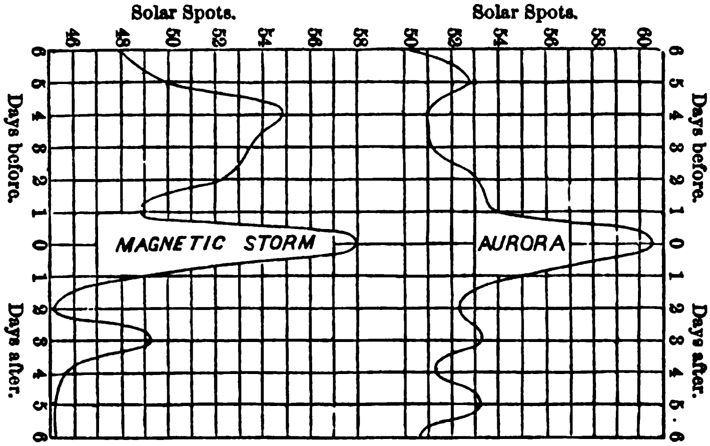
Since the mean daily range of the magnetic needle is greatest on those *years* in which the sun's surface is most disturbed, we could not be surprised if the range of the magnetic needle should prove to be the greatest on those *days* on which the solar spots are most extensive. In order to test this question, I have compared the extent of the solar spots for 6 days preceding and 6 days following each of the great magnetic disturbances at Greenwich for a period of 23 years. In the Greenwich observations for 1862 is given an abstract of the magnetic observations on 177 days of great magnetic disturbance from 1841 to 1857; and in the observations for 1867 are enumerated the days of great magnetic disturbance from 1858 to 1863, amounting in number to 45. These 23 years furnish 222 days of great disturbance, and I have compared Wolf's numbers representing the extent of the solar spots near the time of these dates. Having prepared a table with 13 vertical columns, I insert in the middle column, Wolf's relative number for one of the days of great magnetic disturbance, and I also insert in their appropriate columns the relative numbers for the 6 preceding and 6 following days. In this manner I have treated all the days of great magnetic disturbance at Greenwich, with the exception of those cases in which very few observations of the solar spots were made, and a few cases in which there were two or three successive days of great magnetic disturbance, in which cases I have generally selected the day of greatest disturbance and made but one entry of that period in the table. The cases of disturbance which I have thus employed amount to 135. The following table (see page 168) exhibits these cases for ten years from 1848 to 1857, and at the bottom of the table are given the averages of these numbers for each of the 13 columns; and in another line are given the corresponding averages for the whole number of 135 cases of magnetic disturbance.

The details for the remaining 74 days of observation are omitted in order to avoid incumbering the pages of this Journal. The resulting averages for the 135 cases are represented by the lower curve line in the figure, page 169, where the middle vertical line represents the day of a magnetic storm at Greenwich, and the other vertical lines represent 1, 2, 3, etc., days *before* the storm, and also 1, 2, 3, etc., days *after* the storm. The horizontal lines indicate the amount of disturbance of the sun's surface as measured by Wolf's relative numbers extending from 45 to 58. From this curve it will be seen that the disturbances of the sun's surface about the time of a magnetic storm, bear an analogy to the waves in our own atmosphere about the period of a violent winter storm. We find a well-

Spotted surface of the sun near the time of a magnetic storm.

		Days before the storm.						Magnetic Storm	Days after the storm.					
		6	5	4	3	2	1		1	2	3	4	5	6
1848.	Jan. 28,			100		133	147	151	135	127	105	90		107
	Feb. 21,	116	90		70		20	103	50	106		60	50	
	March 17,	80	106	76		101	60	113	101	75	95	93	79	78
	May 18,	50	91	96	90	54	113	99			85	72	88	68
	July 11,	93	105	112	160	99	122	170	132	126	70	50	77	60
	Oct. 23,		150			70	70	142	94	60	90	40	74	88
	Nov. 17,	20	79		136	163	156		80	60	60		70	54
	Dec. 17,	129	141	133	133	100		105	142	100	135	125	90	170
1849.	Oct. 30,	58	44			34	61	118	66	66	111	40	71	
1850.	Feb. 22,	58	73	80	122	154	55	115	54	60	56	78	75	69
	March 31,	57	23	59	39	67	37	39	27	27	37	0	24	0
	May 7,	25	86	14	36	60	22	80	46	50	74	101	56	92
	June 13,	46	15	132	133	147	129	67	51	34	60	48	56	80
	Oct. 2,		25		45	45	35	55	40	34	39	59	40	22
1851.	Jan. 19,	58	87	27	57	20	20	62	45	23		37	13	11
	Feb. 18,	79	78	46	108	73	105	78	101	105	47	122	116	86
	Sept. 4,	35	46	38	102	40	24	103	66	35	45	69	70	52
	Oct. 28,	101	94	63		82	36	50	54	40	20	37	48	22
	Dec. 6,	79	87	81	46	47	38	26	13	38	36	53	41	
	Dec. 28,	76	60	30	10	71	23	96	94	47	117	82	82	59
1852.	Jan. 4,	94	47	117	82	82	59	52	70	61	83	6	50	66
	Jan. 19,	6	7	60	40	6	96	127	106			14	74	76
	Feb. 15,	2	48			77	77		144	62	16	15	101	10
	Feb. 20,	77		144	62	16	15	101	10	5	77	54	46	15
	April 20,	62	64	34	53	51	38	47	49	28	16	18	50	50
	May 20,	38	46	36	37	11	11	29	28	31	42	57	64	59
	June 11,	57	66	56	44	1	0	11	30	28	4	4	30	24
	June 16,	0	11	30	28	4	4	30	24	25	22	83	48	26
	July 10,	51	65	65	70	11	56	61	34	24	0		25	0
	Nov. 11,	48	10	105	14	81	81	78	61	11	47	34	11	30
1853.	Jan. 10,	25	39	29	27		11	11	23	12	12	11	24	26
	March 7,	15	17	15	12	0	0	11		14	11	11	22	23
	May 3,	26	0	11	14	41	27	31	26	21	16	14	22	13
	May 24,	30	35	47	48	71	46	20	49	59	54	23	29	10
	June 22,	65	26	67	56	56	51	11	20	11	11	11	11	11
	July 12,	35	45	48	75	69	47	48	71	35	63	79	49	47
	Sept. 1,	53	67	33	33	37	56	57	44	68	55	23	34	
	Oct. 25,	48	49	50	46	30	44	44	40	32	26	36	25	
	Nov. 9,			25	25	25		22	22	34		35	25	23
	Dec. 6,	13	11	12	17	17	14	17	17			29	38	18
	Dec. 21,		32	20	31		22	11			11	14	13	
1854.	Jan. 8,	19	27	10	17	10	25	25	0	17	19	19	17	13
	Jan. 20,	13	14	15	17	17	16	14	11	0	0	0	0	0
	Feb. 16,	26	29	35	28	17	10	22	25	17	17	11	0	0
	Feb. 24,	17	17	11	0	0		12	0	0	0	0	12	12
	March 6,	0	12	12	0	0	0	0	0	0	11	34	45	44
	March 15,	11	34	45	44	39	33	33	23	26	27	33	33	27
	March 28,	25	11	13	11	29	22	35	11	0	11	12	12	36
	April 10,	37	38	49	49	56	54	54	55	55	36	35	14	12
	April 23,	12	13	24	22	25	12		11	11	0	0	0	0
	May 25,	13	11	11	13	13	11	0	0	0	0	0	0	14
1855.	March 12,	29	12	38	23	11	16	12	11	14	11	0	11	0
	April 4,	12	16	17	17	12	0	11			18	0		0
	Oct. 18,	15	12	0	0	0	0	0	12	17	11	12	13	11
1857.	March 13,	0	0	0	11	13	12	0	11	11	13	11	13	0
	May 7,	17	16	16	15	15	25	24	20	25	25	26	22	26
	May 10,	15	15	25	24	20	25	25	26	22	20	42	53	60
	Sept. 3,	39	27	27	26	37	38	35	27	48	50	40	29	58
	Nov. 12,	32	26			24	24	24	24	24	36	50	38	58
	Nov. 16,	24	24	24	24	24	36	50	38	58	50	23	45	46
	Dec. 16,	11					71	79	68	54	68			42
Mean of 61 days obs.		40·6	44·4	46·5	46·1	45·8	42·4	52·4	46·2	38·8	40·0	36·6	40·8	38·2
Mean of 185 d'ys obs.		47·9	50·1	54·8	53·5	52·3	48·9	57·9	49·0	45·1	49·3	45·6	45·2	45·3

marked maximum on the day of the magnetic storm; a second maximum four days before the storm, which is also pretty distinctly marked; and a third maximum, more uncertain, three days after the storm. The entire fluctuation within a period of two days is from 45·1 to 57·9, or 28 per cent of the smaller quantity, an effect which is so large and derived from a comparison of so many cases as to indicate a law of nature. Hence we seem authorized to conclude



1. Great disturbances of the earth's magnetism are accompanied by unusual disturbances of the sun's surface on the very day of the magnetic storm; and are, therefore, due to some influence which emanates immediately from the sun.

2. The great disturbance of the sun's surface which accompanies a terrestrial magnetic storm, is generally heralded by a smaller disturbance three or four days previous, succeeded by a comparative calm which immediately precedes the magnetic storm.

Auroral displays and Solar spots compared.

Since the average disturbance of the sun's surface is greatest on those *years* in which auroras are most prevalent, we could not be surprised if the disturbance of the sun's surface should prove to be the greatest on those *days* on which auroras prevail in the middle latitudes. In order to test this question, I have proceeded in the same manner as in the case of magnetic storms; and in order to avoid the suspicion of having selected a few cases for the purpose of confirming a preconceived theory, I have taken the entire series of observations made by Messrs. Herrick and Bradley at New Haven, from 1837 to 1854. These observations are 514 in number, and I have employed them all with the following exceptions:

Spotted surface of the Sun near the time of an Auroral display.

		Days before the Aurora.							Days after the Aurora.					
		6	5	4	3	2	1	Aurora	1	2	3	4	5	6
1850.	Jan. 18,	79	62	73	27	74	65	20	20	15	33		37	56
	Feb. 3,	15	14	79	54	87	75	113	86	86	22	20	30	102
	Feb. 22,	58	73	80	122	154	55	115	54	60	56	78	75	69
	March 3,	56	78	75	69	93	71	97	45	94	56	65	51	168
	March 10,	45	94	56	65	51	168	98	89	65	69	78	70	64
	March 31,	57	23	59	39	67	37	39	27	27	37	0	24	0
	April 7,	27	27	37	0	24	0	12	27	11		30	31	15
	May 7,	25	86	14	36	60	22	80	46	50	74	101	56	92
	May 12,	22	80	46	50	74	101	56	92	50	11	52	34	33
	July 12,	0	12	11	24	46	12	76	33	110	105	110	77	40
	Aug. 9,	77	71	35	47	42	65	60	13	59	47	13	12	51
	Sept. 4,	54	13	82	81	126	105	107	86	104	106	98	86	106
	Sept. 10,	107	86	104	106	98	86	106	74	118	78	58	76	52
	Sept. 30,	46	50		25		45	45	35	55	40	34	39	59
	Oct. 2,		25		45	45	35	55	40	34	39	59	40	22
	Oct. 27,	46	62	40	40		58		86	15	27	68	14	20
	Dec. 26,	81	66	71	60	40	27	58	48	35	39			55
1851.	Feb. 18,	79	78	46	108	73	105	78	101	105	47	122	116	86
	March 22,	31	46	23	47	57	64	81	62	38	54	40	27	35
	March 29,	62	38	54	40	27	35	35	53	54	28	45	12	34
	June 5,	36	81	95	74	27	49	51	44	37	31	42	37	39
	June 27,	108	96	40	34	49	64	61	41	71	41	35	11	12
	July 17,	45	36	34	34	66	45	30	33	52	55	54	35	54
	Sept. 4,	35	46	38	102	40	24	103	66	35	45	69	70	52
	Sept. 7,	102	40	24	103	66	35	45	69	70	52	76		79
	Sept. 10,	103	66	35	45	69	70	52	76		79	80	45	60
	Oct. 1,	40	38	30				14	14	47	109	14	43	32
	Oct. 20,	48	51	15	20	51	64	54	136	101	94	63		82
	Oct. 23,	20	51	64	54	136	101	94	63		82	36	50	54
	Oct. 28,	101	94	63		82	36	50	54	40	20	87	48	22
1852.	Jan. 19,	6	7	60	40	6	96	127	106			14	74	76
	Feb. 16,	48			77	77		144	62	16	15	101	10	5
	Feb. 20,	77		144	62	16	15	101	10	5	77	54	46	15
	March 7,	5	36	2	44	1	39	40	73	56	67	79	81	86
	April 10,	61	77	94	113	117	113	114	90	79	77	62	64	34
	April 15,	113	114	90	79	77	62	64	34	53	51	38	47	49
	April 22,	34	53	51	38	47	49	28	16	13	50	50	81	84
	May 17,	41	25	2	38	46	36	37	11	11	29	28	31	42
	June 5,	4	97	80		5	72	57	66	56	44	1	0	11
	June 11,	57	66	56	44	1	0	11	30	28	4	4	30	24
	June 16,	0	11	30	28	4	4	30	24	25	22	33	48	26
	July 6,	74	49	34	51	51	65	65	70	11	56	61	34	24
	July 10,	51	65	65	70	11	56	61	34	24	0		25	0
	Sept. 6,	49	37	47	34	35	38	35	61	4	38	2	29	38
	Sept. 17,	29	38	26	11	11	0	15	24		57	2	48	54
	Sept. 21,	11	0	15	24		57	2	48	54	44	41	2	33
	Sept. 29,	54	44	41	2	33	34	36	26	3	26	37	42	7
	Oct. 5,	36	26	3	26	37	42	7	37	26	3	27	28	42
	Oct. 20,	69		52	63	76	87	89	109	128	146	40	8	97
	Nov. 11,	48	10	105	11	81	81	73	61	11	47	34	11	30
	Dec. 5,	55	38		40	33	43	58	42	43	29	27	35	33
	Dec. 13,	43	29	27	35	33	27	28	26	14	28	24	10	15
1853.	Feb 14,			28	36	54	44		68	63	43	60	58	85
	March 14,		14	11	11	22	23	24	23		36	64	60	40
	March 27,	34	53	56	68	56	65	63	56	47	58	45	35	34
	April 6,	45	35	34	58	49	20	77	72	27	34	49	40	42
	May 2,	26	26	0	11	14	41	27	31	26	21	16	14	22
	June 1,	59	54	23	29	10	20	38	27	15	28	19	50	44
	June 9,	15	28	19	50	44	40	52	69	80	52	40	47	73
	July 12,	35	45	48	75	69	47	48	71	35	63	79	49	47
Mean of 60 days obs.		48·9	49·3	47·4	49·5	52·5	52·3	56·8	53·2	47·1	48·6	47·5	41·8	47·6
Mean of 251 days obs.		50·3	52·7	51·0	51·2	53·1	53·7	60·5	54·8	52·5	53·3	51·4	53·3	50·7

1. Those cases in which the observers were not entirely confident that there was any auroral display.
2. Those cases in which the corresponding observations of the solar spots were very incomplete; and
3. Those cases in which auroras were observed on two or three successive days, when I have generally selected the most remarkable aurora, and made but one entry for that period.

The number of auroras which I have thus discussed is 251. In order to furnish a specimen of these numbers, the table on page 170 is given, in which the arrangement is the same as in the table on page 168. At the bottom of the table are given the averages of these numbers for each of the 13 columns; and in another line are given the corresponding averages for the whole number of 251 auroras.

These final averages are represented by the upper curve line in the figure page 169, from which it will be seen that there is a well-marked maximum of solar disturbance corresponding to the date of an auroral display. The small fluctuations during the preceding and following days, bear some resemblance to the fluctuations attending magnetic storms, but they are so small in amount that no importance is attached to them. The entire fluctuation within 6 days extends from 50·3 to 60·5, or 20 per cent of the whole quantity; a number so large and derived from so many cases that it is thought to indicate a law of nature. Hence we conclude that

Auroral displays in the middle latitudes of America are generally accompanied by an unusual disturbance of the sun's surface on the very day of the aurora, and are, therefore, subject to some influence which emanates immediately from the sun.

These conclusions may be modified by a comparison of a longer series of observations, and especially by more accurate observations which furnish for each day an exact measurement of the extent of the sun's spotted surface. Such observations have been made for several years at the Kew observatory, and it is hoped that when published they will furnish the materials for the desired comparison. The observations for 1862 and 1863 have already appeared in the Philosophical Transactions for 1869, pp. 23–44; and it is expected that the observations for the subsequent years will soon follow.

ART. XVII.—*On a new Period in Chronology, called the Precession Period;* by J. W. FRENCH. (In a letter to the Editors.)

I PROPOSE in the place of the Julian Period in chronology, another which I will call the Precession-Period.

This latter will be found to have all the advantages of the other without its defects, and beyond these comparative utilities, to have chronological uses of its own which are great and various, both for the subdivisions of history, and for the vast cycles contemplated by science.

The Julian Period in chronology consists of 7,980 Julian years, that number being formed by the continual multiplication of 28, 19 and 15; that is to say, of the cycle of the sun, the cycle of the moon, and the cycle of indiction. The first year of the Christian Era is made the 4,714th of the Julian Period. By such an arrangement we can find for any year its golden number, its number for the solar cycle, and that for its Roman Indiction. Also, we have a fixed period reaching back in history among the local and broken cycles of different peoples and countries. These certainly are great advantages, and have secured for that Period acceptance and commendation.

But some of its defects are these. 1. It has an artificial element, that of the Roman Indiction, instead of having its foundation wholly in astronomy. 2. It is soon exhausted, and in the past, it does not reach far enough even for the Septuagint Chronology in history. 3. It furnishes no unit nor cycle for science.

The period which I propose is founded wholly on astronomy, is exhaustless by being recurrent, has its initial point sufficiently far back for any conceivable historical purposes, gives to science a worthy unit for the vast durations it contemplates, and with these inestimable advantages, has the two practical utilities of the Julian Period;—that of giving the elements needed in the almanac for every year, and that of extending into the past a long and unaltering standard for time.

I propose to take, as the chronological unit, the time for the precession of the equinoxes, 25,872 *years.* By a singular felicity, that number can be formed from the factors 28, 84, and 11. Now, 28 is the number of the solar cycle; 84 is a lunar cycle employed formerly by the Jews, and having peculiar uses in estimating long series of lunations. The other number, 11, is a lunar cycle employed by the early Christians, the errors of which almost exactly counteract those of the cycle of 84 years. These three multiplied together form 25,872. A subordinate felicity will at once be seen by a complete mathematician. *The number* 25,872 *can be divided and subdivided to the last, without a remainder, by a large number of divisors.*

Making the years Gregorian, and calling the initial year of the Christian Era, 0, *I place the beginning of the Precession Period at* 12,693 *B. C.*

Instantly we have by that arrangement, the advantages which have brought the Julian Period into favor.

We can find the solar cycle, the golden number, the Roman Indiction (correctly as we would from the Julian Period) by the following simple rules:

1. To express any year before or after Christ in the correspondent number in the Precession-Period: For any year before Christ, deduct from 12,693, the figures of the year B. C.: For any year after Christ, add to the same number (12,693) the figures of the year A. D.

Thus 752 B. C. (753 *historical reckoning*) is 11,941 P. P. The present year 1870 A. D. is 1,870+12,693=14,563 P. P. (We use P. P. as abreviation for Precession-Period.)

2. To find the Golden Number, Solar Cycle, and Roman Indiction for any year before or after Christ: 1. Turn the year by the first rule into the correspondent number of Precession Period: 2. Divide that number by 19 for the golden number, by 28 for the solar cycle, and by 15 for the Roman Indiction. In the three remainders you have the answer.

Thus the present year 1870 A. D., is 14,563 P. P. Dividing this latter number by 19, I find a remainder of 9; by 28, a remainder of 3; and by 15, a remainder of 13. Opening the Almanac for this year, I find the answer correct. It gives the Golden Number as 9, the Solar Cycle as 3, the Roman Indiction as 13.

The Precession-Period has then in this particular, equal utility with the Julian.

The other advantage, that of a fixed standard of time extending back in past history, it possesses, and adds the great benefit of adequate length in both directions, to 12,693 B. C., and on to 13,179 A. D.; the whole forming one precession. And this precession is not like the Julian Period, an arbitrary straight line stretched over a small portion of duration. It is a definite circle marked on the face of the heavens. The precession begins at 12,693 B. C. with the *point of the vernal equinox in the Zodiac near Spica Virginis*, a brilliant star, forming a good point of departure. When the whole circle of the Zodiac is swept, and the first degree of celestial longitude is again by *Spica Virginis*, the period is completed.

Surely it is better to adopt such a period for our almanacs and histories than the inferior one called the Julian.

But beyond the historical and chronological uses in the *subdivisions* of the Precession-Period, are the advantages of that period as a unit for Astronomy and Geology. We want a unit larger than the year. A precession is a good one. Instead of cumbering a line of page with bewildering cyphers we can say 40, 100, or 1,000 precessions. Thus 40 precessions (and forty is a number easily remembered) would, by another felicity of this period, make a million of years, with a little over. We might call it a millionade, and give that again multiplications to form an *age*. An "age" might be 400 precessions.

Should we adopt any thing like this plan, we should have the same delightful surprise which we often experience, by finding that ancient races and nations have been along the same pathway which we imagined ourselves to be for the first time breaking and exploring. Six days, such as are His, who in His times as in His nature, must be what others are not; six immense periods are reckoned in the Creative work not only in Genesis, but in Oriental Records. Take 100 precessions. Divide them by that number, six. You have the very periods recognized by the Brahmins of India, each about 430,000 years.

West Point, June 7, 1870.

ART. XVIII.—*Upon the Atomic Volumes of Solid Compounds;* by FRANK WIGGLESWORTH CLARKE, S.B.

IN studying the atomic volumes of solid compounds, the materials at my command have been in some respects quite copious, and in others quite limited. Having been unable, through lack of opportunity, to make any new determinations of specific gravities, I have been forced to content myself with the data which are scattered through the various scientific publications. These, apart from the views expressed in nearly a hundred papers written by various chemists upon atomic volumes, consisted of about 1,900 determinations of the specific gravities of 912 different solids of definite constitution, exclusive of alloys. Much of this material had to some extent been already worked over, although the larger part of it had never been examined in this direction. In some cases there are data covering a whole series of compounds, in others only one or two members of a series have been studied, while again, for many important substances no determinations of specific gravity have ever been taken. Again, for some compounds there are many determinations by different authorities, and these bodies can be studied with much certainty; while for other compounds only single observations of specific gravity have been made, and these often with no pretence to rigid accuracy.

Here then at the very outset is a difficulty. If a substance has a very low specific gravity, and a very high atomic weight, a minute error in the determination of the first value must, in calculating the atomic volume, become greatly multiplied; so that a single experimental observation only, may often lead to wholly erroneous results for the atomic volume. Thus, for instance, with Melene, $C_{60}H_{60}$, an error of only 0·04 in the specific gravity will alter the atomic volume about 24·00. And it is very common to find the determinations of specific gravity for a single compound differing more than 0·04. And, as I have already stated, many of the specific gravities observed have never had more than approximate accuracy claimed for them. Hence, one of the chief difficulties in comparing atomic volumes lies in deciding how great variations can be safely ascribed to experimental error. For this purpose, instead of directly comparing the actual atomic volumes with the results of theory, I have preferred to compare the specific gravities calculated from the latter with those really determined by experiment. Even here much care is needful, since errors are more likely to occur with some compounds than with others. Thus, there is much more danger of error in determining the specific gravity of anhydrous magnesic chlorid, than in taking that of baric sulphate.

Furthermore, many different modes of taking specific gravity are represented by the values from which I have to calculate. Some compounds have been examined in the form of powder, and others have been crystallized; some have been studied near their melting point, and others distant therefrom. Nevertheless, in spite of all these possibilities of error and chances of irregularity, certain curious series of relations between atomic volumes are easily demonstrated; which, taken together, hint strongly at a rather more general theory of atomic volumes than has hitherto been enunciated.

But, to begin with, we need a brief resumé of certain points which have been demonstrated by other observers.

First comes the axiomatic statement of Schröder that the atomic volume of a compound must equal the sum of the atomic volumes of its constituent parts. This at once suggests questions of interest. Although many compounds, especially certain sulphids, selenids, and tellurids, have atomic volumes equalling the sums of those of the *free* elements composing them, many other compounds have values lower than such sums, thereby indicating condensation. And some iodids have atomic volumes greater than the sums of those of the metal and the iodine. So here arises the question,—when a compound possesses a greater or less volume than the elements contained in it do in the free state, do those elements condense or expand in equal or in different ratios? It is really upon this point that the study of atomic volumes hinges; so that, stripped of all metaphysical notions of atoms and spheres of heat, the subject simply concerns the distribution of the changes of volume undergone by the various elements in uniting to form compounds.

Second. It has been shown, most definitely by Schröder and Kopp, that, in many cases, when from the atomic volumes of certain series of salts, oxyds, &c., we subtract the atomic volumes of the respective metals contained in them, we obtain constant remainders which may be regarded as representing the values of the various radicals and of oxygen. Thus, if from the atomic volumes of one class of oxyds we remove the values of the metals, we obtain the remainder 2·6. With other oxyds we get 5·2, and with still others, 10·4. These are provisionally regarded as the atomic volumes of oxygen in its solid compounds. These different values for a single element are of course natural consequences of the fact that the various compounds containing it, undergo, in their formation, different degrees of condensation from the free elements composing them. But, in assuming these three numbers to be the atomic volumes of oxygen, one assumption is made which is by no means allowable, viz: that the metals entering into these compounds undergo no condensation themselves, all the change in volume taking place in the oxygen. But, some oxyds have atomic volumes lower than those of the metals contained in them. And yet the remarkable multiple relation between the three values quoted above cannot be due to accident. These points will be referred to again.

Third, we come to some results which I myself published rather more than a year ago.* In my last paper upon this subject I pointed out some curious multiple relations connecting the atomic volumes of similar solid elements. For the proofs of these relations I must refer to the above mentioned paper, but the values for the elements themselves I am obliged to cite here for further reference. I give the values for solids only. Li 11·4, Na 22·8, K 45·6, Rb 57·0. These stand to each other as 1 : 2 : 4 : 5. I 25·6, Tl 17·2, Ag and Au 10·2.

Here arises an interesting question. The atomic volumes of Ag and Li are quite near together. But the specific gravity of lithium was determined comparatively near the melting point of that metal, while that of silver was taken at a temperature greatly removed from the degree at which it fuses. Now *melted* silver, according to Playfair and Joule, has the specific gravity 9·206; and consequently an atomic volume of 11·7, nearly that of solid lithium. May not these two metals then, under similar circumstances, and at strictly comparable temperatures, be supposed to have equal atomic volumes?

O 2·6, 5·2, 10·4. S 10·4, 15·6. Se 10·4, 15·6.(?) Te 20·8. This value for S is that of the octahedral variety, prismatic sulphur having 16·3—16·7. Amorphous selenium has an atomic volume of 18·6.

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* This Journal for March and May, 1869.

Sr, 34·3. Ba, 34·2.
Ca, 25·8. Pb, 18·2.

As and P 12·9, Sb 17·2, Bi 21·5. These stand as 3 : 4 : 5. This value for P is that of the so-called "metallic" variety. Common phosphorus has an atomic value of 16·9—17·0, (perhaps 17·2, like Sb?) and the ordinary red modification 13·9—14·5. Amorphous arsenic has the value 15·9. Bo 4·1. Vanadium in my last paper I calculated theoretically from the atomic volume of one of its oxyds, making it equal to As and P. Since then, however, this idea has been overthrown, Roscoe having determined the specific gravity of the metal itself. This, 5·5, gives an atomic volume of 9·4, which differs widely from my supposititious value, and bears no definite relation which I can see, to those of As and P.

C (graphite), 5·5, Si 11·0, Ti 11·0 (?), Sn 16·5. These stand as 1 : 2 : 2 : 3. C (diamond) 3·4. Zr 21·7 (perhaps 22·0, or 5·5×4.)

Cr, Mn, Fe, Co, Ni, U,[*] and Cu,	6·9	These four values stand as 3 : 4 : 5 : 6.
Zn, Pt, Ir, Os, Pd, Ru, and Rh,	9·2	
Mo and W,	11·5	
Cd, Mg, and Hg, (solid)	13·8	

Gl 43,[†] Al 10·1—10·6, Th 30·4—30·9, Ce 16·7, In 10·2.

Before proceeding farther, it now becomes necessary for me to allude briefly to some regularities in atomic volumes which have already been traced by others. First, the alums have equal atomic volumes. To the evidence which has been cited by other investigators in proof of this, may now be added the atomic volumes of rubidium and cæsium alums, whose specific gravities, taken by Redtenbacher, show that they follow the regular rule. Second, a number of similar carbonates, especially those of Zn, Mg, Fe, and Mn, have equal atomic volumes. Third, a similar relation connects the vitriols with 7 aq. This series is especially important, since it contains the sulphates of Fe, Ni, Co, Mg, Zn, and Cd. Fourth, the oxyds allied to Gahnite, with the general formula MO, M_2O_3, are, with one or two exceptions, equal in atomic volume. Fifth, many corresponding phosphates and arsenates have equal values. Other series have been traced here and there, but these are perhaps the most striking.

Now, in the first place, we may lay it down as a general rule that when two similar elements have equal atomic volumes, the atomic volumes of their corresponding compounds will also be equal. Of the truth of this I will cite a few examples, and point out some exceptions. For want of space I cannot adduce all the cases I have accumulated, however, but will merely attempt to illustrate the principle, and afterwards bring the exceptions under another rule.

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[*] Possibly this may not belong in this group, but its equal atomic volume be merely a coincidence. The values of some of its compounds suggest this idea.

[†] Calculated from the lower atomic weight for Gl. In my last paper I used the higher.

Barium and strontium. $SrCl_2$, sp. gr. 2·8033, Karsten. $BaCl_2$, 3·704, Karsten. At. vols., 56·5 and 56·2.

SrI_2 4·415, Bödeker. BaI_2 4·917, Filhol. At. vols., 77·3 and 79·5. SrH_2O_2, 8 aq., 1·396 Filhol, BaH^2O_2, 8 aq., 1·656, Filhol. At. vols., each 190·2.

The sulphates and nitrates, bromids, carbonates, and simple hydrates, are either doubtful or exceptions. In all these cases the compounds of Ba have the highest values.

Iron group. Here examples are so abundant that I will only mention a few. MnO sp. gr., 5·38, P. & J.* NiO 5·597, P. & J. CoO 5·597—5·750, P. & J. UO 10·15, Ebelmen. CuO 6·130, Boullay. At. vols., respectively, 13·0, 13·4, 13·0—13·4, 13·4, 12·9.

Cr_2O_3 4·950, P. & J. Fe_2O_3 4·679—5·135, P. & J. NiO_3 4·814, P. & J. Co_2O_3 4·814, P. & J. At. vols., 30·7, 31·1—34·2, 34·4, 34·4. Mispickel, FeS_2, $FeAs_2$ 6·0—6·4 Gersdorffite, NiS_2, $NiAs_2$ 5·6—6·9. Cobaltite, CoS_2, $CoAs_2$, 6·0—6·3. At. vols., 50·9—54·3, 48·1—59·2, 52·6—55·3.

FeS 5·035. P. & J.: NiS 5·650, Rammelsberg; 4·601 Kenngott. CoS 5·45. At. vols., 17·5, 16·1–19 7, 16·7. Exceptions, MnS and CuS, having sp. grs. 3·95, Dana's Min., and 4·163, Karsten; and at. vols., 21·8 and 22·9. It is worth noting that these two sulphids stand at the ends of the series; CrS and US, being (if they exist), wholly unstudied in this direction.

One more striking exception may be found by comparing FeS_2, MnS_2, and CoS_2, whose atomic volumes are very dissimilar.

Platinum group. 2KCl, $PtCl_4$; and 2KCl, $IrCl_4$. Sp. grs. Bödeker, 3·586 and 3·546. At. vols., 136·2 and 137·9. PdP_2 and PtP_2 have unequal values.

Molybdenum and tungsten. It has been proved by other authorities that MoO_3 and WO_3 have equal atomic volumes.

Cadmium group. CdO 6·9502, Karsten. HgO 11·344, P. & J. At. vols., 18·4 and 19·0. MgO has an exceptional value which will be studied in another connection.

Phosphorus and arsenic. Cu_3P 6·59, Hvoslef. Cu_3As 7·62, Genth. At. vols., 33·6 and 34·9.

P_2O_5 2·387, Brisson. As_2O_5 4·25 Filhol; 3·7342, Karsten. At. vols., 59·5 and 54·1—61·6.

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* Playfair and Joule. This is the only abbreviation of the sort which I shall have occasion to employ. When I give no authority for a specific gravity, it will be found in "Dana's Mineralogy," last edition.

Phosphates and arsenates have been compared by others, Where their values differ, that of the arsenate is the highest.

Silicon and titanium. The acids of this group will be cited farther along. Fayalite, $2FeO\ SiO_2$ 4·006. $2FeO\ TiO_2$ 4·370 Hautefeuille. At. vols., 50·9 and 51·7.

The only definite exception is found in comparing $2KF\ SiF_4$; sp. gr. 2·6652, Stolba; and $2KF\ TiF_4$, 2 080, Bödeker. At vols., 82·5 and 116·3.

These examples will suffice for the present, although I shall have occasion to cite others to illustrate another point. Upon comparing all the material which I have collected, I find the exceptions to be quite rare, although, perhaps, I have given them undue prominence here.

Second, similar compounds of similar metals often have equal atomic volumes, even when those of the metals themselves are unequal. The corresponding compounds of the iron, platinum, and cadmium groups often exemplify this most strikingly. The exceptions to the previous rule are probably due to this. Thus, although Mg and Cd have equal atomic volumes, their oxyds show no such equality; that of the first metal having shaded off into approximate uniformity with those of its kindred iron group.

Of the rule under consideration, however, the most striking case hitherto adduced is that of the vitriols. As I have already stated, the sulphates with 7 aq. of Fe, Ni, Co, Zn and Mg, have equal atomic volumes, while the double salts of the same class, containing in addition to the above metals Cu and Cd, follow the same rule. A similar regularity connects the anhydrous sulphates of most of these metals. But an equally remarkable series is formed by the chlorids of the same metals, which, of the formula MCl_2, have, as far as they have been studied, atomic volumes which are very nearly equal. If we present these chlorids in tabular form, taking the average of their observed atomic volumes as the real value, we shall find that the variations from their average are wholly within the limits of experimental error.

$FeCl_2$.	Sp. gr.	2·528, Filhol.	At. vol., found,	50·2	Mean, 47·6.	Sp. gr. calc., 2·668
$NiCl_2$	"	2·560, Schiff.	" "	50·7		" " 2·726
$CoCl_2$	"	2·937, P. & J.	" "	44·2		" " 2·726
$CuCl_2$	"	3·054, "	" "	44·0		" " 2·825
$ZnCl_2$	"	2·753, Bödeker.	" "	49·4		" " 2·857
$PtCl_2$	"	5·8696. "	" "	45·7		" " 5·638
$MgCl_2$	"	2·177, P. &J.	" "	43·6		" " 1·996
$CdCl_2$	"	3·6254, Bödeker,	" "	50·5		" " 3·844
$HgCl_2$	"	5·4032, Karsten,	" "	50·1		" " 5·693

Now, if we take into account the difficulty of obtaining perfectly accurate determinations of sp.gr. for some of these substances; and also bear in mind that for most of them only single observations have been made, it is clear that every variation from equality may be ascribed to errors in experiment. The extreme difference in atomic volume is from a minimum of 43·6 to a maximum of 50·7. It is not rare for an equal divergence to occur between different determinations for a single substance.

Again, although chlorids, bromids, and iodids have unequal atomic volumes, those of similar chlorates, bromates, and iodates are equal,—at least as far as we have any data.

$NaClO_3$, sp. gr., 2·289, Bödeker. $NaBrO_3$, 3·339, and $NaIO_3$, 4·277, Kremers. At. vols., 46·5, 45·2, and 46·3.

$KClO_3$, sp. gr., 2·325, Buignet. $KBrO_3$, 3·271, and KIO_3, 3·979, Kremers. At. vols., 52·7, 51·1, and 53·8.

Most similar compounds of silver and sodium have equal atomic volumes. This holds true of the chlorids, bromids, and iodids (as will be shown in another connection hereafter), and of the sulphates, chlorates, nitrates, and probably carbonates. The sulphids and oxyds are exceptions.

Some compounds of As, Sb, and Bi, have equal values.

As_2O_3, sp. gr., 3·695, Guibourt; 3·884, Filhol. Sp_2O_3, 5·11, Terreil; 5·78, Boullay. Bi_2O_3, 8·079, P. & J.; 8·450, Leroyer & Dumas. At. vols., 51·0—53·6; 50·5—57·1; and 55·4—57·9.

In the carbon group compare certain oxyds.

SiO_2, 2·663, Deville. TiO_2 (artificial anatase) 3·700, Hautefeuille. SnO_2 6·720, Daubrée. At. vols., 22·5, 22·2, and 22·3.

Would space permit, I might go on multiplying examples to an almost indefinite extent; but my object at present is merely to illustrate certain principles. There are other regularities yet to be noticed. The first of these is comparatively unimportant, and I will merely state it as it is, without adducing any evidence. All sulphates have atomic volumes which are lower than the sum of the metallic oxyds and the SO_3. That is, the value for $MnSO_4$ is less than the sum of those of MnO and SO_3. To this rule I have found no exceptions. Whether a similar rule holds good for chromates, molybdates, and tungstates, I am not certain; the materials at my command being too limited to settle the question.

Another regularity is more remarkable. It is noteworthy that certain oxyds and sulphids have atomic volumes which are lower than those of the metals contained in them. Now, in a number of cases, I find that those atomic volumes equal *half* the sum of those of the metal and the other element, provided that S=15·6, and O receives its highest value, 10·4. I give oxygen this value, for this reason. Many sulphids have atomic volumes, as I have already stated, equal to the sum of those of the metal and sulphur in the free state; so that it is probable that some oxyds follow the same rule. 10·4 being the highest number for oxygen, renders it likely that that is the true value for this element in those oxyds corresponding to the above mentioned sulphids.

Now, two sulphids appear to follow this rule, viz: Na_2S and K_2S, whose sp. gr., given by Filhol, are respectively 2·471 and 2·130. At. vols., 31·6 and 51·6. Now $\frac{2(22·8)+15·6}{2} = 30·6$; and $\frac{2(45·6)+15·6.}{2} = 53·4$. If these theoretical atomic volumes are true, then the sp. gr. of Na_2S will be 2·549, or 0·078 greater than the value found; while that of K_2S will be 2·059, varying 0·071 from Filhol's number. These variations are wholly within the limits of error for such compounds.

But the results obtained with four oxyds are more striking.

MgO.	Sp. gr.	3·200,	Karsten.	At. vol.	12·5.	$\frac{13·8+10·4}{2} = 12·1.$	Error	0·4
CaO.	"	3·161,	"	"	17·7.	$\frac{25·8+10·4}{2} = 18·1.$	"	0·4
SrO.	"	4·611,	Filhol.	"	22·4.	$\frac{34·8+10·4}{2} = 22·35.$	"	0·05
Al_2O_3.	"	3·928,	Ebelmen.	"	25·9.	$\frac{2(10·2)+3(10·4)}{2} = 25·8.$	"	0·1

The oxyds of barium, sodium, and potassium, do not follow this rule.

One more regularity traced, and I am done. If my views concerning multiple relations are correct, and all the values for oxygen and sulphur are multiples of the lowest, then we must expect that compounds formed by the union of these elements will have atomic volumes which are also multiples. Now, sulphuric anhydrid, SO_3, has, according to Buff, the sp. gr. 1·909 at 25°, and according to Baumgartner 1·975. Its atomic volume, then, is from 40·5 to 41·9. And 41·6 is precisely four times 10·4!

The chlorids, bromids, and iodids of the alkali metals and silver, seem to afford a similar example. In one of my previous papers I showed that Kopp's values for Cl, Br, and I, in their liquid compounds were almost exact multiples of his number for H, 5·5 Consequently, judging from analogy, it is likely that these elements in their solid compounds would follow a similar rule. Now the metals Li, Na, K, and Ag, have atomic volumes which do not vary greatly from multiples of 5·5. And, in accordance with what we should expect, their chlorids, bromids, and iodids have atomic volumes which are either exact, or so nearly exact, multiples of 5·5, that the circumstance cannot be ascribed to accident.

I present a tabular view of this regularity.

LiCl.	Sp. gr.	1·998, Kremers.	At. vol. found,	21·3.	Calc.,	22·0.	Sp. gr. calc.,	1·932
NaCl	“	2·145, Buignet.	“ “	27·2	“	27·5	“ “	2·127
KCl	“	1·945, Kopp.	“ “	38·3	“	38·5	“ “	1·935
AgCl	“	5·130, Herapath.	“ “	27·9	“	27·5	“ “	5·218
NaBr	“	3·079, Kremers.	“ “	33·4	“	33·0	“ “	3·121
KBr	“	2·672, P. & J.	“ “	44·5	“	44·0	“ “	2·704
AgBr	“	5·8–6·0.	“ “	32·3–32·4	“	33·0	“ “	5·697
NaI	“	3·450, Filhol.	“ “	43·5	“	44·0	“ “	3·409
KI	“	3·056, Filhol.	“ “	54·3	“	55·0	“ “	3·018
AgI	“	5·350, Schiff.	“ “	43·9	“	44·0	“ “	5·341

On comparing these numbers, it will at once be noticed, not only that the variations from theory are remarkably slight, but also (which has been previously stated) that the corresponding compounds of Na and Ag have nearly equal, or equal atomic volumes, that the values of potassium compounds exceed those of the sodium compounds by 11·0, (5·5×2), that bromids exceed chlorids by 5·5, and that iodids have atomic volumes 11·0 greater than bromids. These regularities in difference between chlorids, bromids, and iodids, however, do not appear so distinctly in other series of them.

Now, to sum up the important relations traced in this paper, bearing in mind that in many cases exceptions exist.

First. When similar metals have equal atomic volumes, those of their similar compounds will also be equal.

Second. Metals whose atomic volumes are unequal, but simply related to one another, often form similar compounds having equal values.

Third. Some compounds have atomic volumes which stand in very simple relations to the sums of those of the free elements which form them.

Fourth. Compounds formed by the union of similar elements, have atomic volumes which are multiples of the lowest for that group.

And, fifth, we may add the multiple relations traced in my former papers, which not only connect the atomic volumes of different elements, but the various values for each single element.

Now, what do these regularities mean. Are we justified either in drawing any direct conclusions from them, or in basing upon them any general theory of atomic volumes? To this I must answer, that, although no generalization is absolutely established by them, it seems to me that one is decidedly hinted at. May we not say that in all compounds, the atomic volume of every element will be either a perfect multiple of the lowest value for that element, or of the lowest value in the group to which it belongs? Although this theory cannot be regarded as entirely proved, it certainly possesses a considerable degree of probability, and seems to harmonize well with the regularities which I have pointed out. But why an element should have a higher value in one compound than in another, remains to be accounted for, although upon this point, perhaps, Buff's idea that the different degrees of quantivalence of an element in its various compounds, cause the differences in its atomic volumes, may prove correct.*

But at all events, whether the theory which I put forward turns out true or false, it may, perhaps, by lending some system to the study of atomic volume, pave the way for something of greater value.

Boston, May 30th, 1870.

ART. XIX.—*Considerations on the apparent inequalities of long period in the mean motion of the Moon;* by SIMON NEWCOMB.

[Read to the National Academy, April, 1870.]

THE problem of determining the motion of the moon around the earth under the influence of the combined attraction of the sun and planets has, more than any other, called forth the efforts of mathematicians and astronomers. Nearly every great geometer since Newton has added something to the simplicity or the accuracy of the solution, and, in our own day we have seen it successfully completed in its simplest form, in which the earth, the moon, and the sun are considered as material points, moving under the influence of their mutual attractions. The satisfactory solutions are due to the genius of Hansen and of Delaunay. Working independently of each other, each using a method of his own invention more rigorous than had before been applied, they arrived at expressions for the longitude of the moon which, being compared, were found to exhibit an average discrepancy of less than a second of arc. No doubt could remain of the substantial correctness of each.

The solutions here referred to exhibit only inequalities of short period in the motion of the moon. But, it has long been known, from observation, that the mean motion of the moon is subject to apparent changes of very long period, and especially to a secular acceleration by which it has been gradually increasing, from century to century, since the time of the earliest recorded observations. If we inquire into the problem of these inequalities of long period, we shall find it seemingly no nearer a final solution than it was left by La Place, observation having since added more anomalies than theory has satisfactorily shown to exist.

The first inequality in the order of discovery was the secular acceleration. This was discovered about the middle of the last century by a comparison of ancient eclipses with modern observations. Its cause was first discovered by La Place, who showed that it was due to the effect of the action of the planets in changing the eccentricity of the earth's orbit.

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* See Buff's paper in the Annalen d. Chem. u. Pharm., 4th supplement vol., 1865–6. Or, see his "Grundlehren der theoretischen Chemie."

The results of his computations agreed substantially with observations, and was therefore received with entire confidence until less than twenty years ago. The question being then taken up by Mr. John C. Adams, this eminent mathematician was led to the conclusion that La Place's result was nearly twice too large.

The same conclusion was reached independently by Delaunay, and gave rise to a remarkable discussion, the history of which is too familiar to be now recounted. It is now conceded that the value found by Adams and Delaunay is theoretically correct.

The new result no longer agreeing with observation, the difference is now accounted for by an increase in the length of the day. That this length is increasing is also known from theoretical considerations, but the data for its accurate determination are wanting.[*]

In the third volume of the *Mécanique Céleste* (Seconde Partie, Livre vii, Chapitre v) La Place discusses an apparent inequality of long period in the motion of the moon. The discussion is mainly empirical. The existence of the inequality is inferred from observations, these showing that the mean motion of the moon during the half century following 1756 was less than during the half century preceding. He then assumed that the inequality was due to the fact that twice the mean motion of the moon's node, plus the motion of its perigee, minus that of the sun's perigee was a very small quantity, less than two degrees per annum, and determined the coefficient of the varying angle solely from the observations. The result was that these might be satisfied by supposing the inequality of mean longitude

$$\delta l = 47''\cdot51\ [\text{or } 15''\cdot39]\ \sin\,(2\,\text{☊}\text{☽} + \pi\text{☽} - 3\pi\odot)$$

If, in this expression, we substitute Hansen's values of the elements, it becomes

$$\delta l = 15''\cdot39\ \sin\,[173^\circ\ 26' + (1^\circ\ 57'\cdot4)\ (t - 1800)].$$

When in 1811 Burckhardt constructed his tables of the moon, he omitted the sun's perigee from this argument by the authority of La Place, himself, who now attributed the inequality to a difference of compression between the two hemispheres of the earth. The function was also changed from *sin* to *cos* and the coefficient altered. The adopted term thus became

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[*] The time and place when the discordance referred to was first distinctly attributed to the tidal retardation of the earth having been a subject of discussion, the following extract from an article on "Modern Theoretical Astronomy" in the North American Review for October, 1861 (vol. 93, p. 385), may not be devoid of interest.

"It seems to be well established that the new theory is inconsistent with the observations of ancient eclipses, and if it should prove to be correct, we may be driven to the conclusion, that a portion of the acceleration proceeds from some other cause than the attraction of gravitation, or that the length of the day is gradually increasing to an extent which has become perceptible from the cause to which we have already referred [the tidal retardation, p. 374]. If, as centuries roll by, the day should gradually increase, the moon would move a little farther in the course of a day than if no such increase should take place. Since, in our calculations, we suppose the day constant, the apparent acceleration would be greater than the real—precisely the effect observed. The difference can be entirely accounted for by supposing an increase of something less than one thousandth of a second per century in the length of the day, and a corresponding diminution in the lunar month."

$$\begin{aligned}\delta l &= -12''\cdot5 \cos\left[291^\circ\ 57' + (2^\circ\ 0'\cdot45)(t-1800)\right]\\ &= 12''\cdot5 \sin\left[201^\circ\ 57' + (2^\circ\ 0'\cdot45)(t-1800)\right]\end{aligned}$$

Succeeding investigators have regarded the theoretical coefficients of both of these terms as insensible. It does not seem likely that there is any such difference between the two terrestrial hemispheres as could produce the second, but I am not aware that the coefficient of the first has ever been shown to be insensible by any published computation. This coefficient is of the ninth order and the argument is,

In Delaunay's notation, $3D-2F-l+3l'$;
In Hansen's, $w-3w'$.

The period is 184 years, and the large value of the ratio of this period to that of the moon itself might render the coefficient sensible. Both Hansen and Delaunay pronounce it insensible, but neither publish their computations of its magnitude.

These terms have ceased to figure in the theory of the moon since Hansen announced that the action of Venus was capable of producing inequalities of the kind in question. So far as I am aware, Hansen's first publication on this subject is that found in No. 597 of the Astronomische Nachrichten (B. 25, S. 325.) Here, in a letter dated March 12, he alludes to La Place's coefficients, and says he has not been able to find any sensible coefficient for La Place's argument of long period. But on examining the action of Venus on the moon he found, considering only the first power of the disturbing force, the following term in the moon's mean longitude:

$$\delta l = 16''\cdot01 \sin(-g-16g'+18g''+35^\circ\ 20').$$

g, g' and g'' being the mean anomilies of the moon, the earth and Venus respectively. As this expression still failed to account for the observed variations of the moon's longitude he continued the approximation to the fourth power of the disturbing force, and found that the terms of the third and fourth order increased the coefficient to $27''\cdot4$, the angle remaining unchanged, so that the term became

$$27''\cdot4 \sin(-g-16g'+18g''+35^\circ\ 20'),$$

But this increase made the theory rather worse, and the term depending on the argument of Airy's equation between the earth and Venus was then tried with the result—

$$\delta l = 23''\cdot2 \sin(8g''-13g'+315^\circ\ 30').$$

The introduction of this term seemed to reconcile the theory with observation.

Hansen finally remarks that these values of the coefficients are still subject to some uncertainty from his not having employed decimals enough in his computation.

In a letter to the Astronomer Royal, published in the Monthly Notices of the Royal Astronomical Society for Nov. 1854, Hansen gives a statement of the elements employed in his tables of the moon, and refers to the subject of these inequalities in the following terms:—

"The accurate determination of these two inequalities by theory is the most difficult matter which presents itself in the theory of the moon's motion. I have on two occasions and by different methods sought to determine their values, but I have obtained results essentially different from each other. I am now again engaged with their theoretical determination by a method which I have simplified, and hope to bring the operation to a definitive close. I have also applied to my tables some coefficients which are not free from empiricism but which I can justify by the circumstance that they represent the ancient as well as the modern observations with great exactness, and it may be expected that they will represent the future observations equally well."

Hansen's lunar tables were published in 1857.

The terms of long period finally adopted are

$$15''\cdot34 \sin(-g-16E+18V+30^\circ\ 12')$$
$$+21\cdot48 \sin(8V-13E+274^\circ\ 14'),$$

V and E representing the mean longitudes of Venus and the earth. Changing them to mean anomalies the terms become

$$15''\cdot34 \sin(-g-16g'+18g''+33^\circ\ 36')$$
$$+21\cdot47 \sin(8g''-13g'+4^\circ\ 44').$$

It appears that while the first term has been restored to what was substantially its original value, when only the first power of the disturbing force was included, the argument of the second term has been changed by 50°, the coefficient being but slightly changed.

In a letter to the Astronomer Royal, dated 1861, Feb. 2d, found in the Monthly Notices for March, 1861, Hansen again refers to this second term with the statement that its coefficient is one of those somewhat empirical. At the same time he has found the coefficient, by his last theoretical determination of it, by no means insensible, like Delaunay. He adds that in the comparison with observation he has never gone beyond Bradley, nevertheless his tables satisfactorily represent the ancient observations.

A well marked feature of Hansen's published works is the copiousness and perspecuity with which his theoretical calculations are laid down. But, so far as I am aware he has never published any computation of these inequalities except that part of the first inequality which depends on the first power of the disturbing force. This computation is found in vol. xvi of the Memoirs of the Royal Astronomical Society. In the second part of his "Darlegung" we find a general method of treating inequalities of long period, but—unless I have overlooked it—no computation of any particular inequality. Nor do we find any statements of the numerical results of Hansen's various computations except those already quoted.

The only geometer besides Hansen who has attacked the problem of these inequalities is Delaunay. His researches are published in full in the Additions to the Connaissance des Temps for years 1862 and 1863. For the first approximation to the first inequality his result is

$$16''\cdot02 \sin(-l-16l'+18l''+35^\circ\ 20'\cdot2)$$

a result practically identical with that of Hansen. The ulterior approximations change it to

$$16''\cdot34 \sin(-l-16l'+18l''+35^\circ\ 16'\cdot5),$$

so that they increase the coefficient instead of diminishing it as in Hansen's theory. The difference is however so small that the results may be regarded as identical.

But, in the case of the second inequality instead of reproducing the result of Hansen, he finds a coefficient of only 0''·27, a quantity quite insignificant in the present state of the question. We have thus an irreconcilable difference on a purely theoretical question.

I propose to inquire whether we have in either theory an entirely satisfactory agreement with observation. As a preliminary step to this inquiry I have prepared the following table of the mean longitude of the moon from the tables of Burckhardt and of Hansen respectively, for a series of equidistant dates, the interval being 3652·5 days, and the epoch 1800 Jan. 0, Greenwich mean noon. These dates are marked by the year near the beginning of which they fall. Column L_0 gives Burckhardt's mean longitude on the supposition of uniform motion, from the data given on the fifth page of the introduction to his tables. Next is given the acceleration of the mean longitude deduced from Table XLVIII. The inequality of long period is from Table XLIX. The sum of these three quantities gives the corrected mean longitude.

Hansen's mean longitude and secular acceleration are deduced in the same way from the elements given on page 15 of his *Tables de la Lune.* His terms of long period are deduced from Tables XLI and XLII, the constants being subtracted and the remainder reduced to arc by being multiplied by the factor 0''·004703. The last column of the table gives the correction to Burckhardt's mean longitude to reduce it to that of Hansen. That this difference is really the mean difference between the longitudes of the moon deduced from the two tables is shown by its agreement with the known difference at particular epochs. At the end of the British Nautical Almanac for 1862 is found a comparison of the two tables, from which it appears that Burckhardt's mean longitude was then greater than Hansen's by about 14″·2. The general agreement between 1750 and 1800, when both tables agreed with observations, shows that the difference of mean motion is certainly affected with no sensible error.

Year.	Burckhardt.								Hansen.							H.–B.
	L_0			Sec. Var.	Long Period.	Corr. Mean Longitude.			L_0		Sec. Var.	Long Period.	Mean Longitude.			
	°	′	″	″	″	°	′	″	′	″	″	″	°	′	″	″
1630	100	19	28·0	+4·9	−8·0	100	19	24·9	18	14·4	+38·5	−21·4	100	18	31·5	−53·4
40	347	6	45·4	+3·6	−10·8	347	6	38·2	5	36·3	+34·1	−20·0	347	5	50·4	−47·8
50	233	94	2·7	+2·5	−12·3	233	53	52·9	52	58·3	+30·0	−17·2	223	53	11·1	−31·8
60	120	41	20·1	+1·6	−12·3	120	41	9·4	40	20·3	+26·1	−13·1	120	40	33·3	−36·8
70	7	28	37·4	+0·9	−10·8	7	28	27·5	27	42·2	+22·5	−8 1	7	27	56·6	−30·9
80	254	15	54·8	+0·4	−8·0	254	15	47·2	15	4·2	+19·2	−2·3	254	15	21·1	−26·1
90	141	3	12·1	+0·1	−4·2	141	3	7·8	2	26·1	+16·1	+3·9	141	2	46·1	−21·7
1700	27	50	29·5	+0·0	+0·2	27	50	29·7	49	48·1	+13·3	+10·0	27	50	11·4	−18·3
10	274	37	46·8	+0·1	+4·4	274	37	51·3	37	10·0	+10·8	+15·6	274	37	36·4	−14·9
20	161	25	4·2	+0·4	+8·3	161	25	12·9	24	32·0	+8·5	+20·5	161	25	1·0	−11·9
30	48	12	21·5	+0·9	+11·0	48	12	33·4	11	59·9	+6·5	+24·2	48	12	24·7	−8·7
40	294	59	38·9	+1·6	+12·4	294	59	52·9	59	15·9	+4·8	+26·4	294	59	47·1	−5·8
50	181	47	56·2	+2·5	+12·2	181	47	10·0	46	37·9	+3·3	+26·9	181	47	8·1	−2·9
60	68	34	13·6	+3·6	+10·6	68	34	27·8	33	59·8	+2·1	+25·7	68	54	27·6	−0·2
70	315	21	30·9	+4·9	+7·8	315	21	43·7	21	21·8	+1·2	+22·9	315	21	45·9	+2·2
80	202	8	48·3	+6·4	+3·9	202	8	58·6	8	43·7	+0·5	+18·5	202	9	2·7	+4·1
90	88	56	5·6	+8·1	−0·4	88	56	13·4	56	5·7	+0 1	+12·8	88	56	18·6	+5·2
1800	335	43	23·0	+10·0	−4·7	335	43	28·4	43	27·7	0·0	+6·1	335	43	33·8	+5·4
10	222	30	40·4	+12·1	−8·3	222	30	44·2	30	49·6	+0·1	−1·1	222	30	48·6	+4·4
20	109	17	57·8	+14·4	−11·0	109	18	1·2	18	11·6	+0·5	−8·4	109	18	3·7	+2·5
30	356	5	15·2	+16·9	−12·4	356	5	19·7	5	33·5	+1·2	−15·4	356	5	19·3	−0·4
40	242	52	32·5	+19·6	−12·2	242	52	39·9	52	55·5	+2·1	−21·6	242	52	36·0	−3·9
50	129	39	49·9	+22·5	−10·6	129	40	1·8	40	17·5	+3·3	−26·5	129	39	54·3	−7·5
60	16	27	7·2	+25·6	−7 6	16	27	25·2	27	39·4	+4·8	−29·8	16	27	14·4	−10·8
70	263	14	24·6	+28·9	−3·8	263	14	49·7	15	1·4	+6·5	−31·3	263	14	36·6	−13·1

Burckhardt's tables have been selected for this comparison because they have been extensively compared with observations made before 1700. The additions to the Connaissance des Temps for 1824 contain a paper by Burckhardt himself giving a comparison of his tables with observations of occultations made by Flamstead, Hevelius and others, between 1637 and 1700. The general result of this comparison is that the mean longitude of his tables could hardly have been more than a very few seconds in error in the year 1670. But, the preceding table shows that for this epoch Hansen's mean longitude is 30″ less than Burckhardt's. Therefore, unless we suppose Burckhardt's investigation to be affected with some egregious systematic error we must admit that the mean longitude of Hansen's tables for the epoch 1670 is about 30″ too small.

Desiring an independent test of this conclusion I have selected certain observations which, with the data available, seemed well fitted to answer this purpose and compared them directly with Hansen's Tables.

They are

1. Occultation of Aldebaran, 1680, Sept. 13, observed at Greenwich by Flamstead.
2. Occultation of the same star 1680, Nov. 7, observed at Greenwich by Flamstead, and at London by Halley.
3. Total eclipse of the sun 1715, May 3, observed at London, Greenwich and Wanstead by Halley, Flamstead and Pound.

To compute the occultations of Aldebaran the mean position for 1680·0 was derived from Le Verrier's Tables (Annales de l'Observatoire, Tome II) correcting the right ascension by $+0^s{\cdot}01$, and was as follows:

$$\alpha(1680) = 4^h\ 17^m\ 37^s{\cdot}01$$
$$\delta\ .\ .\ .\ .\ +15^\circ\ 49'\ 11''{\cdot}8$$

The corrections for reduction to apparent place are

for Sept. 13, $\Delta\alpha = +2^s{\cdot}90$; $\Delta\delta = +1''{\cdot}1$
Nov. 7, $\Delta\alpha = +4{\cdot}18$ $\Delta\delta = +2{\cdot}4$

The following geocentric positions of the moon were derived from Hansen's Tables.

Date (Julian Cal.)	Sept. 18.						Nov. 7.					
	h	m	s	h	m	s	h	m	s	h	m	s
Gr. Mean Time,	15	0	53	16	12	53	7	50	39	8	48	15
☽'s Longitude,	64°	54′	24″·3	65°	37′	20″·4	64°	33′	11″·6	65°	9′	49″·6
" Latitude,	–4	45	29·8	–4	48	10·6	–4	39	26·9	–4	40	48·0
" Parallax,	0	59	30·0	0	59	28·8	1	1	18·5	1	1	17·8

From these data we derive the following times for the immersion and emersion of Aldebaran for the dates in question. The observed times have been concluded from the observed altitudes and clock times given by Flamstead in the *Historia Celestis*, kindly furnished me by Prof. Winlock. They differ but little from the results of Flamstead himself, when the latter are corrected for the equation of time.

		Computed.			Observed.			O—O.
		h	m	s				s
Sept. 13,	Immersion,	15	2	49	15	0	53	+116
	Emersion,	16	10	5	16	9	12	+ 53
Nov. 7.	Immersion,	7	51	47	7	50	48	+ 64
	Emersion,	8	48	16	8	47	12	+ 64

The great difference between the results of the two phases of the first occultation gives rise to a suspicion of error in the observations or the data of reduction. The second observation is confirmed by that of Halley in London, he having observed the immersion at $7^h\ 50^m\ 9^s$, and noticed that the star was "newly emerged" at $8^h\ 47^m\ 1^s$. His place of observation was probably twenty-five or thirty seconds west of Greenwich, and therefore his observation agrees well with that of Flamstead. The discordance between the observed and computed times, of this second occultation indicates a correction of about +32″ to Hansen's mean longitude at the epoch 1680, and the first may be considered as confirming this correction in direction, if not in amount.

For the eclipse of May 3, 1715 we have the following computed and observed times. I have assumed Halley's station to be in latitude 51° 31′ and longitude 25ˢ west. Pound's is taken in accordance with his own statement to be in latitude 51° 34′, and longitude 8ˢ west. These agree pretty well with Flamstead's statements that Wanstead is seven or eight miles N. by E. from Greenwich,* and that Crane Court is half a minute of time West of Greenwich.

Halley at London.

	Computed.			Observed.			C—O
	h	m	s	h	m	s	s
First contact,	20	2	35	20	2	37	— 2
Beginning of Totality,	21	5	52	21	5	39	+13
End of "	21	9	3	21	9	2	+ 1
End of Eclipse,	22	16	55	22	16	37	+18

Pound at Wanstead.

	Computed.			Observed.			C—O
	h	m	s	h	m	s	s
Eclipse first perceived,	20	3	18	20	3	15	+ 3
The total immersion,	21	6	38	21	6	6	+32
The emersion,	21	9	48	21	9	26	+22
The just end of the eclipse.	22	17	42	22	17	10	+32

The only information I have respecting Flamstead's observations is contained in a letter of his found in Baily's 'Life and Correspondence of Flamstead, p. 315, from which it appears that his times differ only a few seconds from Halley's, instead of differing by the half minute required by the difference of meridians. An obvious slip of the pen, (*later* being written instead of *earlier*) makes it doubtful in which way the "few seconds" are to be counted. It can, however, be fairly inferred from his statement that his observations diverge from the tabular times as much or more than Pound's.

The discordance of the results of first and last contact may be attributed to this cause: that with their imperfect telescopes the observers did not begin to see the moon until several seconds after the actual commencement of the eclipse, and lost sight of it a few seconds before the actual end. The discordance in the duration of totality indicates with a high probability that the computed shadow path falls a few miles too far north. In this case the mean of the results for beginning and end of totality will be about right, and we have for the excess of computed times

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* Baily's Flamstead, p. 316, p. 328.

Halley's observations,	$+ 7^s$
Pound's,	$+ 27$
Flamstead's,	$+ 30 \pm$

I infer from these results that the correction to Hansen's mean longitude at the epoch 1715 is about +11″.

Comparing the corrections thus found for the epochs 1680 and 1715, we find they are substantially those required to reduce Hansen's mean longitude to Burckhardt's. I conclude, therefore, that no egregious systematic error has crept into the researches by which Burckhardt sought to show that the epoch of his tables was substantially correct during the latter half of the seventeenth century, and that the difference between the mean longitude of Hansen and Burckhardt during that period represents approximately, at least, errors of Hansen's mean longitude.

The observations of the moon made at the observatories of Greenwich and Washington during the last ten years, indicate a tabular deviation of a remarkable character. From 1850 to 1862 we find the moon slowly running ahead of the tables, until the latter required a correction of plus two seconds in longitude to make them agree with observation. But this correction, instead of continuing to increase as all analogy would have led us to anticipate, suddenly began to diminish, so that since 1862 the moon seems to have been falling behind the tables at the rate of a second a year. This is shown by the following exhibit of the corrections to Hansen's mean longitude, or right-ascension, deduced from the meridian observations of the two observatories.

Year.	Correction given by		Mean.	Corr. mean.
	Greenwich.	Washington.		
	″	″	″	″
1850	+0·3	−1·3	0·0	+1·0
51	+1·5	+0·6	+1·3	+2·7
52	+0·9	....	+0·9	+2·4
56	+1·0	....	+1·0	+1·4
57	+1·5	....	+1·5	+1·4
58	+2·0	+1·5	+1·8	+1·3
62	+2·4	+2·4	+2·4	+0·9
63	+2·2	+1·2	+1·7	+0·5
64	+0·1	−1·0	−0·4	−1·2
65	−1·1	−2·4	−1·7	−2·1
66	−2·2	−2·5	−2·4	−2·4
67	−3·9	−4·1	−4·0	−3·6
68	−4·4	−4·5	−4·5	−3·6
69	....	−5·5	−5·5	−4·3

The corrections here given as those of Greenwich are, previous to 1859, derived from the comparison found in the Greenwich observations for 1859. From 1863 forward they are derived from a paper by Mr. Dunkin in the Monthly Notices of the Royal Astronomical Society for April, 1869. The work of only the four principal observers is therefore included in the comparison. The object of this comparison being not so much to determine the absolute correction to the epoch of the tables as to show the changes of this correction, it is better to reject the results of the observers whose labors were discontinuous. In the case of the Washington observations, such a selection could not be made: the results given are therefore an indiscriminate mean of all. The systematic personal differences are however found to be very small.

That these corrections are real will not, I conceive, be disputed. To suppose them due to errors of observation, would be to suppose that six or eight long practiced observers divided between the two hemispheres, all progressively changed their habits of observing in the same way, and to nearly the same amount, through a period of seven or eight years.

A portion of the observed discordance may arise from a small error in Hansen's value of the coëfficient depending on the ellipticity of the earth, which is more than a second greater than the values derived by previous investigators, either from theory or observation. The last column of the preceding table shows what the correction would be if Hansen's coëfficient were $1''\cdot5$ smaller than it is.

From all these comparisons it would appear that the problem of the inequalities of long period in the moon's mean motion is really no nearer such a solution as will agree with observation, than when it was left by La Place. By a partially empirical correction, Hansen has succeeded in securing a very good agreement during the period 1750–1860, but, if the results of the preceding examination are correct, this has been gained only by sacrificing the agreement for the century previous to 1750, and for the years following 1860. This failure to reconcile theory with observation must arise from one of two sources. Either:

(1) The concluded theory does not correctly represent the mean motion of the moon. Or:—

(2) The rotation of the earth on its axis is subject to inequalities of irregular character and long period.

The first hypothesis admits of two explanations. We may suppose either that the mean motion of the moon is subject to change from some other cause than the gravitation of the known bodies of the solar system, or that the effect of this gravitation is incorrectly calculated, and that theory and observation will be reconciled by a correct calculation.

There are difficulties in the way of accepting either of these explanations. In reference to the first it may be remarked that anomalies of mean motion cannot be accounted for by a deviation from the received law of gravitation inversely as the square of the distance, because the anomalies produced by such deviation would be regularly progressive, and would be most sensible in the secular motion of the moon's perigee. The comparison of the theoretical and observed values of this motion is, perhaps, the severest test to which the Newtonian law has yet been subjected. That the anomalies proceed from the attraction of unknown bodies passing through the system seems extremely improbable, since, if they were distant, they would affect the earth and planets more than the moon, while the closer passage of bodies could scarcely escape detection. Still, this explanation does not admit of being mathematically disproved. If we attribute the deviation to the impact of meteoric matter, we must suppose the moon to have encountered such matter in quantities nearly incredible.

These three causes exhaust those on which we can base the first explanation, unless we invalidate the third law of motion. For, by that law, matter moves only by the influence of other matter. Other matter can affect the motion of the moon only by impact and gravitation. The gravitation of known bodies. the gravitation of unknown bodies, and the impact of matter is therefore an exhaustive enumeration.

We pass now to the second explanation of the first hypothesis, namely, errors or omissions in the theoretical computation of the effect of gravitation. The wide difference between the conclusions of Hansen and Delaunay suggests the possibility that there may be inequalities still overlooked. We have however the assurance of Hansen that there are none, and we shall find it extremely difficult to introduce any periodic terms whatever which will represent the observed deviation of the moon from the tables during the past ten years, without discordance during the century previous, when the agreement of Hansen's tables with theory is believed to be quite close. It is however hardly worth while to dwell upon this explanation until we have a more rigorous theory of the inequalities of long period produced by gravitation.

Considering that the reconciliation of theory and observation is not very probable, the second hypothesis may become worthy of serious consideration. If we accept it we must admit that between the years 1860 and 1862 the rotation of the earth was so accelerated that our reckoning of time is already eight or ten seconds ahead of what it would have been had the day remained invariable. Such an acceleration could proceed only from a change in the arrangement of the matter of the earth. The possibility of this effect being produced by changes in the quantity of ice accumulated around the poles has, I believe, been pointed out by geologists. But the effect of this cause could scarcely be sensible. But, if we admit that the interior of the earth is a fluid, and also admit that general changes in the arrangement of this fluid are possible, we have all that is necessary to account for considerable changes in the rotation of the outer crust. That this fluid, admitting its existence, is not in a state of entire quiescence is rendered probable by the phenomena of volcanoes and earthquakes. If we suppose a large mass of it to move from the equatorial regions to a position nearer the axis, a mass from the latter position taking its place, the following effects will follow:—

1. A diminution in the angular velocity of the surface of the fluid, accompanied by a corresponding increase in the velocity of the axial portion. The velocity of the outer crust will then be gradually retarded by friction.

2. The gradual transmission of the increased rotation of the central mass to the surface by friction and viscosity. The motion of the crust will then be gradually accelerated. The velocity of rotation finally attained will be greater or less than the original velocity, according as the radius of gyration of the fluid mass is diminished or increased by the change in the arrangement of the fluid.

I conclude, from this discussion, that we have reason to suspect that the motion of rotation of the crust of the earth is subject to inequalities of an irregular character, which, in the present state of science, can be detected only by observations of the moon. This suspicion can be neither confirmed nor removed until we have more positive knowledge than we now have of the possible inequalities which may be produced in the mean motion of the moon by the action of gravitation.

The operation of calculating these inequalities, though complicated and difficult, is certainly within the powers of analysis. When it is completely and thoroughly done, we may ascertain whether the result can be made to represent observations. If so, well; the length of the day is not variable, and the future positions of the moon can be safely predicted. If not, it will follow either that the motion of the moon is affected by other causes than the gravitation of the known bodies of the solar system, or the day is irregularly variable.

By the end of the present century, if not sooner, we shall have an independent test of the latter hypothesis, in the agreement of the observed and theoretical times of the transits of Mercury and Venus. If the hypothesis is a true one, the irregularities may range over half a minute of time in the course of a century, and this quantity might be detected even by meridian observations of the planets in question.

ART. XX.—*Researches in Electro-Magnetism*; by ALFRED M. MAYER, Ph.D.

THE refined experiments of Weber, Tyndall and Knoblauch having fully established the fact of the reversed polarity of a bar of bismuth, when subjected to the magnetizing influence of a helix, the attention of natural philosophers is naturally directed to the necessity of subjecting Ampère's theory of magnetism to as severe deductive tests as can be applied. Thus, from new experiments and lines of research suggested by theoretic deduction, we may hope for such addition to our knowledge as will evolve a theory which will embrace both magnetic and diamagnetic phenomena, as completely as Ampère's beautiful generalization contains the explanation of the magnetization of iron.

Among other experiments, thus suggested, was the following: Ascertain the relative forces of two electro-magnetic cores; one composed of *insulated* wires, the other of the same number of similar wires *uninsulated.*

Theory indicates that the insulated bundle will be found the weaker; for, in this case, we have not only the reaction of the molecular currents, but also, the reaction of the infinitely larger currents flowing around each insulated wire. Also, admitting the truth of the molecular hypothesis, we have in the uninsulated wire-core the interaction of exceedingly small currents separated by distances, great when compared to their size; while in the insulated wire-core we have, in addition, very large currents reacting at distances very small when compared to their diameters.

It was the attempt to solve this problem which led to the invention of the experimental method described in this paper; for, it will appear further on, that it could not have been attacked by methods heretofore used. The satisfactory solution of this question and the ascertained delicacy and precision of the apparatus encouraged me to make other determinations, which I here present; not as a finished and neatly rounded piece of work, but as showing what may be expected from a more complete discussion of the method used, and from that perfect experimental control of the apparatus which a more extended experience will give.

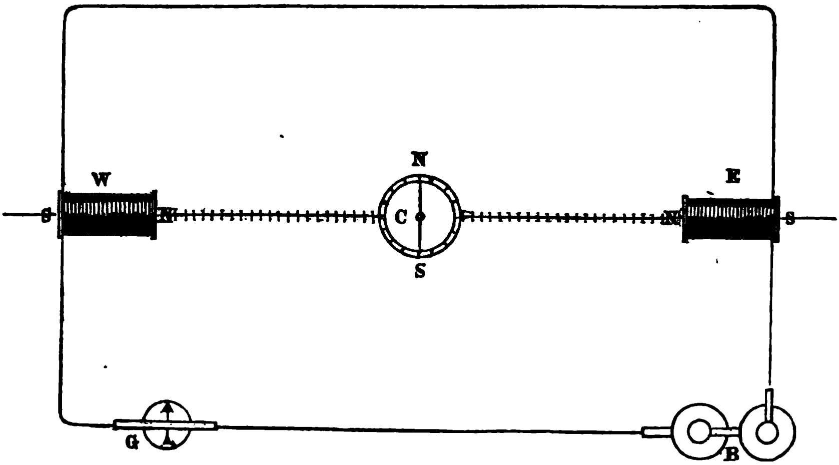
In my first experiments I adopted the plan of Müller (Poggendorff's Annalen, Bd. LXXIX and LXXXII) and tried the forces of the different cores, magnetized in one and the same helix, by their action on a distant magnetic needle; keeping the current, as far as possible, of the same intensity during the two comparisons. This method I found could not serve the purpose of measuring forces differing but slightly in intensity; and the impossibility of obtaining a current so constant as not to produce continual motion and vibration of the needle caused me to devise the following method, which I have found from experience to be both sensitive and precise.

Apparatus for the comparative measures of electro-magnetic forces.—On a table 10 ft. long was drawn a center line and divided into fractions of an inch. This line was then accurately placed at right angles to the magnetic meridian. Two helices, which I designate as E and W, were placed 8 ft. apart with their axes in the same vertical plane as the above line. A surveyor's compass, with a sensitive needle 5·85 in. long was so arranged that the point of suspension of the needle could be moved between the helices in the line of their axes. The same battery current passed through both helices, and in such direction that the N. pole of each was facing the needle; by reversing the current, the S. poles could be opposed to each other.

Both helices were composed of 10 layers of ·1 inch "extra covered" copper wire, wrapped on copper spools of 8$\frac{5}{8}$ inches long, 1·82 inches diam., and having flanges at the ends 1·25 inches high. These spools, with their flanges, were split in the direction of their length by an opening of $\frac{1}{16}$ inch. Each layer of coils was saturated with a thick solution of shellac in alcohol and covered with thick paper, coated with shellac, before the succeeding layer was wrapped.

Helix E contains 557·5 feet of wire in 696 turns, and on account of its better insulation and greater number of turns is superior in strength to helix W. Helix W contains 551 feet of wire in 688 turns.

1.



The accompanying diagram shows the arrangement of the apparatus. Helix E to the east of the compass; helix W to the west; at C the compass; and at G the tangent galvanometer sufficiently removed from E and W, not to be affected by the magnetized cores. The Bunsen battery is shown at B.

Each end of the compass needle is thus subjected to the powerful action of two opposing forces and a magnetic couple is thus formed whose equilibrium, shown by the needle, is readily disturbed by a change in the relative forces of the cores or by a change in the distance of the needle relatively to the two helices.

It was found that when the needle was placed at such a position on the line between the helices that it stood at 0° when the circuit was open or closed, and a current passed from 12 cells of a Gaiffe bichromate battery, which was rapidly decreasing in electro-motive force, the needle remained at 0° for over 20 minutes without change or tremble.

Now if we can find the law which shows the variation of the intensity of action of the electro-magnet on the needle with a change of its distance from the magnetized core, we will have a system of measurement for the electro-magnetic forces which will exceed in accuracy and in delicacy, though be similar in arrangement, to the best photometric processes.

The advantages of this method, as far as my knowledge of it extends, is that you can thus subject the needle to two opposing actions of great intensity and thus any minute difference in the causes which alter their relative intensities will, on account of the nearness of the needle to the helices, give a deflection which would be inappreciable in the methods heretofore used; and above all, the same current traversing both helices will, even if inconstant and fitful, affect both proportionately and the needle, as seen above, will preserve a fixed position at 0° under these circumstances.

The law of variation of the intensity of the force with a change of distance.—To find the law of variation of the force with the distance from the pole of the core (which as is well known exists in the plane of the end-coils of the helix) the following experiments were made.

A core 10 in. long and 1·64 in. in diameter composed of 400 soft well annealed iron wires, was placed in helix E and the plane of the ends of the wires facing the needle were brought into the plane of the end coils of the helix. The center of the compass needle was placed at distances of 4, 5, 6, 7 and 8 ft. from the end of the core; the needle brought to 0° at each of these positions by the action of terrestrial magnetism and the current passed from seven new, freshly amalgamated Bunsen-cells; as soon as the needle had come to rest the angle of deflection was read and at the same time the tangent galvanometer was noted.

The following table gives the results, each number being the mean of four experiments.

Column (1) gives the distance of the center of needle from core; (2) the readings of the tangent galvanometer; (3) the deflection of the compass needle for the distances given in column (1); the nat. tangs. of the angles of (3); (5) the products of the nat. tans. of the angles of deflection by the cubes of the distances in (1); these products being obtained by multiplying the nat. tan. of each angle of deflection by the corresponding distance from the core raised to the cube, should, if the law followed the reciprocals of the cubes, be equal; column (6) gives the products of the nat. tangs. of the needle deflections into their corresponding distances from the core raised to the 2·7404 power; (7) contains the differences of each product of (6) from ·788336 the nat. tan. of 38° 15′. If the experiments had been more extended these differences would have been smaller; as it is, they are not great, and greater approximation cannot be obtained with three decimals in the exponent of the power, and this degree of accuracy is all the determinations will allow. The sum of the + departures equals ·008857, that of the − departures equals ·008651, their difference is + ·000206.

(1) Dist. from Core.	(2) Tan. Gal.	(3) Deflection.	(4) Nat. Tan. Defl.	(5) $\frac{1}{d^3}$	(6) $\frac{1}{d^{2\cdot7404}}$	(7) Dif.
4 ft. or 1	51° 45′	38° 15′	·788336	·788336	·788336	·000000
5 " " 1·25	51° 45′	23°	·424475	·829042	·782392	·005944
6 " " 1·5	51° 30′	14° 30′	·258618	·872835	·785629	·002707
7 " " 1·75	51° 45′	9° 45′	·171831	·917888	·796385	·008049
8 " " 2	51° 30′	6° 44′	·118063	·944504	·789144	·000808

We can therefore assume that *the intensity of the force* (for this apparatus) *varies inversely as the* 2·7404 *power* (*or as* $d^{\frac{1}{2\cdot7404}}$) *of the distance from the core.*

Experiments on the comparative strengths of cores formed of insulated and uninsulated wires.—The wire used in these experiments was of the best soft iron and of two diameters, $\frac{1}{10}$ and $\frac{1}{20}$ inch. The wire was cut into lengths of 10 in. and then placed in iron tubes, (to keep them from the action of the air), and heated to bright redness; the tubes were then covered thickly with lime and allowed to cool slowly. This process was twice repeated on all the wire and iron tubes and cylinders used in this investigation, and they were found after magnetization to have very little residual magnetism. The caution however was taken always to present the same end of a core toward the needle; thus all the cores were placed in the same circumstances. Three cores were made of each size wire. Each core of the $\frac{1}{10}$ wire contained 100 pieces, while each core of the $\frac{1}{20}$ wire consisted of 400 wires. The wires of one core of each size wire, (after having been tested, as in Ex. 7, that they were equally affected in the same circumstances of magnetization), were insulated by dipping them separately into parafine, melted in a tube about 12 in. long, and were thus uniformly coated with a firm layer of that substance. They were then closely tied together into bundles by means of silken thread. The helices, in all the experiments on the cores, were placed 8 feet apart.

To abbreviate the descriptions of the experiments we will always place the E helix east of the compass and the W helix west, and to indicate the deflection of the needle we will use such expressions as "S. 17° W." which means, S. *end* of *needle* deflected 17° W. or toward helix W. In the case of N. poles facing the compass the above expression would show that the core of helix W was stronger than that of helix E. If the current was reversed it would indicate that the E helix was the stronger. When not otherwise stated it will be understood that the N. poles of the helices face the needle, and therefore the expression given of the deflection will always show which of the cores has the greater action on the needle. To designate the different wire-cores we will use "100 or 400 UC" or "100 or 400 I C," which respectively mean, *the uninsulated core* of 100 or 400 wires and *the insulated core* of 100 or 400 wires. T. G. stands for tangent galvanometer.

Experiment 1. Helices alone, without cores. Needle placed midway between helices and brought to 0°. Current passed from 10 Bunsen cells. Needle S. 4° E. T. G. 58°.

Ex. 2. Reversed current S. 4° W. T. G. 58°.

Ex. 3. Moved needle toward W. helix so that it stood at 0° when circuit was open or closed. Distance from E helix = 52·77 in. T. G. 58. Expts. 1, 2 and 3 show that E helix is the stronger.

Ex. 4. Three cores of 400 wires each, one of insulated wires, were compressed to exactly the same diameter of 1·64 ins. An uninsulated core was placed in each helix, and the needle brought to 0°, midway between the helices. Current passed S. 27° E. T. G. 50°.

Ex. 5. Current reversed S. 27° W. T. G. 50°.

Ex. 6. Other circumstances remaining as in (4) moved needle toward W helix until it stood at 0° when circuit was open or closed. Needle distant from E helix 55·6 in. T. G. 46°. Expt. repeated many times, with current direct and reversed, with the same result.

Ex. 7. Interchanged the cores in the helices. Current passed. Needle still at 0°. T. G. 48°. Current reversed. Needle at 0°. This shows that the cores were equally affected in the same circumstances.

Ex. 8. Replaced E core by the *insulated* core of 400 parafined wires. Needle having remained in same position as in (6) and (7). Current passed. S. 55′ W. T. G. 46°.

Ex. 9. Current reversed S. 55′ E. T. G. 46°.

Expts. 8 and 9 repeated several times with same results, showing that the insulated core was the weaker.

Ex. 10. Moved needle toward E helix, which contained the 400 insulated wires, until it stood at 0° when circuit was open or closed. In this position needle was 55·4 in. from E helix.

The 100 wire-cores substituted for the 400 wire-cores gave similar results only differing in degree.

It is very important to remark that errors may be introduced into such determinations as the above, by not taking care to have the various cores which we compare of *exactly the same diameter.* To show the effect of a change of diameter in the same bundle of wires the following experiment was made.

Ex. 11. The two cores of 400 uninsulated wires of 1·64 in. diameter were placed in the helices and needle brought so that it stood at 0° when circuit was open or closed. The E core was now so firmly compressed that its diameter was reduced to 1·48, and replaced in E helix, otherwise the circumstances were the same. Current passed. S. 1° 15′ W., showing a considerable diminution in the magnetic force of the compressed core; and also, beautifully exhibiting the *surface* action of magnetic force.

It will be observed, in the above comparative experiment, that the two uninsulated wire-cores were of exactly the same strength when equally magnetized, (Ex. 7.) and that one of them remained in the W helix as *unit of comparison* to the uninsulated and insulated cores placed in the E helix. This method is necessary when the magnetizing effect of the helices are not the same. The uninsulated core which remains in the W helix stands in the place of the "unit-candle" in Bunsen's method of photometry, and the needle which is moved to such a position between the helix that it remains at 0° when the circuit is open or closed is the equivalent of the paper screen with its central translucent circle. But if it should happen that when the needle is placed midway between two similar helices that it remains at 0° when the circuit is open or closed, then two cores would answer the purpose.

Calculation of the relative forces of the uninsulated and insulated cores.—The data for the calculation are given in Experiments 6 and 10.

$$\text{W.UC} : \text{E.UC} :: 40{\cdot}4^{2{\cdot}7404} : 55{\cdot}6^{2{\cdot}7404} : 1 : 2{\cdot}39925.$$
$$\text{W.UC} : \text{E.IC} :: 40{\cdot}6^{2{\cdot}7404} : 55{\cdot}4^{2{\cdot}7404} : 1 : 2{\cdot}34374.$$

Hence *Uninsulated Core*: *Insulated Core*:: 1·: 0·9768.

The insulated core is therefore ·02318 weaker than the uninsulated.

In Expts. 8 and 9 it was seen that the above difference of ·02318 in magnetic force caused a deflection of 55′ in the compass needle, and as 3′ or even less of deflection can be accurately read it follows that a difference of $\frac{1}{1000}$th in the magnetic force of the two cores can be detected and measured by this method. This example will convey an idea of the delicacy and accuracy of this plan of measurement, which no doubt can be increased by the careful selection of the proper force of current, distance of helices and other related functions of the apparatus.

It thus appears that this result sides with the theory of Ampère, and also, to my mind, points to that postulate of the atomic hpyothesis which states that the atoms of bodies are separated from each other by distances which are great when compared to the size of these atoms.

Attempts to determine what thickness of tube in fraction of its diameter, is equal to a solid cylinder of the same diameter.—According to the theory of Ampère, the magnetization of a bar of steel, or of an iron core subjected to a helical current of electricity, is due to the surface currents, which are the resultants of the interaction of all the molecular currents in the interior of the bar. It is therefore exceedingly interesting in a theoretic point of view to determine accurately what thickness of tube is equivalent to a solid cylinder of the same length and diameter. The experiments of Barlow, Harris, de Haldat, and of Du Moncel have shown the truth of this deduction.

There is evidently a certain thickness required to develope an equivalent action, but a certain portion of the interior of an electro-magnetic core can be removed without diminishing its force *when in a condition of "saturation."* This determination will therefore inform us, (accepting the hypothetical theory of Ampère), what central currents are prevented by their interaction from affecting the resultant surface currents of the bar; and it appears to me that we should express the result in terms of the central cylinder which can be removed and not of a certain thickness of tube which remains.

Out of well annealed soft iron a series of tubes, 10 in. long, were turned so that they fitted closely into each other; into the innermost tube slid a solid iron core of ·88 in. diameter. The diameter of the outer tube was 1·68 in. With this construction of core, tubes of various thicknesses could be rapidly produced and compared in effect. Two such cores were constructed.

Ex. 12. 400 uninsulated wire-core in W helix; in E helix was placed the combination of tubes and core. Needle brought to such a position that it stood at 0° when circuit was open or closed. T. G. 46°.

Ex. 13. Removed inner core and tubes, and drilled out the innermost remaining tube until the thickness of the combination tube was $\frac{1}{6}$ of its diameter. Current passed. Needle remained at 0°. On further decreasing the thickness the S. end of the needle was deflected toward the W helix. T. G. 45°.

It thus appears that a tube of this size having a thickness of $\frac{1}{8}$ of its diameter is, when magnetized with this strength of current (8 cells, T. G. 46°) equal in effect to a solid cylinder of the same diameter.

Ex. 14. Another tube was constructed having a diameter of 1·3 in. and a thickness of $\frac{1}{8}$ the diam. Into this tube slid a solid core. Placing the uninsulated wire-core of 400 in W. helix and tube with inner core in E. helix, the needle was placed so that it stood at 0° when circuit was open or closed. T. G. 46°.

Ex. 15. Withdrew inner core. Current passed. S. 40′ W. T. G. 46°. Thus a tube of the above diameter, having a thickness of $\frac{1}{8}$ of its diameter is not equal to a similar solid cylinder. This subject will therefore have to be the object of further investigation.

Thinking that in the case where a current did not saturate the above tube it would equal the solid cylinder, the following experiments were made.

Ex. 16. The tube used in (14) was placed with inner core in E. helix, and a similar tube and core in W. helix, and the needle placed so that it stood at 0° when circuit was open or closed. A current from 4 cells was passed. T. G. 32°.

Ex. 17. Solid core removed from tube in helix E. Needle remains at 0°. Hence for this strength of current (T. G. 32°) the tube is equal to an equivalent solid cylinder.

Ex. 18. Other things remaining as in (17) a current from 5 cells, T. G. 37°, was passed through helices. Needle S. 2′ W.

Ex. 19. Current passed from 6 cells. T. G. 40°. Needle S. 15′ W.

Ex. 20. Current passed from 7 cells. T. G. 42°. Needle S. 20′ W.

Ex. 21. Current passed from 8 cells. T. G. 44°. Needle S. 40′ W. It is clearly seen from above experiments that with a current of T. G. 32° the tube is equal to the similar solid cylinder, but on gradually increasing the current up to T. G. 44° that the tube falls more and more below the strength of the cylinder, and the needle is again deflected S. 40′ W. toward the cylinder with that current. Therefore in determining what portion of the interior of a cylinder can be removed so that what remains shall equal the solid cylinder we should see that the tube is "saturated," otherwise the comparison does not solve the problem. Whether this condition was fulfilled in Expt. 13 was not ascertained.

In Daguin's *Traité de Physique*, vol. iii, p. 615, Paris, 1861, we read: "M. Du Moncel finds that it is necessary, in order that the magnetization [of a tube] may remain the same as a solid cylinder, that the thickness should be at least $\frac{1}{4}$ that of the radius of the cylinder." I have not seen Du Moncel's research on this subject and do not know the method he adopted in comparing the magnetization, but I think that his estimated fraction of $\frac{1}{8}$ of the diameter is too low, as Ex. 13 clearly shows. The cause of this error, I imagine, resides in the fact that he used a current not sufficient to saturate the tube and cylinder, and thus his determination would be explained by Expts. 15 to 21.

This subject is worthy of careful research, and its theoretical bearings on the subject of magnetism are very interesting. My experiments thus far cause me to doubt if any *constant* fraction of the diameter will be found, but that this fraction will depend upon the diameter of the tube; being larger as the tube is smaller.

Experiments on a Tube slit longitudinally.—To determine the influence, if any, of destroying the continuity of the exterior surface of a tube by a longitudinal slit, I obtained a tube of well annealed iron, 1·68 inch in exterior diameter, ·16 in. thick and 10 ins. long, and slit it in its whole length by an opening $\frac{1}{20}$ inch wide. This opening could be closed at pleasure by placing in it a tightly fitting piece of iron 10 in. long.

Ex. 22. The above tube, with the slit closed, was placed in the E. helix and the 400 U. C. in the W. helix, and needle brought to such a position between them that it stood at 0° when circuit was open or closed. Eight cells of Bunsen. T. G. 46°.

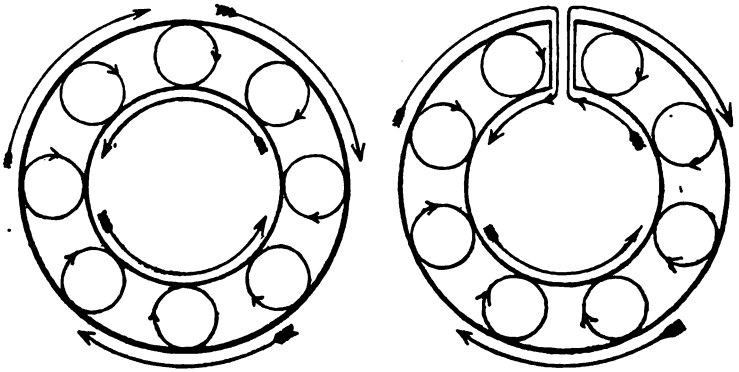
Ex. 23. Slit opened by removing the piece of iron. The needle remains at 0°. T. G. 46°.

Expts. 22 and 23 repeated several times with the same result.

In the accompanying diagrams is shown, according to the theory of Ampère, the direction of the circulation of the exterior and interior surface-currents. It is seen that in both cases the interior surface current circulates in an opposite direction to the exterior current; and that when the tube is slit longitudinally the circulation is not cut off but *facilitated* by the joining of the exterior and interior surfaces. It is therefore natural to suppose that on account of this facility afforded to the circulation that a *quicker* magnetization and demagnetization will occur in the split than in the closed tube; but no difference is found in the magnetic force of the two tubes, for the loss in surface by the slit seems made up in the greater facility of circulation.

I would therefore suggest that the cores of the electro-magnets of astronomical chronographs and of telegraph instruments be of soft well annealed Norway iron, made into tubes having a thickness of $\frac{1}{3}$ of their diameters, and slit longitudinally by a narrow opening.

2.



On the magnetic condition of the interior surface of a tube.—Referring to fig. 2 it will be seen that the Ampèrean currents on the interior surface of a tube flow in a direction contrary to those on the exterior. The magnetic condition of this surface was therefore an extremely interesting point for investigation; it has been the subject of over two hundred experiments, and is by far the most difficult research mentioned in this paper.

In my first experiments, steel rods and iron cores were introduced into soft iron tubes which were then placed in helix E. The current was then passed and the tube with enclosed core very slowly withdrawn. The steel or iron core was then removed from the tube and tested by placing it to the W. of the center of a compass needle. The magnetic force of the core was then compared with what it had before it was introduced into the tube. This method was thoroughly tested by withdrawing tube and core both before and after contact was broken in the helix, but no decisive conclusions could be drawn because the effect of the inductive action which took place when the core was withdrawn from the tube changed the magnetic relations which existed while the core was in the tube.

The following method was then devised. Two tubes of very soft iron were constructed, having both an internal diameter of ·84 inch, and one an external diam. of 1·03 inch, the other of 1·31 inch. Both tubes were 9 in. long. One hundred and twenty well annealed iron-wires a little less than 9 in. long were tightly bound together by silken cord and then wrapped in six thicknesses of writing paper, so that the wire core fitted neatly the interior of the tubes.

The wire core was now deprived as far as possible of magnetism by passing a weak magnet over it, and could thus be so reduced that it caused a deflection of only 1° when placed 7·5 in. W. of the center of a surveyor's compass needle. By now inverting the core in the line of the dipping-needle this small residual magnetism could be reduced to 10′ or 15′ deflection, and in some experiments even a reversed magnetization was thus produced. The core was now handled very carefully to prevent all vibrations or blows, and placed in an E. and W. horizontal line.

The tube was placed in helix E and the current passed. It was then *very slowly and steadily* withdrawn, so as gradually to let down its magnetism, the tube removed several feet from the helix and the current broken. This operation was repeated until the tube had attained as high a degree of magnetization as could be given to it with the same constant current in the helix, and a further repetition of the process caused no increased deflection in the needle. The tube was then carefully placed in an E. and W. horizontal line and the core slowly introduced, and so that the S. pole of the core was in the S. end of the tube. It is important in all these manipulations to keep the core and tube in a horizontal plane, for if they are inclined to the horizon, during the experiment, the magnetic condition is changed and the operations have to begin anew. The degree of susceptibility of very soft iron to a change of inclination can only be appreciated by those who make the trial.

The tube with enclosed wire-core was now introduced into the helix and the current passed; then withdrawn, and this repeated (as in the above process on tubes alone), until the needle-deflection showed no increase of magnetic force. This deflection was then compared with that previously produced by the tube acting alone.

Now if the interior electric currents really flow as Ampère's theory states, then when the core is in the tube and the maximum magnetization reached, the combined effect of tube and enclosed core on the needle should be less that given by the tube alone without the central core; for, in the former case the core has a magnetism given it the *reverse* of the tube; i. e., supposing marked end of tube S and marked end of introduced core S, then, after magnetization in the helix, the marked end of core is N and will therefore neutralize a portion of the tube's action on the needle. Now this was found to be *invariably* the case in all the experiments made; and how can it otherwise be explained. It is known that the combined force of two magnets with like poles placed together is less than the sum of the forces of the separate bars separately measured; but the force of the two together is always greater than that of one alone. But we here have a tube and enclosed insulated core with like poles together giving, after magnetization (in the same conditions *exactly* as that given to the tube alone) a less effect than that given by the tube alone. Therefore these facts show, to my mind, that the deduction from Ampère's theory is verified by experiment; and that we have succeeded in giving to a core in the interior of a tube a polarity the reverse of that given to the tube itself.

We here present the following experiments, which are typical of all made; the others only differing in quantity with a change of distance from the needle and a change in the intensity of the current in the helix.

The marked ends of tubes were S. poles, and S. 10° E. means that when tube or core or both were placed W. of the center of the compass-needle that the S. *end of needle* was deflected 10° E. If the same end is deflected W. then the marked end is a N. pole.

Ex. 24. Experiments with tube of 1·03 in. diam. is given under column A; those with tube of 1·31 in. diam. under column B. Tangent galvanometer in all the experiments was at 30°. Tube and core placed 7½ inches W. of needle.

	A	B
Marked end of tube placed in S. pole of helix and withdrawn (as descibed above). Repeated 6 times	S. 17° 18′ E.	S. 21° 40′ E.
Introduced tube again and withdrew	“ 17 30 “	“ 21 40 “
“ “ “ “ “	“ 17 30 “	“ 21 40 “
Core placed W. of needle	“ 0 33 “	“ 0 18 “
Core slowly and steadily introduced into tube so that S. end of core was in S. end of tube; then tube and core introduced into helix E. and withdrawn 6 times	“ 16 2 “	“ 20 18 “
Introduced and withdrew tube and core again	“ 16 13 “	“ 20 18 “
“ “ “ “ “ “ “	“ 16 15 “	“ 20 18 “
“ “ “ “ “ “ “	“ 16 15 “	“ 20 18 “
While tube remained in position W. of compass the core was slowly withdrawn from the tube. Tube now alone deflected	“ 17 40 “	“ 21 38 “
Core now alone deflected	“ 0 50 “	“ 0 10 W.

Referring to Expts. A and B it is seen that when the tubes were magnetized alone as fully as can be by the current (of T. G. 30°) through the helix, they deflect the needle in Exp. A 1° 15′ more and in Exp. B 1° 22′ more than when the tubes with enclosed cores acted together on the needle after having been magnetized in exactly the same circumstances; also, that when the tubes, remaining undisturbed in their position, W. of the needle, had the cores withdrawn, the needle was deflected 1° 25′ more in Exp. A and 1° 20′ more in Exp. B than when the tubes with enclosed cores both acted upon the needle.

In the break-circuit experiments the effects are more complicated by reason of the instantaneous and intense extra-current in the helix which causes a sudden inductive action on the tube and core, and thus other actions are introduced which are foreign to the object of the investigation. I have therefore only given examples of the make-circuit experiments, though the others showed even a greater difference between the action of the tube alone and the tube and core together; this difference amounting in some experiments to 5°.

After the results of the above experiments, it occurred to me to investigate the action of *a helix introduced into the tube*, and to ascertain if a magnetism could be given to the tube the reverse of that which is induced when the helix surrounds it. The former experiments show that the tube can give a reversed magnetism to an enclosed core, and we will now see that a helix can effect a reverse magnetism on a surrounding tube.

The first Expts. were made with opposing helices, one being helix E with the 400 wire core, the other a tube of soft iron 9 in. long, 1·3 in. external diam. and ·12 thick, around which was wrapped a helix of one layer of 148 turns of $\frac{1}{20}$ inch wire, and in its interior fitted a helix also of one layer 148 turns of the same wire wrapped upon a cylinder of wood. This last I will call the inner helix, the other the outer helix.

Ex. 25. The inner helix was placed in the circuit (near the battery, far removed from the compass) and the compass was so placed that its needle stood at 0° when circuit was open or closed. The inner helix was now placed inside the tube and the current passed through helix E and the outside helix; and also through the inside helix in a direction the same as the current in the outer helix; and the needle was noted. The current was now reversed in the inner helix and needle again noted; but as the opposing helices were 8 ft. apart, and the current not strong, no effect (after allowance made for direct action of inner helix on needle) was obtained from the action of the inner current.

Ex. 26. I then devised the following experiment. The opposing helix E with 400 wire-core was removed and the compass-needle brought to 0° by the action of the earth's magnetism. The iron tube was then placed 1 foot W. of center of needle with its S. pole toward the compass. The deflection produced by its magnetism was S. 4° 40′ E. The inner helix was now placed in the position W. of the compass previously occupied by the tube and the current passed so that its S. pole was opposed to the needle; the deflection was S. 3° 13′ E. I now placed the helix in the tube and both 1 ft. W. of needle and passed current so that S. end of helix was in S. end of tubes. The deflection produced was S. 4° 50′ E.

From above it is seen that when tube and inner helix act together on the needle the deflection is only 10′ greater than when the tube acts alone, although the helix acting alone causes a deflection of 3° 13′. The neutralization of such a quantity of magnetism can only be accounted for by the fact that the greater part of the magnetism of the tube was reversed by the action of the heliacal current on the inner surface of the tube.

Ex. 27. To obtain directly a reversed magnetism by the action of the *outside of the helix* the following experiments were made. Several wires 9 in. long and $\frac{1}{16}$ inch diam. were heated in a cupel furnace to bright redness and slowly cooled; one end of each wire was then marked. A wire was then taken and placed with its marked end 2·75 in. W. of the center of the compass-needle and the deflection produced was S. 18′ E., showing that the marked end was a feeble S. pole. A current was now passed through the inner helix and the wire laid upon it so that its marked or S. end was upon the S. pole of the helix, after remaining here for a few moments it was conveyed to the compass and placed, as before, with its marked end 2·75 in. W. of the center of the needle. The deflection was now S. 28′ W., showing that the magnetism of the wire had been reversed and that a wire placed on the *outside* of a helix has given to it a magnetism the reverse of that given when it is placed *inside.*

Ex. 28. Twelve wires were now placed with their marked ends 3·25 in. W. of the needle, and the deflection being S. 8′ E. showed that their marked ends had a slight S. magnetism. They were now removed and placed with their marked ends on the N. pole of the helix at equal distances from each other around the helix, through which a current was passing; they were then removed from the helix before the current was broken and again placed in their former position 3·25 W. of the compass-needle. The deflection was now S. 55′ E., showing that their marked ends were of south magnetism. They were again placed on the helix with their marked ends on its S. pole and removed, as before; they now deflected the needle S. 20′ W., showing that their marked ends were now N., their magnetism having been reversed.

Ex. 29. The inner helix alone was placed 1 foot W. of needle and the current passed so that its S. pole was opposed to the needle. Deflection S. 3° 30′ E. T. G. 20°. Reversed current. Deflection 3° 15′ W. By holding a bar of soft iron in the line of the dip and passing the wires over its end I succeeded in rendering them without action on the helix when put in the place of the helix in the above experiment. The twelve wires were then tied around the helix equidistant from each other and separated about ·2 inch. The current was then passed so that the S. pole of the helix was opposed to the needle. Deflection S. 2° 40′ E.. Reversed the current. Deflection S. 2° 30′ W. We therefore have in the 1st experiment 50′ less and in the 2d 45′ less action on the needle when the wires are around the helix than when they are away. These results correspond to the above experiments on the tube, the helix giving the wire a polarity the reverse of its own.

Beccaria, Coulomb and Faraday have, by their well known experiments, proved that frictional electricity when *at rest* only exists on or just within the outer surfaces of bodies, and Prof.

Joseph Henry, by many experiments, has shown that this is also true when the same species of electricity, of high tension, is *in motion.* The following beautiful experiment, so conclusive in its evidence, appears so little known, that I will here give it as stated by that philosopher. "A copper wire, of the size usually employed for ringing door bells, passed through the axis of an iron tube, or a piece of gas pipe, about three feet long. The middle of this wire was surrounded with silk, and coiled into a magnetizing spiral, into which a large sewing needle was inserted. The wire was supported in the middle of the tube by passing it through a cork at each end, covered with tin-foil, so as to form a good metallic connection between the copper and the iron. On the outside of the tube and opposite each other were placed two other magnetizing spirals, their ends soldered to the iron. When these two spirals were also furnished with needles, and a discharge from a Leyden jar sent through the apparatus, as if to pass along the wire, the needle inside of the iron tube was found to exhibit no signs of magnetism, while those on the outside presented strong polarity. This result conclusively shows that, notwithstanding the interior copper wire of this compound conductor was composed of a material which offered less resistance to the passage of the charge than the iron of which the outer portion was formed, yet when it arrived at the tin-foil covering of the cork, it diverged to the surface of the tube, and still further diverged into the iron wire forming the outer spirals. We must not conclude, however, from this experiment, that the electricity actually passes on the outside of the tube. On the contrary, we must infer from the following fact, that it passes just within the surface. If the iron be coated with a thin coating of sealing wax, the latter will not be disturbed when a moderate discharge is passed through it, though with a large discharge in proportion to the conducting power of the rod, the outward pressure may become so great as to throw off the stratum of sealing wax."

Barlow and Harris have made experiments which show that magnetism is also a surface action; and in Exp. 11 of this paper we saw that when the surface of a wire-core was diminished by compressing the bundle the magnetism diminished with it. To show that this diminution of force was not, in major part, owing to the increased repulsion produced between the bars when brought nearer together, the following experiments were made.

Ex. 30. About 200 $\frac{1}{30}$ inch wires were pressed together as tightly as could be by binding them in a bundle with silken cord, and the deflection they caused in the needle, when magnetized in the helix, was noted; they were now taken apart and bound as tightly as before around a wooden cylinder about 1 inch in diameter; and being magnetized again in the helix with the same strength of current, the bundle caused a far greater deflection in the needle than when it acted without the central wooden cylinder. I consider this experiment as very conclusive of the surface action of magnetism, for in the two measures we used one and the same mass of metal, subjected to exactly the same magnetizing influence, and only differing in the extent of exterior surfaces existing during the two experiments. That the increase of force with the surface was not owing to a change of distance of the wires from the interior surface of the helix is conclusively shown in the next section.

But there are differences to be made between these analogous phenomena of frictional electricity and of magnetism; in magnetism a *considerable thickness* of metal is required to develope this action at the surface; so that (Ex. 13) a tube must have a thickness of about $\frac{1}{6}$ of its diameter to equal a solid cylinder of the same length and diameter, both being, when compared, "saturated" with magnetism; also, it appears (Ex. 26) that a magnetic action can be effected on the interior surface of a tube, while no similar action can be obtained with frictional electricity.

Experiments to determine whether a change of positon of a bar in the interior of a helix causes a change in the intensity of its magnetization.—Theory indicates that no change in the degree of magnetization will follow a change of position of a bar in the interior of a helix, and the following experiments conclusively prove the truth of this deduction.

Ex. 31. In the interior of helix E resting on the bottom of the opening was placed a cylinder of soft iron ·83 inch diam. and 9 ins. long. Opposed to this helix was helix W, (in the same circuit) with the 400 wire-core. Needle was brought to such a position that it stood at 0° when the circuit was open or closed. T. G. 44½°.

Ex. 32. The axis of the iron cylinder was now made to coincide with the axis of the helix. Current passed. Needle still at 0°. T. G. 44½°. Thus showing that the change of position makes no difference in the intensity of the magnetization.

Experiments on the comparative magnetizing effects of a helix and of a combination of spirals, formed of similar wire, and containing the same number of turns, arranged in a length equal to that of the helix.—According to the theory of Ampère the currents which encircle a magnetized iron bar or a steel magnet are in planes at right angles to the axis of the bars; and it seemed to me interesting to determine with this sensitive apparatus what difference, if any, existed between the magnetizing effects of a helix whose turns were inclined in the successive layers alternately in opposite and equal angles with the axis, and the effects of a combination of spirals composed of an equal number of turns of wire as the helix and existing in the same length.

Fifty spirals, each composed of 14 ft. 8·06 ins. of $\frac{1}{30}$ inch wire in 20 turns, were made by a process to be described in a subsequent communication. These spirals were placed vertically in a frame at equal distances from each other so that they formed a cylinder 9 ins. in length, 3·9 ins. in exterior diameter and with a cylindrical axial opening of 1·68 ins. The turns of the spirals were carefully insulated from each other by saturating the covering of the wires with melted parafine.

A helix was constructed of the same wire, wrapped in 20 layers, each layer consisting of 50 turns. The wire in each layer was wrapped parallel with two lengths of twine so that the 50 turns in a layer occupied a length of exactly 9 ins. This gave a "pitch" to the turns of the helix of ·18 ins. and the innermost turns of the helix formed an angle of 1° 57′·25, and the outside turns formed an angle of 50′·5 with the axis of the helix, alternately to the right and to the left, as it was wrapped. Each layer of turns of wire and twine was carefully saturated with melted parafine of a high temperature, so that the copper was seen through the saturated covering after the parafine had solidified.

Ex. 33. The helix and combination of spirals were placed 8 feet apart and an uninsulated 400 wire-core placed in each. The compass was placed midway between them and the needle brought to 0°. The current was now passed so that the N. pole of the cores faced the compass. The S. end of the needle was slightly deflected toward the helix, showing that the core of this was somewhat stronger than that of the spirals.

The greater strength of the helix could not be attributed to the excess of wire it contained over the spirals, for this only amounted to about 2 inches; thinking that the intense inductive action of the spirals on each other might have some influence, the following experiments were made.

Ex. 34. Placed the needle between helix and spirals so that it stood at 0° when the circuit was open or closed. Then I introduced between the spirals 49 copper discs having central openings a little smaller than those of the spirals. This arrangement, as Prof. Joseph Henry has shown, so effectually cut of the mutual inductive action of the spirals that, on passing a current through them and breaking a mercury contact, the spark of the "extra-current" was (on account of the greater resistance of the spirals) less than when only the circuit of the battery wires was similarly broken. With the exception of the interposed copper discs things remained as in Exp. 33. Current passed. Needle remained at 0°. Showing that the inductive action had no influence on the intensity of the magnetizing effect. I am therefore of the opinion that the increased effect of the helix was due to superior insulation.

Ex. 31. The combination of 50 spirals was separated into two, each containing 25 spirals. Between the spirals of one combination were introduced the 25 spirals of the other, so that every alternate spiral belonged to the same combination. If the current is now passed through one of the combinations and the two terminal wires of the other combination joined, Faraday has shown, that the "extra-current" in the first combination is entirely given up to second. The spirals thus arranged, with the terminals of the interposed spirals separated, were placed opposite the helix E, and distant 8 feet, and the needle so placed between them that it stood at 0° when the circuit was open or closed. On connecting the terminals of the interposed spirals and passing the current, the needle remained at 0°. Thus conclusively showing that *in a combination of spirals or in a helix the inductive action of the wire on itself or of adjoining spirals or turns on each other has no effect on the power of their magnetization and therefore no effect on the intensity of the current passing through them.*

I do not remember ever having seen a solution of this question, and these experiments have given it under conditions of a very strong inductive action, and with a very delicate apparatus for detecting any effect which might have been produced. The result is one which has an important theoretic bearing on dynamical inductive action, but I reserve for another communication my views on that point.

In bringing this research to a conclusion I think I may safely say that these results and experiments have shown the delicacy and precision of this method of comparing and measuring the electro-magnetic forces; and at a future time I propose using it to solve the problems which relate to the variation of the intensities of cores with their diameters and with their surfaces, and to examine the varying magnetizing effects of helices of different lengths, diameters, and number of turns of wire, and traversed by currents of various intensities.

Some of the above experiments, which relate to the inverse polarity given to cores placed inside of magnetized tubes and wires placed outside of helices, are very suggestive as to an explanation of diamagnetism, but I reserve for the present the hypothetical notions which they have originated in reference to those phenomena.

South Bethlehem, Pa., June 2, 1870.

ART. XXI.—*Abstract of the Second Series of Professor Meissner's Researches upon Electrized Oxygen;*[*] by GEORGE F. BARKER.

IN the first series of Dr. Meissner's researches upon oxygen, published in 1863—an abstract of which, by Professor Johnson, appeared in this Journal, volume xxxvii, page 325, and volume xxxviii, page 18—he arrived at the remarkable conclusion, that oxygen, under the influence of electrical tension, was converted not only into ozone, but also into another modification which always appeared simultaneously, and which formed, when brought into contact with watery vapor, especially after the absorption of the ozone, a peculiar dense mist. This second modification of oxygen Meissner identified with Schönbein's antozone. So remarkable were these results, and so important their bearing, if true, not only upon our theories of ozone itself, but also upon the philosophy of chemistry, that Meissner desired to repeat his experiments, studying particularly the character of the antozone-mist and the effects of electrical tension upon the volume of the oxygen submitted to the discharge. The results of these experiments constitute the paper now referred to.

I.

Section I is devoted to "Electrized Oxygen." The experiment which is to be critically investigated is thus described: "Oxygen is submitted to the action of electricity in a Siemens's or von Babo's apparatus, is then passed into a receiver containing a concentrated solution of potassium iodid, in which the ozone is completely absorbed, and finally through water contained in a second receiver; the gas as it issues from the water, forms above it a thick white mist, which also appears in a less degree over the solution of potassium iodid, but which is denser, the less concentrated the solution and the more favorable the ozonizing conditions." To prove that this mist consists solely of electrized oxygen and water, Meissner proposes to show:—1st, that no other gas but oxygen is in any way concerned in the production of the phenomenon; particularly no nitrogen, chlorine, hydrogen, or carbonic acid. 2d, that for the production of the result, the presence of aqueous vapor in the electrizing tube is not necessary. And 3d, that the potassium iodid solution used for the absorption of the ozone, has nothing whatever to do with the appearance of the phenomenon, further than is implied in effecting the removal of the ozone from the current of electrized oxygen.

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[*] **Neue Untersuchungen über den elektrisirten Sauerstoff. Von Dr. G. Meissner. Mit zwei lithographirten Tafeln. Aus dem vierzehnten Bande der Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen. 4to, pp. 110. Göttingen, 1869.**

The apparatus used to establish the first point, consists of two pieces, the first of which is intended for the production of chemically pure oxygen, the second for the electrization of this oxygen, and its subsequent analysis. We have space here for only a general description of these marvellously accurate specimens of Geissler's glass-work, referring those specially interested to the original plates. The oxygen is evolved by electrolysis from acidulated water contained in a U tube, and then passes through a second U tube filled with bits of glass moistened with sulphuric acid, then through a straight tube filled with oxydized copper-turnings to which heat can be applied, then through a second U tube containing glass and sulphuric acid, and finally through a horizontal tube a foot in length, containing anhydrous phosphoric acid: being collected, after passing an ingeniously-constructed mercury-valve, in a receiver over mercury. All these tubes are joined, either by fusion or by the most carefully ground joints. By means of the heated copper oxyd, the hydrogen, which, by diffusion or mechanically, may be mixed with the oxygen,—as well as the ozone produced by the electrolysis, and the vapor of hydrogen peroxyd—if any there be—are destroyed. The entire apparatus is first washed out with several liters of pure oxygen chemically prepared, then with 15 liters of the electrolytic oxygen; after which the oxygen, being assumed pure, is collected. The second piece of apparatus was made in duplicate; one has a tube filled with sulphuric acid between the receiver and the electrizing tube; the other has a tube filled with anhydrous phosphoric acid in this position. Moreover, in the former, Siemens's tube, in the latter von Babo's, is used for the electrization. With these exceptions they are alike; a description of one suffices. The gas passes from the receiver through the tube filled with the anhydrous phosphoric acid, to free it from any traces of moisture it may have acquired in the receiver, then into the electrizing tube, thence into two small flask-shaped receivers, the first containing potassium-iodid solution, the other water—and then to the Jolly's mercury pump. Between the drying tube and the electrizing tube is a barometer tube by which the exhaustion may be regulated. After thoroughly washing out the entire apparatus with pure oxygen, it is thoroughly exhausted, the cock beyond the electrizing tube closed, and the one connected with the receiver gradually opened, the electrizing tube thus filled with oxygen, and this submitted to the action of the silent electric discharge.* The electrized oxygen now passes successively through the solution of potassium iodid and the water, upon the surface of which latter liquid the dense antozone-mist appears, increasing momentarily in density until the flask, two inches in diameter, becomes perfectly opake. This experiment, performed in precisely this way, was repeated twelve times, and always with the same result; Dr. Meissner believes therefore, that neither hydrogen nor nitrogen is in any way concerned in the production of this peculiar mist.

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* The electrizing tube of von Babo is described in Prof. Johnson's abstract above alluded to.

The second point, that the presence of aqueous vapor in the electrizing tube is not necessary to the result, was established by filling this tube with pure oxygen dried over anhydrous phosphoric acid, then electrizing it, and, after intermitting the current, passing it through the solutions. The result was the same as before.

Nitrogen having been shown to be without effect on the production of the mist, the especial apparatus for the electrolytic preparation of oxygen was given up, and this gas was obtained from potassium chlorate, and washed with a solution of potassium iodid. In the next experiment this oxygen passed from the gasometer through a wash-bottle filled with potassium hydrate solution, then through a U tube filled with calcium chlorid, then through another wash-bottle containing sulphuric acid, and finally over anhydrous phosphoric acid, to the electrizing tube, being now free from chlorine, carbonic acid, ammonia, and moisture, though containing perhaps a trace of nitrogen. On passing a stream of this electrized oxygen, first over phosphoric anhydrid, then into iodid of potassium solution, and afterward through water, the dense cloud appeared as before. Meissner believes therefore "that these experiments, not once only or a few times repeated, but performed very frequently, prove the following point: that the mist formed by de-ozonized electrized oxygen with aqueous vapor, appears when neither chlorine, nitrogen, ammonia, hydrogen, carbonic acid nor watery vapor is present in the tube where the electrizing occurs; and that the presence or aid of neither of these substances is necessary afterward for its formation. That in other words, the mist-phenomenon requires only dry electrized oxygen, the potassium iodid used for de-ozonization, and the vapor of water, for its production."

The third point, that the potassium iodid acts simply by absorbing the ozone, and thus setting the mist-forming oxygen free, Meissner proves by replacing this substance by a great variety of other bodies, differing widely in chemical properties and agreeing only in the property of absorbing ozone. Sodium pyrogallate,—which must be free from even a trace of gallic acid—potassium ferrocyanid, potassium manganate, the higher sulphids of potassium and sodium, barium sulphid—the hydrosulphuric gas being removed by passing the de-ozonized oxygen through a solution of cupric sulphate before entering the receiver of water—ammonium sulphid, hydrosulphuric acid, sulphurous acid—either free or as sodium sulphite—and ammonioferrous tartrate, were all used with good results, the ozone being entirely removed. It is not necessary however, to absolutely de-ozonize the oxygen, in order to produce the mist; partial absorption will produce it, though it is weaker in proportion to the amount of ozone remaining. By using potassio- or sodioferrous tartrate, instead of the ammonia salt, such a partial absorption is effected. The same is true of certain metals, when moistened with water; lead, zinc, copper, iron, tin, cadmium, antimony, aluminum, and thallium all giving the mist, though lead, zinc and aluminum are the best for this purpose. Meissner compares the mist rising from these metallic bars to the cloud which rises from moistened phosphorus. Even carbon, in the form of gas-carbon, and sometimes animal charcoal, effects this absorption. The noble metals gold and platinum remain completely unaltered in both moist and dry ozonized oxygen. Chemically pure silver in the dry gas remains unchanged except at single points which show a beautiful greenish play of colors, without a trace of black peroxyd, into which, in the presence of moisture, it is so rapidly converted. Mercury, perfectly dry and still, remains unaltered, though on the least agitation a film appears on its surface, by which it adheres strongly to the glass. On replacing the ozonized by common oxygen, the mercury becomes again mobile, a black powder—of mercurous oxyd probably—being left. If the mercury be moist, reddish mercuric oxyd results. With none of these latter metals, however, is any mist formed.

Though these experiments prove conclusively that no single substance can be yielded by such widely different de-ozonizing substances, to form the mist, yet may not each yield one, which, though unlike the others in all else, may have, in common with them, this mist-forming property? This hypothesis, Meissner considers disproved by the fact that the mist is chemically identical, whatever the absorbing agent. It may be agitated with, or passed through water, not only without losing, but often with actual increase of, its properties. It may be passed through dilute sulphuric acid or alkaline solutions unchanged. "In a word, the mist which appears in my experiments under the given conditions, is neither acid nor alkaline, consists of a body neither soluble nor insoluble in water, but is solely a mechanical or adhesive combination of oxygen and water, which, when washed and collected in a gas-holder, gradually disappears, fine fluid drops collecting upon the walls of the vessel which when examined are found to be pure water, containing possibly under certain conditions, a trace of hydrogen peroxyd." When passed through strong sulphuric acid, or over calcium chlorid or phosphoric anhydrid, the cloud disappears because of the removal of the water; but it reappears again, though weaker, when brought again in contact with water. The unwashed mist, indeed, contains impurities coming from the absorbent employed; as iodine and traces of iodic acid from the potassium iodid, ammonia, hydrosulphuric gas, and probably, also, traces of ammonium nitrate. But all de-ozonizing substances do not yield such impurities, and even these may be removed without affecting the mist.

Again, may not the mist be produced either by the action of the oxygen unaffected by the electricity, or by the action of the ozone which thus results? Oxygen itself, under no known conditions, exhibits this property; and as to ozone, the above experiments show that it may be completely removed by various absorbing solutions without interfering with the phenomenon. Indeed, the mist is the stronger, the more perfect the removal of the ozone from the electrized oxygen. So that, contrary to the opinion expressed in the first paper, not a trace of mist appears until the ozone has been, at least partially, removed. That this mist is not due to a compound of ozone with water, is established by the fact that contact of the electrized oxygen with water before de-ozonization is prejudicial to its subsequent production. Further, such a compound must hold the ozone with such force as to prevent such easily oxydized bodies as potassium iodid, pyrogallic acid, or alkaline sulphids, from withdrawing it, and yet be at the same time one of the most unstable of substances. And moreover, were there such a compound of ozone and water, the de-ozonized oxygen, after careful drying, would yield water when subjected to a high temperature; Meissner however, has failed entirely to detect any moisture under these circumstances. Finally, the only known compound of water and oxygen, hydrogen peroxyd, forms no such mist. If therefore, neither the ozone itself, nor any compound of it, is concerned in the phenomenon, there must be contained in the pure dry electrized oxygen, besides the unaltered oxygen and the ozone, some third body, a third condition or modification of oxygen, to which the result is to be ascribed.

Assuming now the existence of antozone, Meissner proceeds to study more minutely the action of absorbing agents, with special reference to this substance. He divides them into two classes, one of which removes ozone alone from the solution, the other absorbs both modifications. The first substance examined is pyrogallic acid itself, by which the ozone is completely removed, the solution becoming beautiful hyacinth-red in color; but, unlike the action of the alkaline pyrogallates, the free acid removes also the antozone, so that not a trace of mist appears when the gas is subsequently passed through water. On adding a few drops of sodium hydrate, the liquid becomes dark brown, and the mist at once appears. Meissner accounts for this result, by supposing that in the oxydation of the free acid both ozone and antozone are taken into combination in equal proportions, while in that of its alkaline salts, the ozone combines to a larger extent than the antozone. Another substance examined is sodium thio-sulphite (hypo-sulphite). When the electrized oxygen passes through a concentrated solution of this salt, both ozone and antozone disappear, not a trace of mist being formed. This result is especially noticeable, inasmuch as sodium sulphite removes only the ozone, and inasmuch as the product is, in both cases, sulphuric acid. Since to oxydize sulphurous acid, two equivalents of oxygen are required for every two of sulphur; and to oxydize thio-sulphurous acid four equivalents of oxygen are needed to two of sulphur; Meissner believes that the two of oxygen in the former case are ozone; while the two times two in the latter are not of equal value, one pair being ozone, the other antozone, atoms. So also by the use of arsenous acid as an absorbent, both modifications are removed from the electrized stream of oxygen, both when the acid is free as well as when its sodium salt is employed. By using a very dilute solution, or the ordinary saturated solution in small amount, the ozone may be imperfectly removed; and by passing the gas afterward through potassium iodid solution to remove the ozone entirely, the antozone which remains gives a feeble mist with water; thus showing here also, that ozone and antozone are absorbed by the arsenous acid in equivalent proportions. Here again Meissner believes that of the two atoms needed to make arsenic acid from arsenous, one is ozone, the other antozone. Mercurous nitrate, in concentrated solution, absorbs, under the conditions of the experiment, both ozone and antozone only partially, though equally; thus acting like arsenous acid. The partial absorption in these cases is to be distinguished from that effected by the metals or alkaline ferrous salts; in the former case both the oxygen modifications are equally absorbed, while in the latter the ozone is almost entirely removed, but the antozone is unaffected.

Of particular interest is the action of electrized oxygen upon potassium hydrate. When fragments of this substance slightly moist upon their surfaces are placed in a horizontal glass tube, and subjected to the current of electrized oxygen, they shortly become covered for a short distance from the end of the tube, with orange-yellow potassium peroxyd. But however long the experiment continues, the other pieces remain unchanged, and not a trace of either ozone or antozone issues from the tube. So soon as the electrical action ceases, and ordinary oxygen again enters the tube, the yellow crust disappears, to be again formed as often as the electrized oxygen passes over it. Since the peroxyd is at once decomposed by water, and since too, the maximum production is very soon reached, it would seem that the peroxyd is successively produced and decomposed in the experiment, causing in this way the disappearance of ozone and antozone and yielding common oxygen. Absolutely dry hydrate has no effect on the electrized oxygen. In aqueous solution, the formation of the yellow peroxyd takes place only in the narrow tube which delivers the gas; but as the antozone and the ozone are materially lessened, though never absolutely destroyed in any of Meissner's experiments—it is fair to infer an action similar to that in the tube. Sodium hydrate acts similarly though less energetically; the two modifications appear moreover to be unequally absorbed, more of the antozone disappearing than of the ozone. These results illustrate well the two classes of absorbing agents; in the one case complete absorption of the antozone takes place with that of the ozone, in the other a portion of the antozone remains. In the one case partial absorption of both is equal, in the other unequal. One of these classes requires for its oxydation more ozone than antozone, the other requires them in equal proportions; possibly there is a third class requiring more antozone than ozone.

An important practical application is made of these facts to the employment of potassium iodid as an absorbing solution. A neutral solution of this substance becomes alkaline when subjected to the action of ozone, and contains free potassium hydrate, which, acting as above, diminishes the antozone. By so arranging the apparatus that acid or alkali can be added at pleasure to the iodid solution, the mist is seen to be diminished on making it alkaline and increased when it is made acid. The neutral solution of potassium iodid is not therefore a suitable de-ozonizing agent, if it is desirable subsequently to produce the mist. It should be previously acidulated with hydrochloric or sulphuric acid, especially where the quantity of antozone is small. Though for very delicate investigations Meissner prefers an aqueous solution of iodine.

The same injurious action of free alkali appears with ferrous oxyd and pyrogallic acid solution. It is completely obviated by using ammonia in place of potassa or soda, ammonium pyrogallate and ammonio-ferrous tartrate being far preferable to the same compounds of the fixed alkalies. Even with the sulphids this is true; the strongest antozone-mist Meissner observed was obtained by using ammonium sulphid as de-ozonizer, this compound absorbing more ozone and less antozone than any other substance tried. When ammonium hydrate is submitted to a stream of electrized oxygen, the ozone is partially absorbed by it, and the antozone cloud rests on its surface. By passing the mist through dilute sulphuric acid, its ammonia may be removed; and then, on collecting it in an open vessel, the water which it deposits is found to contain nitric acid which comes *from the oxydation of the nitrogen of the air.* Solutions of ammonium carbonate act in the same way. The energy with which ammonium sulphids absorb ozone, is further shown by the fact that, like moist phosphorus, some of them can polarize common oxygen, absorbing the ozone and emitting a powerful antozone-mist. Under the name "Liquor fumans Boylii," such an ammonium sulphid has long been known. It fumes strongly, but only with oxygen; neither hydrogen nor nitrogen causing it. Every unsaturated ammonium sulphid solution, if it contain sufficient sulphur, is such a fuming liquor. By passing pure oxygen through it and then through water, a dense mist is obtained which can be washed, be dried, and then be reproduced; a true antozone mist.

A singular cause of error in his previous paper is here pointed out. When dry air is electrized, the mist is easily obtained on contact with water, even without the previous use of any de-ozonizing solution. Meissner hence stated that the mist could be formed without previous removal of the ozone. But in fact the nitrogen of the air, being oxydized to nitric acid by the ozone, acts as a de-ozonizing agent. He obtained however the same result with pure oxygen. But upon examining his apparatus, he found a cork previously used with a solution of potassium iodid, which had iodine upon it and thus acted as the de-ozonizer. In his later researches all these sources of error were avoided, and the conclusion established that to produce the antozone mist, the ozone must first be wholly or partially removed.

"As the result of many oft-repeated experiments," says Meissner, "it may be asserted that there is no single fluid through which, or no single solid over which, electrized oxygen can be passed without exerting some action upon it; even when the substance is itself not oxydable, or does not fix in chemical combination any constituent of the electrized oxygen, some action takes place by which the amount of ozone or antozone in the stream of oxygen is more or less diminished." Not only finely divided gold and platinum, but also entirely indifferent bodies, such as asbestus, cotton, calcium chlorid, charcoal-powder, or finely broken coal, destroy the ozone and antozone in electrized oxygen. Even after the removal of the ozone, if the mist be passed over finely granular calcium chlorid, the antozone will disappear. The same is true of liquids; a sulphuric acid valve included in an apparatus to prevent the entrance of moisture into the electrizing tube, lessens the amount of ozone and antozone obtained.

II.

Section II treats of the "Quantitative estimation of Ozone, and the contraction of volume on electrizing oxygen." The apparatus employed is similar to that above described, the electrizing tube being widened to enclose a thermometer, and having a delicate mercury-manometer attached to it. A bulb-apparatus of simple construction holds the de-ozonizing solution, and a second similar one contains sulphuric acid. The last bulb of the former apparatus contains fine asbestus, which is found to destroy almost perfectly the cloud-forming substance, and so to retain the iodic acid and iodine carried over with it. Both bulb-tubes have wires by which they may be attached to the balance. In making an observation, the apparatus is filled with pure oxygen, the manometer, barometer, and thermometer carefully noted, the oxygen electrized, time allowed for the temperature to be equalized, communication established with the manometer, the diminution of volume ascertained, and the quantity of active oxygen determined: 1st, by the increase in weight of the bulb apparatus, and 2d, by titration with sodium thio-sulphite or occasionally, sodium sulphite. The free iodine is first determined, and then the solution is acidulated with hydrochloric acid; the hydriodic acid thus set free reacts with the iodic acid to set free more iodine, which is then titrated by itself. Hence every iodine-equivalent represents one of oxygen, and by dividing the weight of iodine found by 15·875 (127÷8) the quantity of oxygen absorbed is ascertained; or in other words, the oxygen absorbed shown by the increase of weight, must be to the iodine set free as measured by titration, as 1 : 15·875. In the experimental results given in the table, this proportion is not reached, but varies from 1 : 14·9 to 1 : 13. This variation, observed by other experimenters, was investigated. It was found not to be due to the action of the unozonized oxygen, nor to any loss of iodine in the gas-current, nor to imperfections in the titrition itself; nor did it appear when the iodine was completely oxydized to iodic acid, both weighing and titrating then giving the same results. These last experiments, Meissner believes in passing, establish the fact that ozone is simply a modification of oxygen, not an oxydized water; that no hydrogen peroxyd is produced in it; and that the method of experimenting here employed is free from errors. He believes too, that the variations observed when the potassium iodid is incompletely oxydized, may be due to the production of some intermediate oxyd of iodine; and hence, regards the method by weight as less liable to error in estimating the amount of ozone, than the method of titration. If, however, the solution of potassium iodid be either very dilute or very concentrated, or particularly, if it be acidulated; or, what is the same thing, if a solution of hydriodic acid be used,—in which case no iodic acid is formed —then the method of titration becomes accurate, the results agreeing with those obtained by weight. The objections however to the acidulated potassium iodid solution are: 1st, there is a loss of iodine, carried off by the antozone; and 2d, the iodine separates in thick masses of crystals which stop entirely the delivery tube. When too, the ozone enters the absorbing solution very dense, as when a given portion of oxygen is electrized for a long time, and then passed at once into it, the differences between the results of weighing and titrating are very much increased; the results then varying from 1:8 to 1:4, instead of 1:15·8.

The experiments on the influence of electrical tension upon the production of ozone showed that with a velocity of 2·25 to 2·50 liters per hour, a spark of 1·5 centimeters gave ·006 grms. ozone, of 4 to 5 centimeters ·0274 grms, of 5 to 6, ·0322 grms., and of 6 to 7 centimeters ·0389 grams, as the maximum; no increase being obtained with that apparatus, on increasing the length of the spark.

On the question of the contraction of volume on electrizing oxygen, a series of twelve experiments, made with the apparatus already described, show that, as a mean, the weight of oxygen absorbed as ozone, bears to a weight of oxygen equal to the observed contraction, the ratio of 1·984 to 1; i. e., essentially of 2:1. That is to say, the portion of electrized oxygen absorbed by potassium iodid, weighs twice as much as the volume of oxygen which disappears during the electrization. This result was so extraordinary, that the experiment was carefully repeated, the potassium iodid being completely oxydized; the oxygen absorbed weighed 0·0182 grams, the contraction of volume 0·00938 grams, being virtually 2:1. Meissner is not prepared to claim this numerical relation, however; he simply maintains that the electrized oxygen absorbed by the iodid, weighs more than the volume corresponding to the contraction observed. He then goes on to discuss the methods by which Andrews and Tait, and von Babo and Claus, came to different results; criticising in the former case, the apparatus used, the formula for calculation, and the method of determining the ozone by titration; and in the latter, the use of a neutral solution of potassium iodid having a wrong concentration, and the velocity of the stream of oxygen, both of which, as shown above, affect the result. As to Soret's results, which were made with electrolytic oxygen, Meissner merely remarks that if made with the use of a neutral solution of iodid as absorbing agent, as seems to be the case, they may not be reliable; though not having prepared ozone in this way, he can pass no further judgment upon them. No conclusion however, upon the density of ozone can be drawn by Meissner from his own experiments, since the results obtained are due to both the modifications of oxygen produced by the electrization; while the ratio of the two present, is unknown.

Finally, Meissner takes up the question of the electrizing of confined oxygen. He finds that the limit of the production of ozone is nearly reached in the first 10 to 20 minutes; that on diminishing the electrical tension after a time, the amount of ozone is increased; that the maximum contraction is 8·61 per cent, corresponding to $\frac{1}{11\cdot6}$, instead of $\frac{1}{12}$ as obtained by Andrews and Tait; that more than eight times the quantity of ozone is produced when the oxygen flows in a current through the electrizing tubes, than when confined, with an equal tension; that the agency which causes the return of the ozone and antozone to common oxygen, increases in activity with the density, with the percentage of electrized oxygen, and, in a given apparatus, with the electric tension; so that when this electrized oxygen reaches a certain density, the further action of the electricity destroys as much ozone and antozone as is produced. He believes that the electrization of the oxygen is dependent on the tension, the destruction of the ozone and antozone on the character of the discharge, so that for every apparatus there is a limit to the production of ozone, beyond which, if the tension be increased and the glass be not broken, the ozone and antozone rapidly disappear. If the oxygen be dry, it retains its diminished volume for many days; but if moist, much less electrized oxygen is produced, and it disappears entirely in the course of two days. Mercury appears also to have a specific action in destroying electrized oxygen.

This second research of Dr. Meissner, though not as startling in its results as the first, is yet quite as valuable a contribution to science. He has fully established the main positions taken in 1863, and it cannot longer be doubted that electrized oxygen contains two modifications of this element. The bearing of this fact upon our theories of ozone is evident.

ART. XXII.—*Description of Sclerostoma pinguicola, a new species of Entozoa, from the Hog;** by A. E. VERRILL.

ON two occasions I have received specimens of a rather large parasitic worm, which lives in the fat of hogs. In the first case, five specimens were obtained, at New Haven, by Dr. M. C. White, from the fatty portion of a spare-rib; in the second instance, at Middletown, Conn., Dr. N. Cressy found large numbers of the worms in the fat about the kidneys of a young Suffolk pig, brought from New Jersey. Unfortunately, none of these specimens are in so good a state of preservation as to enable me to determine with certainty all the points of their structure. Those which I owe to the kindness of Dr. White had been mounted in glycerine, as microscopic objects, and pressed out flat, before they came into my possession, and the tissues were thus injured and the organs deranged. Those from Dr. Cressy were both pressed flat and dried. Yet by careful maceration, and considerable labor, I believe that most of the important characters have been made out. The body is rather robust, especially in the female, and tapers to both ends; the color is yellowish white, and the integument is seen to be finely striated transversely, when considerably magnified. The head is smaller than the body, truncated at the end; the mouth is terminal, roundish, or somewhat angular, surrounded by the thickened rim of the chitinous capsule, or pharynx. The edge of this chitinous ring rises at intervals into four to six denticles, or very small angles, which correspond to thickened, longitudinal, chitinous bands, that strengthen the pharynx, and give a slightly angular form to the mouth. The pharynx itself is small, short, and rather squarish, when seen in profile, and has three or four small, conical teeth at the bottom. The œsophagus is thick, club-shaped, and very muscular.

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* Extracted from "The External and Internal Parasites of Man and Domestic Animals;" by A. E. Verrill, page 138, figure 89*a*, July, 1870.

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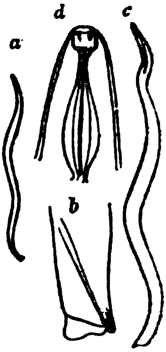

The male is 1·12 of an inch long, and about ·05 of an inch in diameter; the tail ends in a small blunt lobe, united with two small, entire, membranous expansions, one on each side, forming a small bursa, which is strengthened by several short rays, the exact number of which could not be determined in my specimens; there are two long, slender spicules. The females are 1·25 to two inches in length, and, as flattened between glass, they are ·10 to ·13 of an inch in breadth. The posterior end suddenly and obliquely narrows to a small conical point, which is turned to one side. The anal opening is close to the end, and the genital orifice appears to be adjacent to it. The oviducts are long, voluminous, much convoluted, and unite in a large and capacious uterus, which fills most of the cavity of the body toward the posterior end. The uterus and oviducts are both filled, in the larger specimens, with immense numbers of small oval eggs.

It is probable that this parasite is by no means uncommon.

Figure 1.—*Sclerostoma pinguicola* Verrill; *a*, male, natural size; *b*, posterier end, enlarged, showing the bursa and spicules; *c*, female, natural size; *d*, anterior end of the same, enlarged, showing the chitinous capsule and œsophagus.

ART. XXIII.—*Notes on the structure of the Crinoidea, Cystidea and Blastoidea;* by E. BILLINGS, F.G.S., Paleontologist of the Geological Survey of Canada.

(Concluded from this Journal, II, vol. xlix, p. 58).

6. *On some points relating to the Structure of Pentremites.*

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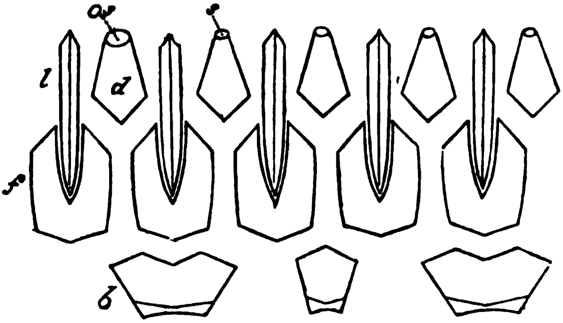

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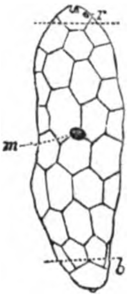

Fig. 1.—Calycine plates of *Pentremites*,—*b*, the basals; *f*, one of the five forked plates; *d*, deltoid plate; *l*, lancet plate; *os*, oral spiracle; *s*, spiracle.
Fig. 2.—*Caryocystites testudinarius*, Hisinger,—*b*, basal plates; *r*, radials; *m*, mouth.

Professor Wyville Thompson has proposed a division of the skeleton of the existing Crinoid, *Antedon rosaceus*, into two systems of plates, which he terms respectively the "*Radial*," and the "*Perisomatic*" systems.* These he considers to be thoroughly distinct from each other in their structure and mode of growth. The radial system consists of the joints of the stem, the centrodorsal plate, the radial plates, the joints of the arms, and also those of the pinnules. In the perisomatic system he includes the basal and oral plates, the anal plate, the interradial plates, and any other plates or spicula which may be developed in the perisom of the cup or disc. This I think a good arrangement, except in so far as it regards the stem, which appears to me to be, always, an appendage of the perisomatic, rather than of the radial system.

Throughout the whole range of the Crinoidea, the plates of the radial and perisomatic systems, are easily distinguished from each other. In general, the Cystidea have no radial plates in their calyces except, perhaps, in a small area around the ambulacral orifice. This accords well with an important observation of Professor Thomson's on the structure of Antedon, while in the earlier periods of its growth. "The entire body of the Pentacrinoid is," he says, "at first, while yet included within the pseudembryo and during its earliest fixed stage, surrounded and enclosed by plates of the perisomatic system alone, and it is quite conceivable that plates belonging to this system may expand and multiply so as to form a tessellated external skeleton to the mature animal, the radial system being entirely absent, or represented only in the most rudimentary form." (Op. cit., p. 541). Such is the structure of all of the Cystidea. On referring to fig. 2, it will be seen that the whole of the body of *Caryocystites testudinarius*, is covered with polygonal plates, without any trace whatever of a radiated arrangement. The plates are disposed in nine transverse ranges, girding the body like so many rings. This species is, (and so are most of the elongated subcylindrical Cystideans), annulated rather than radiated, so far as regards the external integument. The lower range, below the line, *b*, consists of the basals, whilst the upper, above the line, *r*, may, possibly, be radiated. In all the globular or ovate Cystideans, with numerous plates, such as *Spœnonites*, *Malocystites*, *Comarocystites*, *Amygdalocystites*, and others, the shell is neither annulated nor radiated, but composed of an indefinite number of plates, increasing with the age of the individual, and arranged without any well defined or constant order. It seems clear, therefore, that the test of the Cystidea belongs mostly to the perisomatic system.

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* On the Embryogeny of Antedon rosaceus Linck (*Comatula rosacea* of Lamarck). By Professor WYVILLE THOMPSON, LL.D., &c. Philosophical Transactions of the Royal Society, vol. clv, Part II, p. 540.

In *Pentremites* the three plates which are usually called the basals, consist each of two pieces, one placed above the other, and, in general, closely anchyclosed together. The lower pieces have each a re-entering angle, in their upper edges, for the reception of the upper pieces which stand upon them. This structure was first pointed out by Mr. Lyon (Geol. Ky., vol. iii, p. 468), and is not generally admitted, although I believe it certainly does exist. It is said that the lower pieces consist of the upper joint of the column, divided into three by vertical sutures. To me they appear to be calycine plates. It is true that they do not form the bottom of the visceral cavity, but this may be due to the growth inward of the lower edges of those of the upper series. Something like this occurs in *Antedon*, where, at first, the bottom of the cup is formed by the basals, but afterwards principally by the first radials.

The forked plates are usually called "*Radials*," but they certainly do not belong to the radial system. If they did, they would represent the first radials of the Crinoidea, and therefore they should support the bases of the ambulacra. A little consideration will, however, enable any one to perceive that in *Pentremites* the bases of the ambulacra, are situated in the apex of the fossil, and do not come in contact with the forked plates. The apex of *Pentremites* is identical with the actinal center of Sea-urchins and Star-fishes, in which the mouth is situated. It is here that the ambulacra originate and grow outward by the addition of new plates to their distal extremities. There can be little doubt that such was the mode of growth of the ambulacra of the *Pentremites.* The smaller extremity, therefore, of their ambulacra, which is received into the forked plate, is not the base, but corresponds with the apex of the ambulacrum of a Sea-urchin or of a Star-fish. It also represents the tip of the arm of a Crinoid. If the forked plate is radial, then the arrangement of the ambulacrum must be the same as that which would be exhibited in a Crinoid, with the upper end of the arm downward, and resting on the first radial, whilst the lower end would be upward, the tip being formed of the second radial. From this it follows that the forked plates do not belong to the radial, but to the perisomatic system.

The five deltoid plates alternate with the forked plates, and are also perisomatic.

It is not certain that the lancet plates represent any of those plates which in the Crinoidea are usually called "radials." They are so arranged that if they were loosened from the walls of the cup, and their smaller extremities turned upward, whilst their bases or larger ends retained their position, they would stand in a circle around the apex, as do the arms of an ordinary Crinoid. Their bases would alternate with the apices of the deltoid plates. They would form the outside of the arms, whilst the grooves and pinnulæ would be inside. Each would bear, on its outer or dorsal aspect, two elongated sacks, the two hydrospires that belong to the ambulacrum. I believe that the small groove in the ambulacrum of *Pentremites* was occupied by the ovarian tube only. If this be true, and if, also, the lancet plates represent the radial plates of the arms of the Crinoids, then the arm of *Pentremites* would have the respiratory portion of the ambulacral system on its dorsal, and the ovarian portion on its ventral aspect.

In the true Crinoids, both the respiratory and ovarian tubes are situated in the groove in the ventral side of the arm.* In the Crinoids the pinnulæ are attached to the radial joints of the arm. In *Pentremites* they are not connected with the lancet plate, but with the pore plates. In *P. pyriformis* they appear to me to stand in sockets excavated in the suture between the pore plates proper, and the supplementary pore plates. Miller compared them to the series of azygos plates, which underlie that portion of the ambulacrum of *Pentacrinus* that runs from the mouth to the base of the arm. These resemble the lancet plates, in their being azygos and not connected with pinnulæ; but then, on the other hand, they differ from them in having, a portion at least, of the respiratory tubes on their ventral aspect. Mr. Rofe says that, "in many species of Pentremite, if not in all, this lancet plate is in reality a compound plate, formed of two contiguous plates, extending from the bottom of the sinus to the top, and, then turning right and left round the summit-openings, they pass down the adjoining sinus, to form half its lancet-plate, leaving at the apex of the body a pentagonal aperture, supposed to be the mouth. In some weathered specimens, the two parts of the lancet plate are separate; and in many they appear to meet only at the top and bottom of the cross section, leaving a lozonge-shaped opening between them." (Geol. Mag., vol. ii, p. 249.) In a large specimen of *P. obesus* (Lyon and Cassiday) which was given to me by Mr. Lyon, a polished section shows that one of the lancet plates is thus divided, but in general no trace of a suture can be seen in these plates.

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* Thomas Say, who was the first to recognize the Blastoidea as a group distinct from the Crinoidea, also supposed the function of the ambulacra to be respiratory. He says, "I think it highly probable that the branchial apparatus communicated with the surrounding fluid through the pores of the ambulacræ, by means of filamentous processes; these may also have performed the office of tentacula, in conveying food to the mouth, which was, perhaps, provided with an exsertile proboscis; or may we not rather suppose that the animal fed on the minute beings that abounded in the sea water, and that it obtained them in the manner of the Ascidia, by taking them in with the water. The residuum of digestion appears to have been rejected through the mouth." (Jour. Acad. N. S. Phil., vol. iv, p. 296, 1825).

There are several points in the structure of the ambulacra of *Pentremites* that are well worthy of the study of those who have plenty of well preserved specimens. Among these, I would direct special attention to the markings in the ambulacrum of *P. pyriformis.* The median groove, which I suppose to have been exclusively occupied by the ovarian tubes, sends off branches, right and left alternately, toward the sides of the ambulacrum. These branches do not run directly to the ambulacral pores. Each of them terminates at a point between the inner extremities of two of the pores. There is at this point a small pit which appears to be the socket of an appendage quite distinct from the pinnule. The groove does not reach the socket of the pinnule, which is situated further out, between two of the pores. On the other hand a small groove runs from each pore, inward, and terminates at another socket, about half-way between the pore and the main median groove of the ambulacrum. It would thus appear that besides the ordinary pinnules, there were two other rows of appendages on each side of the median groove.

The general conclusions at which I have arrived from the above, are, that all the principal plates that compose the shell of *Pentremites*, belong to the perisomatic system of Professor Wyville Thompson; that it is doubtful whether or not the lancet plates are homologous with the radial plates of the Crinoids; and that the ambulacra are more complicated in their structure than is generally supposed.

7. *On the Structure of the genus Nucleocrinus.*

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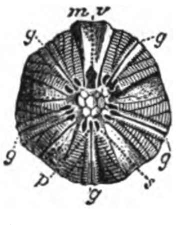

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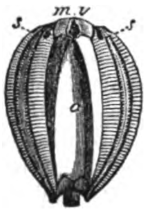

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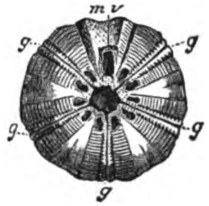

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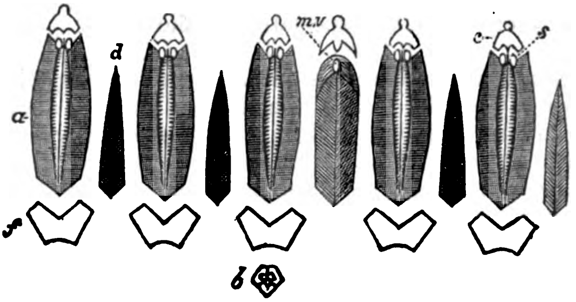

Fig. 3.—Apex of *Nucleocrinus Verneuilii* Troost. *g*, ambulacral groove; *p*, pore through which groove enters into the interior; *s*, one of the ten spiracles; *mv*, oro-anal aperture. 4. Anterior side of a specimen; *a*, the anterior interradial. 5. Apex of a specimen which has lost the integument that covered the center. 6. Diagram of the plates of the test; *a*, ambulacral plate; *b*, the basals; *c*, plates of the apex; *d*, one of the interradials; *f*, forked plate.

The body of this remakable genus is ovate, elliptical or oblong, and inclosed in a shell of strong perisomatic plates, which are, in general, so closely anchyclosed that the sutures between them cannot be distinguished. According to Mr. Lyon, who, through his long continued geological researches, has collected and studied a vast number of specimens, there are three minute lozenge-shaped, or quadrilateral basal plates, situated at the bottom of the columnal pit; always concealed when the column is present. These are surrounded by three other plates, the six altogether corresponding to the six pieces which constitute the compound basal plates of *Pentremites*. They are represented at fig. 6, *b*, as figured by Mr. Lyon (Geol. Ky., vol. iii, pl. v, fig 1, *b*.)

In the next series there are five plates which are undoubtedly the homologues of the five forked plates of *Pentremites.* They are very short and confined to the base of the body. They form a shallow basin with ten re-entering angles in its margin. Fig. 6, *f.*

Alternating above the forked plates, are five pieces corresponding to the deltoid or interradial plates of *Pentremites.* Some of these are lanceolate in form (fig. 6, *d*), their broader extremities fitting into the angles between the forked plates. They taper to a point upward, and their sides are bevelled so as to pass under the ambulacral plates, to which they are, in general, so closely united, that the line of junction is indicated only by the difference in the markings of the surface. Owing to this structure, these plates have not always been recognized by the authors who have described this genus. They were first pointed out by Mr. Lyon. The fifth deltoid or interradial plate is truncated at its apex for the reception of the ovo-anal orifice (*mv*, figs. 4, 6). The sutures on each side of this plate are generally distinctly visible, especially in the upper part of the body.

The ambulacra are narrow—one line wide in a specimen fifteen lines in length, with a fine median groove, about large enough to accommodate a tube of the size of a horse-hair. There are two rows of pores, those on one side of the groove alternating in position with those on the other side. These pores lead into the hydrospires. There appear to be only two rows of ambulacral ossicles. The pores are situated in the sutures between them. On each side of the ambulacrum there is a broad transversely grooved marginal plate. From each pore a small rounded ridge runs across this plate. The grooves between the ridges originate at the outer extremities of the ambulacral ossicles. In well-preserved specimens the surface of these marginal plates exhibits no other structure than the transverse grooves and ridges; but in one weathered specimen that I have examined, they seem to be composed of a number of narrow elongated pieces, arranged transversely, in such a manner that two of them abut against the outer extremity of each of the ambulacral ossicles, and extend outward toward the interradials. This seems to prove that the marginal plates belong to the ambulacra, as pointed out by Mr. Lyon, and not to the interradials, as represented by other authors. Although I have studied a large number of specimens, none of them were sufficiently perfect to enable me to make out the whole structure of this part of the test of *Nucleocrinus.* I have, however, seen enough to convince me that the ambulacra are much more complex than is usually supposed. The lancet plate, if it occur at all in this genus, must be very narrow. The ambulacral groove, as in *Pentremites*, sends off branches, right and left.

There is also evidence of the existence of minute marginal plates on each side of the groove.

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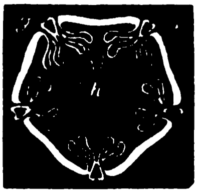

Fig. 7. Transverse section through a specimen which has all the hydrospires preserved. *h*, the two anterior hydrospires; *p*, pore leading into the hydrospire; *g*, one of the grooves.

The hydrospires are ten elongated sacks, each with two deep folds. They are perfectly homologous with those of *Pentremites*, only differing therefrom in not being united in pairs; consequently there are ten spiracles instead of five. The mouth, or oro-anal orifice, is larger in proportion to the size of the body than it is in *Pentremites*. Mr. Meek informs me that the mouth in some of the Blastoidea is protected by a single valve that covered it like the lid of a jug. From the structure of the orifice, I am inclined to think that in *Nucleocrinus* it possessed a similar protection.

In the apex, nearly all the space within the circle of apertures is covered by a thin integument of small plates, fig. 3. When this is not preserved, a large sub-pentagonal aperture is seen, as shown in fig. 5. This aperture occupies the position of the mouth in the existing echinoderms. The integument, as will be shown further on, represents that which covers the mouth of an embryonic Star-fish. Mr. Conrad described this genus in 1842, as having only one aperture in the summit. "This genus differs from PENTREMITES, Say, in having only one perforation at top, which is central." (Jour. Acad. Nat. Sci. Phil., vol. viii, p. 280, pl. xv, fig. 17). His figure represents the fossil with the apex downward. Dr. Ferd. Roemer, showed that, when perfect, there is no central opening, and he made this one of the grounds for separating the genus from *Pentremites*. He described the apex as being provided with six apertures, five of which were divided by a partition within each. These he considered to be the ovarian orifices. The sixth he supposes to be both mouth and vent, which accords with my view. (Mon. der Blastoideen, p. 378). In 1868 I discovered the five small pores at the apical extremities of the ambulacral grooves. (This Jour., II, xcvii, p. 353, and Annals Nat. Hist., IV, vol. 4, p. 76). In general it is difficult to see these pores, but if a silicified specimen, which has been fossilized in a calcareous matrix, be placed in an acid for two or three minutes, the acid cleans them out and they then become distinctly visible. I believe these to be the pores through which the ovarian tubes passed outward along the grooves to the pinnulæ. There are thus, sixteen apertures in the apex of *Nucleocrinus*,—ten spiracles, five ovarian orifices, and one oro-anal aperture. There are no true radial plates. The whole of the test with the exception, perhaps, of the ambulacra belongs to the perisomatic system.

8. *On the occurrence of Embryonic forms among the Paleozoic Echinoderms.*

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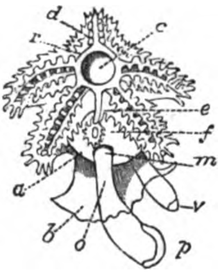

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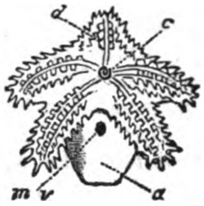

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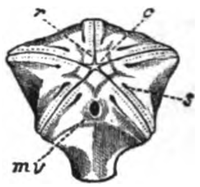

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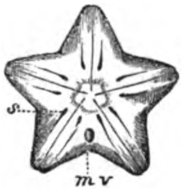

Fig. 8. ***Bipinnaria asterigera*** **Sars, (copied from Müller). *a*, the stomach; *b*, part of the body of the larva; *c*, ambulacral centre, position of the permanent mouth, in this stage not open; *d*, one of the five ambulacral canals; *e*, sand canal, *f*, madreporic plate; *m*, entrance into the stomach; *o*, œsophagus; *p*, larval mouth or pseudostome; *r*, œsophageal ring; *v*, vent. 9. Ideal figure described below. 10. *Codonites stelliformis*, oblique view to show both body and summit. 11. Summit of fig. 10.**

No proposition in Natural History has been more clearly demonstrated than this:—That, in general, the paleozoic animals resemble, both in external form and internal structure, the embryonic stages of those of the same class at present existing. Prof. Agassiz has long taught in his lectures and various publications, that this is especially observable in the Echinodermata. Judging from the figures and descriptions of Müller, Agassiz, Thomson, Carpenter and others, I should say, that in this class, the most striking resemblance is that which occurs between the adult stages of the Cystidea, Blastoidea, and Crinoidea, on the one hand, and the embryonic Star-fishes on the other. The structural character that has the most important bearing on the subjects discussed in these notes, is, that in all four of these groups, the mouth is situated in one of the interradial areas,—not in the ambulacral center, as it is in the adult forms of the existing Echinodermata.

In *Bipinaria asterigera* Sars, according to Müller, the digestive cavity is a sub-globular sack without any extensions into the rays, as there are in the adult Star-fishes. The œsophagus, fig. 8, *o*, is a fleshy, consistent tube, with a large mouth or pseudostome, *p*. It passes through the wall of the stomach by an opening somewhat smaller than the mouth, and situated in one of the interradial spaces at *m*. The madreporic plate, *f*, and sand canal, *e*, the latter holding the convoluted plate (when it occurs), are situated above the orifice, *m*, and between it and the ambulacral center, *c*. The circular space at *c*, is undoubtedly the homologue of the central space in the apex of *Nucleocrinus*, figs. 3 and 5, and of *Codonites*, figs. 10 and 11. It is also the position of the mouth in the adult Star-fish; but in the larval stage it is completely closed by the soft external skin and sarcode of the body. In the fossils it is also closed, but by an integument of thin calcareous plates. The *Bipinnaria* is nourished by minute particles of matter diffused through the water, and drawn into the digestive sack through the mouth and œsophagus by the action of interradial cilia. I believe that all the fossil Crinoidea, Blastoidea and Cystidea, ingested their food in this way, and without any aid whatever from the arms or pinnulæ.

Perhaps there is no embryologist who will not admit, that it is possible for an animal like *Bipinnaria* to develope organs of reproduction and propagate its species, none of its other parts making any farther advance. Such an animal, with some slight modifications, would not be very widely different from a paleozoic Crinoid. If the sarcodic body wall were to be consolidated into a thin calcareous integument, with the mouth even with the surface, the swimming appendages aborted, and the vent closed up, it would resemble the cup of an *Actinocrinus*, fig. 9, *a*. The lateral orifice would then be both mouth and vent, as it is, at first (according to Prof. A. Agassiz, Seaside Studies, p. 125), in the embryo of *Asteracanthion Berylinus*. The ambulacral canals of *Bipinnaria* are the homologues, in a general way, of those which are found beneath the vault of *Actinocrinus*, and extend out into the grooves of the arms. If the ventral perisome of the Crinoid were to be removed (the internal organs remaining undisturbed) the arrangement disclosed would be that represented in fig. 9,—a convoluted plate in the center with the canals radiating from it. The most striking difference is the absence of the œsophageal ring. According to the organization of *Actinocrinus* there could be no œsphagus at that point, and consequently there is no ring. The convoluted plate represents the madreporic apparatus. The sucking feet of the Star-fish, most probably, represent the respiratory tentacles that border the grooves of the Crinoids, but modified into prehensile and locomotive organs. *Bipinnaria* and *Actinocrinus* agree in having the mouth in one of the interradial areas, and in the absence of an orifice through the perisome at the ambulacral center. These two characters are embryonic and transitory in the Star-fish, but they were permanent in most paleozoic Crinoids.

In *Codonites stelliformis* (*Pentremites stelliformis* Owen and Shumard), figs. 10, 11, the ambulacral center, *c*, is completely closed. Five minute grooves radiate out to the extremities of the five angles of the disc. These grooves are identical with those of *Pentremites* and *Nucleocrinus* and were occupied by the ovarian tubes. The ambulacral canals of the true Crinoids and of the Star-fishes are represented in a rudimentary condition, in this species, by the hydrospires which open out to the surface through the ten fissure-like spiracles, *s.* The oro-anal orifice is interradial. *C. stelliformis* in external form, the interradial position of the mouth, and the closed ambulacral center, resembles *Bipinnaria* and *Actinocrinus*, but differs importantly in having its respiratory organs arranged in ten separate tracts, all totally disconnected from each other. It is a lower form than *Actinocrinus*, which in its turn is lower than *Bipinnaria*, and yet all three are constructed on the same general plan.

C. stelliformis, although much resembling a *Pentremite*, is a true Cystidean. Its affinity to *Codaster* was first pointed out by Dr. C. A. White, who also suggested that it should be assigned to a distinct group. (Bost. Jour. N. H., vol. vii, pp. 486, 487). The main difference between the Cystidea and the Blastoidea is, that in the former the hydrospires do not communicate with the pinnulæ, whilst in the latter the cavities of the pinnulæ and hydrospires are directly connected by the ambulacral pores.

The developement of the recent Crinoid *Antedon rosaceus*, as described by Prof. Wyville Thomson (Phil. Trans., 1866), pursues a course that could not possibly result in the production of such an animal as *Actinocrinus*. The pseudembryo, as it is called by Prof. Thomson, is a small ovate organism, with four transverse ciliated bands, a large key-hole-shaped mouth (pseudostome), and a small circular vent (pseudoproct). These orifices are connected by a rudimentary intestine (pseudocele). In this stage there is no trace of radiation, and the mouth, therefore, cannot be said to be interradial in its position.

The nascent Crinoid originates within the pseudembryo, but developes a mouth, vent and stomach, of its own, all quite distinct from those of its nurse. This new, or permanent mouth, is for a short time both oral and anal in its function, but although in this respect it resembles that of *Actinocrinus*, its position in the center of the ambulacral system, shows it to represent the mouth of the adult Star-fish, while that of *Actinocrinus* rather homologates with the oral orifice of the *Bipinnaria.* At no time during its development does the ventral perisome exhibit the structure of that of the paleocrinoids, i. e., no orifice in the ambulacral center, and at the same time one in an interradial space. In the central position of its mouth, and in the possession of an œsophageal ring, *Antedon* stands above *Actinocrinus* in rank, and between it and the adult Starfish. In none of its stages does it resemble a *Bipinnaria* either in form or in structure.

9. *On some of the objections that have been advanced against the views advocated in the preceding notes.*

In all the known species of the existing Echinodermata, the mouth is situated in the center of the ambulacral system, and it is contended that this fact proves that such must have been its position also in the paleozoic forms.

This reasoning is not strictly logical. It is true that in the known existing species, the mouth is in the center, but it does not certainly follow that it is so in all the Echinodermata, living and extinct. Whether it be so or not in any particular fossil species whose structure may be under investigation, *is a question of fact which can only be positively determined by direct observation of specimens.* On appealing to these we find that, in a large proportion of the fossil forms, there is no aperture in the perisome at the ambulacral center. It also becomes evident by the comparison that, in general, the paleozoic species resemble the embryonic stages of some of the recent Echinoderms, and that in these, (*Bipinnaria* for instance), the mouth is interradial. Rules such as is relied on in this case, afford a certain amount of presumptive evidence, which, however, cannot prevail against material and visible facts. When we can see clearly that there is no aperture in that point, in the vault of a Crinoid, beneath which we know the ambulacral center is situated, it is perfectly useless to supply one by deduction.*

The second objection is, that many of the fossils have a *Platyceras* attached to them, in such a position as to cover the aperture which I call the mouth, and under such circumstances as to induce the belief that it lived parasitically on the Crinoid. The only answer I can make to this is that, admitting the facts, we must suppose that space was left for a stream of water to pass under the edge of the shell, into the mouth of the Crinoid. In general, where one animal lives parasitically upon another, it does not destroy its host. Some of the gasteropods of the Devonian and Carboniferous ages, were carnivorous, as is proved by the bored shells and Crinoids that are occasionally found. I have seen a number of such specimens, and several years ago I read a paper on the subject (which was never published) before the Natural History Society of Montreal. There were several good Conchologists present, and the specimens exhibited were compared with bored shells of existing species. All pronounced the style of workmanship to be precisely the same. I have the proboscis of an *Actinocrinus* that is bored near the base, and among the fossils lent me by Mr. Wachsmuth, is a *Codonites stelliformis*, that is bored through one of the ambulacra. The view I took of the subject in my paper, was that the gasteropod ascended the stalk of the Crinoid, and thrust its proboscis into the mouth of the latter. The Crinoid then slowly drew its arm together, and held the shell fast until both died.

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* The position of the ambulacral center may thus be found. When the mouth is eccentric, the ambulacral tubes usually converge to the center of the vault. But when the mouth is central, we first find the azygos interradius, in general easily recognized by its possessing a greater number of plates than do any one of the other four interradii. On the opposite side of the fossil is the azygos arm. The ambulacral center is always situated between this arm and the mouth, never on the side of the mouth toward the azygos interradius.

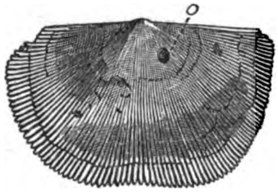

Fig. 12. ***Streptorhynchus Pandora.*** **A specimen bored at *o* by a carnivorous gasteropod. From the Corniferous Limestone, Devonian, Canada.**

A third objection is the small size of the aperture in some of the species. In general, where there is no proboscis, the orifice is from one-twentieth to one-tenth of an inch in diameter, quite sufficient for an animal that subsists on microscopic organisms. It is stated by Meek and Worthen that where there is a proboscis, the aperture is sometimes scarcely "more than *one-hundredth* of an inch in diameter." I believe that in many such instances the tube filled up by calcareous deposits on its inside, and that when entirely obstructed, either a new aperture opened out in the side of the proboscis, or that the animal died. In Mr. Wachsmuth's collection, I saw a specimen with a second aperture in process of formation. A ticket was attached to it by him, giving this explanation. I am also informed that in some of the existing species of *Antedon* "the mouth is an exceedingly minute aperture."

A fourth objection is that the aperture is so situated that the arms could not have conveyed food to it. It is, however, proved by Dr. W. B. Carpenter, that in the recent Crinoids the arms are not prehensile organs. The animal while feeding remains motionless, attached by its dorsal cirrhi to a stone, shell, or other object on the bottom. Its arms are either stretched out to their full length, or more or less coiled up, but quite immovable. As Dr. Carpenter's remarks have a very important bearing upon the subject, I shall take the liberty of quoting the following :—

> "Whatever may be the purpose of the habitual expansion of the arms, I feel quite justified that it is *not* (as stated by several authors whom I have cited in my historical summary) the prehension of food. I have continually watched the results of

the contact of small animals (as Annelids, or Entomostracans and other small Crustaceans) with the arms, and have never yet seen the smallest attempt on the part of the animal to seize them as prey. Moreover, the tubular tentacula with which the arms are so abundantly furnished, have not in the slightest degree that adhesive power which is possessed by the "feet" of the ECHINIDEA and ASTERIADA; so that they are quite incapable of assisting in the act of prehension, which must be accomplished, if at all, either by the coiling-up of a single arm, or by the folding-together of all the arms. Now I have never seen such coiling up of an arm as could bring an object that might be included in it into the near neighborhood of the mouth; nor have I seen the contact of small animals with a single arm produce any movement of other arms towards the spot, such as takes place in the prehensile apparatus of other animals. Moreover, any object that could be grasped either by the coiling of one arm, or by the consentaneous closure of all the arms together upon it, must be far too large to be received into the mouth, which is of small size and not distensible like that of the ASTEROIDA."*

Farther on Dr. Carpenter says:

"It was affirmed by M. Dujardin (l'Institut, No. 119, p. 268) that the arms are used for the acquisition of food in a manner altogether dissimilar to ordinary prehension; for recognizing the fact that the alimentary particles must be of small size, he supposed that any such, falling on the ambulacral (?) furrows of the arms or pinnæ, are transmitted downwards along those furrows to the mouth wherein they all terminate, by the mechanical action of the digitate papillæ which fringe their borders. This doctrine he appears to have abandoned; since in his last account of this type (Hist. Nat. des Echinoderms, p. 194) he affirms that the transmission of alimentary particles along the ambulacral (?) furrows is the result of the action of cilia with which their surface is clotted. Although I have not myself succeeded in distinguishing cilia on the surface which forms the floor of these furrows, yet I have distinctly seen such a rapid passage of minute particles along their groove as I could not account for in any other mode, and am therefore disposed to believe in their existence. *Such a powerful indraught, moreover, must be produced about the region of the mouth, by the action of the large cilia which (as I shall hereafter describe) fringe various parts of the internal wall of the alimentary canal, as would materially aid in the transmission of minute particles along those portions of the ambulacral (?) furrows which immediately lead toward it;* and it is, I feel satisfied, by the conjoint agency of these two moving powers that the alimentation of Antedon is ordinarily affected. In the very numerous specimens from Arran the contents of whose digestive cavity I have examined, I have never found any other than microscopic organisms;

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* Researches on the Structure, Physiology, and Development of *Antedon* (*Comatula*, Lamk.) *rosaceus*.—Part I. By W. B. Carpenter, M.D., F.R.S. Philosophical Transactions of the Royal Society, vol. clvi, Part II. 1866

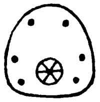
and the abundance of the horny rays *Peridinium tripos* (Ehr.) has made it evident that in this locality that Infusorium was one of the principal articles of its food. But in Antedons from other localities, I have found a more miscellaneous assemblage of alimentary particles; the most common recognizable forms being the horny casings of ENTOMOSTRACA or of the larvæ of higher CRUSTACEA." (Op. cit., p. 700).

The existence of large cilia within the intestinal canal, capable of producing a powerful indraught of water, renders any movement or concurrent action of the arms quite unnecessary in the ingestion of food. It does not matter, therefore, in what part of the body the mouth of a Crinoid may be situated, or how remote from the reach of the arms. Attached permanently to the bottom of the sea by their columns, the paleozoic Crinoidea, Cystidea and Blastoidea remained, while feeding, most probably motionless, drawing in streams of water through their mouths by the action of their intestinal cilia. The long tubular proboscis, with which many of the species are provided, would be, thus, analogous in function to the siphon of the acephalous mollusca. The indigestible particles would be, from time to time, thrown out through the mouth, just as a Star-fish or a Zoophyte frees itself of the refuse portions of its food, by casting it out of the same aperture through which it entered.

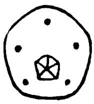
10. *On the Theory that the ambulacral and ovarian orifices are the oral apertures.*

Assuming that the four objections above noticed are sufficient to prove that the aperture which I call the mouth is not that organ, it is contended that the Cystidea, Blastoidea and Palæocrinidea ingested their food through their ambulacral and ovarian orifices. This appears to me in the highest degree improbable. In the recent Crinoids the grooves of the arms are occupied by four sets of tubes, which Dr. Carpenter calls the cœliac, the sub-tentacular, the ovarian and the tentacular canals. None of them communicate with the stomach. It is impossible that the most minute particle of food could gain access into the interior of the animal through any of them. The structure of the arms of the paleozoic Crinoids is such, that we must presume that their grooves were occupied by similar tubes, which passed through the ambulacral orifices into the perivisceral space. In the Cystidea and Blastoidea the respiratory organs were not situated in the grooves of the arms, and the ambulacral orifices were therefore only ovarian in their function. The improbability of their being also oral apertures is best shown by an illustration.

13.



14.



In fig. 13, is represented (natural size) the apertures of the smallest specimen of *Caryocrinus ornatus*, in our collection, selected for the present purpose because in the young of this species, the valvular orifice is larger in proportion to the size of the disc, than it is in the adult. It is in this specimen, about one-third of the whole width of the apical disc, while in a full grown *Caryocrinus* it is only one-ninth of the width. The same proportional size of the mouth according to age, occurs in *Antedon rosaceus.* The valvular mouth at first is as wide as the disc. But as the age of the animal increases the disc grows wider but the mouth does not. The ovarian pores in *Caryocrinus* are, however, as large in the small ones (once they make their appearance) as they are in those full grown. For recognizing these as ovarian pores we have the following reasons:—1. They are situated at the bases of the arms where the ovarian tubes must pass from the grooves into the perivisceral cavity. 2. When compared with the ovarian pores of a Sea-urchin they have the same size, form and aspect. Fig. 14, represents the ovarian pores of the Sea-urchin *Toxopneustes Drobachiensis* Ag. natural size and arrangement. It may not appear at first view that this latter comparison has any probative effect. But it has, in this way. If these apertures in *Caryocrinus* were large openings a line wide, as are some of the ambulacral orifices of the Crinoids, I would say that they were unlike true ovarian apertures.

According to the new theory, this Echinoderm *Caryocrinus ornatus* was a polystome animal, and drew in its food through its six ovarian apertures, the large valvular orifice being the anus. To me this appears to be utterly incredible.

In fig. 14 I have represented the mouth of *Leskia mirabilis* Gray. Both Dr. I. E. Gray and Prof. Lovén have pronounced this aperture to have the structure of the valvular orifice of the Cystidea. I have not the slightest doubt whatever but that the mouth of the Cystideans foreshadows that of the Sea-urchins. There is nothing whatever in its structure to show that it is not the mouth but on the contrary.

The new theory is not founded upon any peculiarities in the structure of the ambulacral orifices, which would show that they are oral apertures, but only upon the four objections above noticed. The first of these is not logical, while at the same time it is purely theoretical, and avails nothing against material and visible facts. The fourth is completely disposed of by Dr. Carpenter's observations, which prove that in the Crinoidea the arms have no share whatever in the ingestion of food. The second and third objections are the same in substance, i. e., according to the second the supply of water to the mouth, is diminished by the occurence of a *Platycerus* over it, while, according to the third, the same effect is produced by the small size of the aperture itself in some instances. It does not require much consideration to convince one, that if these two objections are fatal to my views, they are equally so to the opposite theory. In *C. stelliformis*, for instance, the pores through which we must suppose the ovarian tubes issued from the interior are only large enough to admit of the passage of a fine hair. They are scarcely visible to the naked eye. The tube, under any circumstances, must have filled them almost entirely. If any space at all were left for the passage of a stream of water through the pore by the side of the tube it must have been exceedingly minute.

When weighed as above, therefore, the evidence gives the following results:—The first and fourth objections avail nothing. The second and third militate against both theories. But when we take into account that in no instance, in the existing Echinodermata, where ovarian pores occur, are they at the same time oral orifices, the balance seems to be in favor of my view. This is all I desire to say upon the subject at present. Although I now firmly believe that the valvular orifice in the Cystidea, the larger lateral aperture of the Blastoidea, and the so-called proboscis of the paleozoic Crinoids are all oro-anal in function, yet I shall not maintain that view obstinately against good reason shown to the contrary.

ART. XXIV.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School.* No. 12.

§ 1.

On the precipitation and determination of the metals of the magnesium group in the form of oxalates; by W. GOULD LEISON.

PROF. GIBBS has recently* called attention to the fact that a number of metallic oxyds may be completely precipitated from their neutral solutions by means of oxalic acid, provided that a large excess of alcohol be also added. As it is not easy to obtain precise quantitative results by igniting the oxalates so precipitated, in consequence of the extreme subdivision of the resulting oxyds, Prof. Gibbs suggested the employment of potassic hypermanganate for the combustion of the oxalic acid, a method which—as is well known—gives excellent results in the case of calcic oxalate precipitated in the ordinary manner. The following investigation was undertaken for the purpose of testing this method of analysis:—

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* This Journal, xliv, p. 213.

Cadmium.—Cadmic sulphate was dissolved in the least possible quantity of water, oxalic acid added in excess, and then a large quantity of strong alcohol. The resulting oxalate was beautifully crystalline, and the precipitation was so complete that SH_2 gave in the filtrate a scarcely perceptible yellowish tinge. The oxalate was washed with alcohol by Bunsen's method and dried at 110° C., until every trace of alcohol was expelled. The filter was then pierced with a glass rod, and the cadmic oxalate washed into a flask with hot diluted sulphuric acid. A few cubic centimeters of strong sulphuric acid were then added, and the hot solution titrated with potassic hypermanganate. In this manner four experiments gave 44·19 pr. ct., 44·65 pr. ct., 44·88 pr. ct., and 44·27 pr. ct. of cadmium, as computed from the oxalic acid. These results are all much too high, and show that the acid had acted sensibly upon the filter. Two other experiments were then made. In the first a hot solution of ammonic sulphate was used as a solvent for the oxalate; in the second hot dilute chlorhydric acid was employed. Of the hypermanganic solution employed 100 c. c. contained 0·1103 gr. of available oxygen.

I. 0·4330 gr. cadmic sulphate required 24·5 c.c. hypermanganate = 43·68 pr. ct. Cd.
II. 0·3724 gr. cadmic sulphate required 21·1 c.c. hypermanganate = 43·74 pr. ct. Cd.

The received formula $3CdSO_4 + 8H_2O$ requires 43·75 pr. ct. In these two analyses the filters were not broken.

Barium.—Baric chlorid gave extremely variable results in my first experiment, notwithstanding the fact that the barium is completely precipitated by oxalic acid and alcohol. The resulting oxalate, after washing and drying, was not completely decomposed by sulphuric acid, which appeared to form a crust of baric sulphate upon the crystals of the oxalate. This difficulty was finally overcome by dissolving the baric oxalate in chlorhydric acid and diluting the solution largely. In this manner:

0·6505 gr. baric chlorid required 80 c.c. hypermanganate = 56·21 pr. ct. Ba (100 c.c. hypermanganate solution contained 0·053 gr. available oxygen). The formula $BaCl_2 + 2H_2O$, requires 56·15 pr. ct. Ba.

Strontium.—To avoid the use of paper filters so as to be able to employ sulphuric acid as a solvent, I resorted to sand filters. A light funnel was ground truly conical near the throat. A little pear of glass with a long stem was then dropped into the funnel, stem upward. In this manner a valve was formed impassible to the sand laid upon the ball of the glass, but allowing liquids to pass freely. By means of the stem the valve could be lifted from its seat, and the sand and precipitate washed together into a flask, after careful drying. With this arrangement:

0·4292 gr. strontic nitrate required 47·8 c.c. hypermanganate = 48·90 pr. ct. SrO.

0·3657 gr. strontic nitrate required 40·8 c.c. hypermanganate = 48·90 pr. ct. SrO. (100 c.c. hypermanganate solution contained 0·1099 gr. available oxygen.)

The formula $Sr(NO_3)_2$ requires 48·93 pr. ct. SrO. Sulphuric acid only was used to decompose the oxalate.

Calcium.—Iceland spar was dissolved in chlorhydric acid, and the solution treated with oxalic acid and alcohol. The filtrate contained calcium. When, however, the solution was evaporated to dryness before adding alcohol, and the oxalate was washed on a sand filter, no traces of calcium could be detected in the filtrate. In this manner:

0·5090 gr. $CaCO_3$ required 70·6 c.c. hypermanganate = 56·10 pr. ct. CaO. (100 c.c. hypermanganate corresponded to 0·11559 gr. oxygen.)

0·5590 gr. $CaCO_3$ required 77·5 c.c. hypermanganate = 56·08 pr. ct. CaO. (100 c.c. hypermanganate corresponded to 0·11495 gr. oxygen.)

The formula requires 56·00 pr. ct. CaO. Sulphuric acid only was employed.

Magnesium.—When magnesic sulphate is treated with oxalic acid, the mixture evaporated, but not to dryness, and alcohol added, the filtrate is perfectly free from magnesium. In this manner:

0·3243 gr. $MgSO_4 + 7H_2O$ required 39·6 c.c. hypermanganate = 16·18 pr. ct. MgO.

0·3949 gr. $MgSO_4 + 7H_2O$ required 48·4 c.c. hypermanganate = 16·25 pr. ct. MgO.

In these analyses the oxalate was collected on a paper filter and washed into the flask with water after piercing the filter, which was washed with cold dilute sulphuric acid. The formula requires 16·26 pr. ct.

Zinc.—Zinc is completely thrown down from its sulphate by the unmodified process. The oxalate forms an extremely fine powder. A sand filter and warm dilute sulphuric acid were employed.

0·9301 gr. sulphate required 47·1 c.c. hypermanganate = 28·14 pr. ct. ZnO.

1·0788 gr. sulphate required 54·6 c.c. hypermanganate = 28·15 pr. ct. ZnO.

The formula requires 28·22 pr. ct. ZnO.

Cobalt.—Perfectly pure anhydrous cobaltous chlorid was prepared by igniting chlorid of purpureo-cobalt. The chlorid was then precipitated by oxalic acid and alcohol, collected on a sand filter and digested with dilute sulphuric acid. The solution was intensely red. A solution of nickelous sulphate was then added, until the red color disappeared and a faint smoky hue took its place.* In this manner:

0·4292 gr. $CoCl_2$ required 47·8 c.c. hypermanganate = 45·30 pr. ct. Co.

0·3657 gr. $CoCl_2$ required 40·8 c.c. hypermanganate = 45·37 pr. ct. Co.

The formula requires 45·38 pr. ct. (Co = 59).

Nickel.—In the case of nickelous sulphate it was found necessary, after adding the oxalic acid, to concentrate the mixture on a water bath before adding alcohol, and then further digest for about half an hour, replacing the alcohol as fast as it evaporated. The oxalate was collected on a paper filter, and, after washing, dissolved in ammonia on the filter. The filtrate was then acidified with sulphuric acid, and the color finally discharged by a solution of cobaltous sulphate. In this manner:

0·9585 gr. nickelous sulphate required 42·2 c.c. hypermanganate = 28·57 pr. ct. NiO.

1·0287 gr. nickelous sulphate required 45·3 c.c. hypermanganate = 28·58 pr. ct. NiO.

The formula $NiSO_4 + 6H_2O$ requires 28·24 pr. ct. NiO (Ni = 58), but it is very difficult to obtain the sulphate in a perfectly definite state of hydration.

Manganese.—Although manganese is completely precipitated from its soluble salts by oxalic acid in the presence of alcohol, my results with the method have not been satisfactory, owing as I suppose to my not having a definite salt for analysis. The following analyses show at any rate that closely corresponding results can be obtained when the same substance is taken:

0·3760 gr. manganous oxalate required 30·50 c.c. hypermanganate = 38·38 pr. ct. MnO.

0·4013 gr. manganous oxalate required 32·55 c.c. hypermanganate = 38·38 pr. ct. MnO.

The salt $2C_2MnO_4 + 5H_2O$ when dried in air requires 37·77 pr. ct. MnO, while the salt analyzed was dried at 100° C. In like manner two analyses of a sulphate, which had probably absorbed a little water, gave 45·28 pr. ct. and 45·29 pr. ct. of MnO. The crystallized sulphate $MnSO_4 + 7H_2O$ requires 46·67 pr. ct.

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* Compare, as regards this method, W. Gibbs, in this Journal, vol. xiv, p. 204.

Iron.—Good results could not be obtained by the application of this method to the determination of iron, but I am not at present able to assign the reason of the failure in this case.

To complete my work it remains for me to point out the applicability of the process to the determination of the whole quantity of oxygen contained in a number of bases present together in solution—a problem which is sometimes of interest. Sulphates of Manganese, Magnesium, Nickel, Cobalt, Cadmium and Zinc in undetermined quantities were dissolved together in water, and the solution well shaken. Four portions, of 100 c.c. each, were then taken, and the acid determined in each by baric chlorid. In two other equal portions the bases were precipitated as oxalates and titrated as above. The oxygen in the acid was then calculated from the amount of baric sulphate. In this manner it was found that the oxygen in the acid was to that in the mixed bases as 3 to 1 very nearly, the precise ratio being in the one case as 0·054 is to 0·018 and in the other as 0·055 is to 0·018. Another experiment was made with a crystallized dolomite containing 0·45 pr. ct. of insoluble residue. The lime, magnesia and iron were precipitated together, and titrated as oxalates; the carbonic acid was determined by ignition. In this manner the oxygen of the acid was found to be to the oxygen in the bases as 34·48 is to 17·28. The bases after ignition amounted to 52·41 pr. ct.

In another experiment with a dolomite from a different locality containing 0·13 pr. ct. of insoluble residue, the oxygen ratio was found to be as 34·56 is to 17·28, the bases amounting to 52·33 pr. ct. If we calculate the relative quantities of calcic and magnesic carbonates from the sum of the two oxyds in the last analysis and the oxygen found by titrating the oxalate, we find:

$MgCO_3$	42·77
$CaCO_3$	57·07
Insol. residue	0·13
	99·97

the small quantity of ferrous oxyd present being neglected. This analysis will serve to show the applicability of the method in indirect analyses. My results appear to me to furnish a generalization of the use of potassic hypermanganate, which will be received with favor by those who employ this reagent frequently in volumetric analyses.

§ 2.

On new analytical processes; by J. H. Talbott.

1. *On the precipitation of zinc and manganese as sulphids.*—Zinc is thrown down from cold solution by an alkaline sulphid in the form of a slimy mass which settles slowly and is ex-

tremely difficult to wash. The precipitation is, however, more complete than when sodic carbonate is used, and may be rendered very easy and rapid by the following process: The solution of zinc, if acid, is to be neutralized as nearly as possible by sodic or ammonic carbonate. To the boiling solution sodic or ammonic sulphid is to be added, a large excess being very carefully avoided. The white precipitate, on continued boiling, soon becomes granular, and settles readily. The supernatant, clear liquid is then to be tested with a drop of the alkaline sulphid, to be sure of complete precipitation, and the sulphid then washed with hot water by Bunsen's method. The filtrate is perfectly clear, and absolutely free from zinc; the washing is easy and rapid. The sulphid of zinc is then to be partially dried with the filter, in the manner recommended by Bunsen, brought into a porcelain crucible and ignited, at first gently, and afterward strongly with free access of air. The expulsion of the last traces of sulphuric acid is much facilitated by occasionally dropping fragments of ammonic carbonate into the crucible. Pure ZnO finally remains, the ignition being continued until a constant weight is obtained. In this manner the following results were obtained:

0·3216 gr. pure ZnO gave 0·3216 gr. = 100·00 pr. ct.
0·3208 gr. " " " 0·3209 gr. = 100·03 pr. ct.
0·2412 gr. " " " 0·2410 gr. = 99·91 pr. ct.
0·1785 gr. " " " 0·1784 gr. = 99·94 pr. ct.

In zincic sulphate, which had probably lost a little water:

0·6485 gr. gave 0·1851 gr. ZnO = 28·54 pr. ct.
0·6510 gr. " 0·1858 gr. " = 28·54 pr. ct.
0·8198 gr. " 0·2338 gr. " = 28·52 pr. ct.

The formula requires 28·29 pr. ct. ZnO. The advantages of this process over the older methods of precipitating in the cold are, I think, very evident, even if only the saving of time be taken into consideration.

Manganese may be precipitated completely from its boiling solutions by precisely the same process. The flesh-colored sulphid is granular and sometimes even sandy, though not distinctly crystalline, and may be washed with the utmost facility. The precipitated sulphid, after washing upon a filter, is to be redissolved in chlorhydric acid, and precipitated as ammonio-phosphate in the manner proposed by Prof. Gibbs. To test the method with a perfectly definite salt of manganese, manganous pyrophosphate was selected, dissolved in dilute chlorhydric acid, and the solution nearly neutralized by sodic carbonate. To the boiling solution sodic sulphid was then added, and the manganese finally weighed—in one analysis as pyrophosphate, in another as anhydrous sulphid by ignition in a currrent of SH_2. In this manner:

0·3132 gr. $Mn_2P_2O_7$ gave 0·3126 gr. $Mn_2P_2O_7$ = 49·56 pr. ct. MnO.

0·3786 gr. $Mn_2P_2O_7$ gave 0·2310 gr. MnS = 49·65 pr. ct. MnO.

The formula requires 49·64 pr. ct. MnO. It is perhaps worthy of notice that ammonic sulphid does not completely decompose manganous pyrophosphate under the circumstances above described. The greater portion of the salt is precipitated at once as crystalline ammonio-phosphate of manganese.

2. *On the quantitative separation of tin and tungsten.*

The quantitative separation of tin from tungsten has always been regarded as a difficult problem not hitherto solved in a satisfactory manner. The following method will, I think, be found to leave nothing to be desired as respects both ease and accuracy. It depends upon the fact that stannic oxyd, SnO_2, is reduced by potassic cyanid with great facility, while tungstic acid, WO_3, undergoes no reduction, even when heated with the cyanid to a high temperature. The oxyds of tin and tungsten are to be heated in a porcelain crucible with 3 or 4 times their weight of commercial potassic cyanid, previously fused, pulverized, and thoroughly mixed with the two oxyds. The mass is kept fused for a short time, when the tin separates in the form of metallic globules, while the tungstic acid unites with the alkali of the potassic cyanate and carbonate present. After cooling, the mass is to be treated with hot water, which dissolves the alkaline tungstate and other salts, and leaves the tin as metal. This is to be filtered off, washed, dried and weighed as stannic oxyd after oxydation in the crucible with nitric acid. The tungstic acid is most conveniently estimated by the difference, but may of course be precipitated by mercurous nitrate, after boiling the solution with nitric acid to decompose the excess of potassic cyanid present, and then redissolving the precipitated tungstic acid by means of an alkali. To test the method, weighed portions of pure stannic and tungstic oxyds were mixed and treated as above:

0·6662 gr. SnO_2 and 0·5880 gr. WO_3 gave 0·6679 gr. SnO_2 = 53·24 pr. ct. The calculated percentage of stannic oxyd is here 53·11.

0·7098 gr. SnO_2 and 0·5460 gr. WO_3 gave 0·7096 gr. SnO_2 = 56·51 pr. ct., the calculated percentage being 56·52.

0·5378 gr. SnO_2 and 0·4373 gr. WO_3 gave 0·5405 gr. SnO_2 = 55·43 pr. ct., the calculated percentage being 55·15.

0·5073 gr. SnO_2 and 0·4334 gr. WO_3 gave 0·5081 gr. SnO_2 and 0·4349 gr. WO_3. This corresponds to

	Found.	Calculated.
Stannic oxyd,	54·01	53·92
Tungstic oxyd,	46·23	46·08
	100·24	100·00

As it might perhaps be objected to the examples given above that I employed only purely mechanical mixtures of stannic and tungstic acids, and that this is not the case which occurs in practice, I made the following additional analyses: Portions of the two metallic oxyds were fused in a silver crucible with pure sodic hydrate; the fused mass was then dissolved in water and the two oxyds precipitated together from the solution by nitric acid with the usual precautions. The ignited mixed oxyds were then fused with potassic cyanid, as above. In this manner:

0·7292 gr. of a mixture of SnO_2 and WO_3 gave 0·4211 gr. SnO_2 = 57·74 pr. ct.

0·9826 gr. of the same gave 0·5661 gr. SnO_2 = 57·61 pr. ct.

Tin cannot be separated from molybdenum by fusing the mixed oxyds with potassic cyanid, as the molybdic acid is always more or less completely reduced to a lower oxyd.

§ 3.

On the treatment of gelatinous precipitates; by THOMAS M. CHATARD.

The inconveniences and loss of time which attend the washing of gelatinous precipitates are familiar to all chemists. Even the methods of washing recently introduced by Bunsen are not always perfectly satisfactory in their operation when applied to this class of substances. The following method will be found, I think, to give results which leave nothing to be desired: The solution containing the substance to be determined is to be simply evaporated to perfect dryness with a small excess of the precipitant, and the gelatinous mass stirred with a rod until it becomes a perfectly dry powder. In this manner the precipitate diminishes extremely in bulk, and may then be washed with the greatest ease upon the filter. The evaporation is usually effected with sufficient rapidity on a water bath. The following analyses will be sufficient to show the degree of precision attainable by this process in the cases of some of the more familiar precipitates: Weighed portions of potassic dichromate were dissolved in very small portions of water, reduced with chlorhydric acid and alcohol, the excess of alcohol expelled, and ammonia added in excess. After evaporation, in the manner above described, the chromic sesquioxyd presented a greenish blue granular powder very easily washed.

0·7782 gr. $K_2Cr_2O_7$ gave 0·4023 gr. Cr_2O_3 = 68·02 pr. ct. chromic acid.
1·5646 gr. $K_2Cr_2O_7$ gave 0·8102 gr. Cr_2O_3 = 68·13 pr. ct. chromic acid.

The formula requires 68·04 pr. ct.

Alumina treated in the same manner is also very easy to wash.

2·4097 gr. potassic alum gave 0·2626 gr. Al_2O_3 = 10·89 pr. ct.
1·9571 gr. " " " 0·2130 gr. " = 10·88 pr. ct.

The formula requires 10·86 pr. ct.

The process applies with almost equal advantage to iron. Weighed portions of ammonio-ferrous sulphate were dissolved in water, and sodic chlorid added in large excess to furnish solid matter to be washed out. The iron was then oxydized with nitric acid, precipitated by ammonia, and evaporated as above.

1·5824 gr. gave 0·3229 gr. Fe_2O_3 = 14·28 pr. ct. Iron.
1·4840 gr. gave 0·3019 gr. " = 14·24 pr. ct. Iron.

The formula requires 14·29 pr. ct.

Nickelous carbonate also loses its gelatinous character when treated as above. 0·2201 gr. metallic nickel gave, after solution, precipitation as carbonate, and reduction by hydrogen, 0·2199 gr. metallic nickel = 99·91 pr. ct. of the quantity taken. Cobaltous carbonate may be treated in the same manner, but the alkali cannot be completely washed out, and the method is in this case not to be recommended.

It seems at least extremely probable that other gelatinous oxyds and hydrates will give equally good results when treated in the manner which I have described.

§ 4.

On the precipitation of antimonous sulphid from boiling solutions; by Stephen P. Sharples.

In the precipitation of antimonous sulphid I have found it of very great advantage to employ the following process: Into the solution, containing as usual tartaric and free chlorhydric acid, a current of sulphydric acid gas is to be passed, the liquid being, during the passage of the gas, gradually heated to the boiling point. The boiling is then to be continued for 15 or 20 minutes, the current of gas passing uninterruptedly, until the voluminous sulphid has become a dense granular powder, occupying but a small portion of the original volume of the sulphid. The sulphid may then be washed with great facility, and dried upon a sand filter at 200°–300° C. All the determinations of antimony made in this Laboratory for some years have been executed in this manner, the results leaving nothing to be desired. Arsenous sulphid does not become granular and dense under the same circumstances. In this connection I may be permitted to mention that the sulphids of nickel and cobalt, when precipitated from boiling solutions in the manner recommended by Prof. Gibbs some years since, should be filtered off, and washed immediately after precipitation. In this manner there is no oxydation upon the filter, even during the drying of the precipitate. But if the sulphids are allowed to stand in the solution from which they have been precipitated, for even a few hours, they will usually oxydize upon the filter during the washing.

§ 5.

On the introduction of the principle of repetition into chemical analysis; by Bryant Godwin.

The method of repetition, so frequently and so advantageously employed in physical investigations, has not, so far as I am aware, been applied to chemical analysis. It seems at least desirable that it should be so applied, and I will here give a particular instance in which it may be employed with advantage. In the determination of iron by means of potassic hypermanganate, all the iron is, at the end of the operation, in the form of sesquioxyd, while there is also a very small excess of unreduced hypermanganate. When the solution is boiled for a short time with pure zinc-dust and then filtered through a ribbed filter, which is quickly washed with water previously boiled to expel air, the iron is wholly in the form of ferrous oxyd, and the process of titration may be repeated a second time. After a new reduction the iron may again be determined, and this process may be repeated until the volume of liquid becomes too large to be easily handled. The following analyses were made to test this process:

0·4725 gr. ammonio-ferrous sulphate required in 5 successive titrations 49·0, 47·2, 48·7, 48·0, 48·5 cubic centimeters of potassic hypermanganate, 1 c.c. corresponding to 0·0014 gr. iron. The mean of these 5 determinations gives 14·31 pr. ct. iron in the salt.

0·4888 gr. required in 7 successive titrations 49·5, 48·75, 50·5, 49·8, 49·7, 49·8, 49·5 c.c. of hypermanganate, the mean of which gives 14·23 pr. ct. iron.

The formula requires 14·27 pr. ct.

These analyses, which with more practice and experience on my part would doubtless have corresponded much more closely, will at least serve to show that the principle of repeated observations of the same quantity to be measured may sometimes be introduced into chemical analysis.

ART. XXVI.—*On the Corona seen in Total Eclipses of the Sun;* by Professor W. A. NORTON.

CERTAIN observations made on the total eclipse of the sun of August 7, 1869, have led some of the observers to the conclusion that the corona seen on that occasion, and in previous eclipses, is of the nature of a *Solar Aurora.* It is proper that it should be publicly stated that this theory is not a new one. It has been advocated for several years by the author of the present communication, both in publications and in public lectures. It is essentially involved in the explanation of the Zodiacal Light, propounded in his Treatise on Astronomy, 2nd edit. (1845); and in the theoretical views set forth in a note appended to the discussion of the topic of Terrestrial Magnetism, in a memoir on Molecular Physics, published in this Journal (1864–6).* It is distinctly presented in the last edition of the Treatise on Astronomy (1867), pp. 172, 174, 175, and 178. I propose, in a communication to the next No. of this Journal, to state the principal grounds upon which I have maintained the auroral origin of the corona in different publications, as well as give the results of my own observations on the eclipse of 1869, and of those of other observers of that and previous total eclipses, which lend a powerful support to the auroral theory of the corona.

This introductory notice is now published mainly with the view of calling, at an early day, the attention of astronomers who may observe the eclipse of December next, to the importance of noting the exact positions, with respect to the plane of the sun's equator, of the more prominent portions of the corona. From two to four points of special outstreaming have been observed in different eclipses. In the eclipse of last year the more conspicuous extensions of the corona were nearly in the plane of the sun's equator, and from the vicinity of the poles. The figure of the corona, accompanying the Report of P. Prof. Capelotti of observations on the eclipse of April 15, 1865, made at Chili, shows the same to have been the case in that eclipse. The delineations of the corona as seen in other eclipses, so far as I have been able to ascertain, fail to give any accurate indication of the positions of the more prominent parts from the absence of all lines of reference in the drawings. It is to be hoped that observers of subsequent eclipses will take the precaution to ascertain these positions, and note them in their Reports. If they really have any general uniformity, in different eclipses, the fact cannot fail to throw light on the origin and nature of the corona.

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* See this Journal, vol. xli, No. 121, pp. 76, 77.

ART. XXVII.—*Observations on Prehistoric Archæology in Greece;* by GEORGE FINLAY, LL.D.*

A FEW objects belonging to the stone period were observed in Greece before it was known that they are relics of the people who inhabited the country in prehistoric times. The pieces of obsidian, generally called flint by travelers, that were picked up on the tumulus of Marathon, were termed Persian arrow-heads. This arose from a strange misapplication of the mention of stone arrow-heads having been employed by the Ethiopians in the army of Xerxes by Herodotus (Polymnia, vii, 69), who says they used short arrows of reeds pointed with a stone with which they engraved their signets. But why Datis, who can hardly have had any Ethiopians in his army, thought it expedient to bring to Marathon immense quantities of stone arrow-heads has not been explained. They do not appear to have been likely to prove efficient missiles against Athenian hoplites. Yet a sagacious traveler like Dodwell, in 1805, says that he found "a great many small arrow-heads of black flint, which probably belonged to the Persian army."† Even Colonel Leske, the ablest and most observant of modern travelers, was misled by this opinion. He says, "while I was employed on the summit of the *Soros*, as the tumulus of the Athenians is called, my servant amused himself in gathering at the foot of the barrow, a great number of pieces of black flint which happened to strike his observation. These flints are so numerous, and have been so evidently chipped by art into their present form like gun flints, that there is good reason for believing them to have been the heads of arrows discharged by the Persians who fought at Marathon, and to have been interred with the Athenians after having been gathered from every part of the plain after the battle. Herodotus shows that some of the barbarians were armed in this manner, though his remark is applied not to the army of Darius but to that of Xerxes. Flint of this kind, if produced in any of the adjacent parts of Greece is at least very rare." (*Travels in Northern Greece*, vol. ii, p. 431). The great quantity and small size of the fragments found in the tumulus of Marathon caused the writer of these pages to doubt the possibility of these fragments having anything to do with the Persians, for such feeble weapons as they could form must have been useless against the panoply of the Greek infantry of the period. Had they been employed the glory of Marathon would be a vain boast. Sir William

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* From a letter to Mr. H. T. DeForest, dated Athens, Feb. 26, 1870, communicated by him for this Journal.

† A classical and topographical tour through Greece during the years 1801, 1805 and 1806, by Edward Dodwell, Esq., vol. ii, p. 159.

Gell, in his *Itinerary of Greece*, page 166, mentions that similar fragments of flint are found at the *σχίστη ὀδὸς*, where was the tomb of Laius, and he adds "perhaps a confirmation of the discomfiture of the barbarians in the *Odos schiste.*" These fragments of obsidian, wherever they are found in Greece, are now admitted to be relics of prehistoric times, and a careful examination of the tumulus of Marathon convinced the writer of these pages, as early as the year 1835, that they were scattered about in the soil in their actual state when it was heaped up to form the tumulus over the bodies of the Athenians who were slain in the battle. The material is obsidian from the island of Melos. * * * *

No traditions of the existence of a stone period appear to have reached the inhabitants of Greece in historic times, though the mythical history of the remains of Tyrinths and Lykosura ascend almost to the prehistoric ages. As I have already mentioned, my attention was first called to the certainty that a numerous race of people in Greece used stone implements by the fragments of obsidian picked up on the tumulus at Marathon. I subsequently observed that similar fragments of obsidian are found in various parts of the neighborhood in the rear of the Greek position, and far out of reach of the arrows of the Persians. I also found myself similar chips of obsidian over all Attica, and in many parts of Greece, and several of the islands of the Archipelago, where no native obsidian can ever have existed, which I visited after my attention was directed to the subject. I have picked up these so-called Persian arrow-heads even in the now barren island of Hydra. My first notice on the subject was published in the year 1836. In that year, while examining the topography of Attica I discovered the extensive deposit of fossil bones at Pikermi, of which there is a valuable collection in the Museum of Natural History at Athens. A detailed description of these remarkable fossil bones was published by Professor Roth of Munich, in the Transactions of the Royal Academy of Bavaria, and they have since been described in the splendid work of Monsieur A. Gaudry, *Animaux fossiles et Geologie de l'Attique.* In a notice of the discovery which I read at a meeting of the Society of Natural History of Athens on 13th December, 1836, I observed that I had picked up fragments of obsidian, called Persian arrow-heads, not only on the tumulus of Marathon but also at Liosia near Aphidna, at Kakosialesi near Tanagra, and at Aghios Kosmas on the Attic coast. When my memoir on the battle of Marathon, which was read to the Royal Society of Literature in January, 1838, was printed, I added a note "concerning the pieces of flint called Persian arrow-heads found in the tumulus at Marathon." (Transactions of the Royal Society of Literature, first series, 4to, vol. iii, p. 392).

I have since collected several specimens of stone implements, particularly celts, that is, axes and chisels very similar in form to those found in the lake dwellings of Switzerland. I have obtained six specimens of jade, one of which I have presented to the Museum, with a similar piece which I procured at Robenhausen. I have also a fine small axe of nephrite, and several others of extremely hard stones. Unfortunately it has not yet been in my power to ascertain the precise mineralogical character of the most remarkable of my specimens.

I have obtained several fine specimens from Dobrena (the site of the ancient Thisbe) where a lake must have existed in prehistoric times, and where there is still a marsh. Two small axes found at Orchomenas, near the lake Copaïs, were given to me by Mr. Merlin, Her Majesty's Consul for Northern Greece.

Lake dwellings continued to exist in Macedonia down to the time of Herodotus. His description (Terpsichore, v, 16) proves that the dwellings of the Pæonians, in the Lake Prasias, were very similar to those constructed on the lakes of Switzerland. "They who dwell on the lake Prasias construct their dwellings in this manner. They fix strong piles in the lake, and on these piles they fasten planks, making a bridge with a narrow entrance from the land. The piles supporting the planks were in former times fixed by the inhabitants in common, but afterward the law established that every one who married a wife (and they take many wives), should bring down from Mount Orbelos three piles and fix them in the lake. The manner of their dwellings is in this fashion. Each man has his own hut on the piles, and a trap door through the flooring by which he can descend to the lake. The young children are tied by the foot lest they should fall into the water." The lake Prasias is the lake of the Strymon of Thucydides, v, 7, the Kerkinites of Arrian, Anab. 1, 11, 3, and is now called Tachynós, from a village on its western side. The fisheries are still valuable as they were in ancient times, and the fish caught for sale are principally carp, tench and eels. (Leake, Travels in Northern Greece, vol. iii, 198).

The description which Herodotus gives of these lake dwellings makes it an object of the greatest importance to the archælogists of Switzerland and Greece that the lake Prasias and other lakes in Macedonia, Thessaly and Greece, should be carefully examined in order to ascertain whether any traces can still be discovered of lake dwellings. Some traces of the piles on which dwellings were constructed are said to have been observed in the lake Prasias, in 1862, by Monsieur Deville of *l'Ecole française d'Athénes.* But a superficial examination might easily lead to considering stakes for nets or fishing huts as remains of ancient pile dwellings, and a searching examination of the locality ought to be undertaken by skillful and experienced observers.

The lakes in Greece that deserve particular attention are, the lake Copais, Hylica or Livadi, and Paralimni in Bœotia, the lake Trichonis with its connected lake Hyrie in Etalia, the lakes in Acarnania and the lagoons between the mouths of the Evenus and the Achelous. In the Peloponnesus there are, the lakes of Pheneus (which becomes in alternate periods of years a deep lake as at present, and a plain that dries in summers as it was in the year 1821), and Stymphalus, with their physical peculiarities and mythical associations running back toward a prehistoric period. The lake of Orchomenos, the marsh of Mantinea, the lakes near Tegea, the lagoons at the mouths of the Eurotas and the Alpheus, and the marsh at Pylos, (Palæo Avarino) all these places, and some others that might be pointed out, offer an extensive field for research. It may also be possible to identify sites of prehistoric habitations in the mountains, from the remains found in their vicinity. Such positions would have been selected because they were easily defensible by men having weapons of stone only. They must have commanded access to an abundant supply of water equally capable of defense. I have observed such a position overlooking the plain of Aphidna, where I have picked up a considerable quantity of fragments of obsidian and flint artificially worked.

Pliny contains several passages in his Natural History that refer to stone axes and chisels (celts), with particular reference to those found in Greece, for he quotes Greek authorities about them. He speaks of *Ceraunia* (thunderbolts) as being, according to the testimony of Sotacus (an ancient Greek writer on minerals), black and red, and resembling axe-heads in shape. I have specimens of red celts from Eubœa made of red ironstone, one 3¼ in. long and 2 in. broad; and several that are black, of the same size but narrower.

The stone period has been divided into a paleolithic and a neolithic period. In western Europe, particularly in France and England, numerous remains of stone implements of the paleolithic period have been found in strata with the bones of the mammoth and other extinct animals. But I am not aware that any stone implements that can be attributed with any certainty to this period, have yet been discovered in Greece, though bones of these animals have been found in several places in great quantities both in Attica, Eubœa and Arcadia. All the stone implements that have fallen under my notice consist of specimens that belong to the neolithic or polished-stone period, and many display considerable skill in their workmanship, being composed of the hardest stones.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the theory of the Bunsen flame.*—The non-luminosity of the flame of the Bunsen gas-burner is commonly ascribed to the more complete combustion of the gas by the air which is mixed with it before it is burned. But this is an assumption entirely unproved as yet; and moreover, the positive experiments of Knapp show that this cannot be the only cause of the phenomenon. His burner was so constructed that by means of a lateral tube, other gases beside air could be mixed with the coal-gas; and the results obtained prove that nitrogen, hydrochloric acid, or carbon dioxyd gas causes the flame to burn fully as blue as is seen in the burner as ordinarily used. The experiment is most easily tried with carbon dioxyd gas; but with this gas it might be urged that dissociation into carbonous oxyd and oxygen takes place, and that the latter gas assists the combustion. Though this is hardly conceivable at the low temperature there present, the assumption may be entirely disproved by using in the experiment only pure nitrogen; which for this purpose may be conveniently obtained by boiling a solution containing equivalent quantities of potassium nitrite and ammonium chlorid. The explanation of this result is not easy; Knapp believes that the disappearance of the luminosity is due partly to the cooling of the flame, but principally to the dilution of the illuminating gas; and that the flame of the Bunsen burner is non-luminous from the same cause which lessens the light of a candle burned in vacuo, or at high altitudes.—*J. Pr. Ch.*, II, i, 428, June, 1870. G. F. B.

2. *Production of Ozone in Rapid Combustion.*—The observation of Loew upon the production of ozone in quick combustion, published in the May number of this Journal, seems to have been anticipated not only by Pincus (noticed in vol. xlvii, page 238,) but much earlier—in December, 1864—by Than, and communicated to the Hungarian Academy at Pesth. Becoming interested in the application to vegetable physiology of the fact that ammonium nitrite is formed when hydrogen burns in an atmosphere containing nitrogen, Than sought for a method of exhibiting the result as a lecture-experiment; for this purpose he drew the air which closely surrounds the flame of a Bunsen burner through a Varrentrapp and Will's bulb-apparatus, containing an acidulated solution of potassium iodid and starch. The air which was drawn from about the lower ⅔ of the flame, colored the solution blue very rapidly. As the odor of ozone was not detectable in this air after it had passed through the apparatus, Than substituted a bulb-tube filled with water; and then on drawing through this a rapid current of the air from this part of the flame, the odor of ozone was distinctly obtained. Were ammonium nitrite formed at the same time, it would be retained by the water; and this, on adding a

few drops of the acidulated potassium iodid solution, would become at once blue. But no blueing took place, even when the air was passed through the water for 15 or 20 minutes. The blueing in the former case must be due, therefore, entirely to ozone. Than next investigated the conditions of this ozone-production. He found that a small quantity of ozone is present in the air which surrounds the lower portion of every flame composed of burning hydrogen; that this ozone may be detected by withdrawing this air rapidly from the flame through a glass tube drawn out to a fine point, when it gives its characteristic odor and reactions; that the point of the tube should not exceed a millimeter in diameter, and that it should be placed in the lower half of the flame where the flame comes in contact with the cold air. In a candle-flame, or that of an alcohol-lamp, most ozone is found near the lower blue portion. The current of air must be rapid enough to deflect the flame, yet not so rapid as to draw in any unburned gas. By the ozone thus obtained, the solution of potassium iodid and starch was blued in a few seconds, a mixture of manganous sulphate solution with a few drops of potassium hydrate became dark brown from the formation of the peroxyd, and when inhaled, the mucous membrane was irritated and a transient catarrh produced. No ozone could be detected in the air surrounding ignited charcoal; and all attempts to determine it in the other cases quantitatively, failed. Than then goes on to say: "Wenn man durch das obere Drittheil einer nicht leuchtenden Gasflamme, durch ein schief aufwärts gerichtete zwei Linien weite Glasröhre, einen sehr kräftigen Luftstrom mittelst eines Blasebalges durchtreibt, so kann man in der durchgeblasenen Luft nicht unbedeutende Mengen von Ozon theils durch den Geruch, theils durch Jodkalium-Stärkepapier nachweisen;" an experiment almost precisely that mentioned by Loew. In Than's view, the diatomic molecule of oxygen in the air, when it comes in contact with the hydrogen of the flame, gives up one atom, to form water, thus: $H_2+O_2=H_2O+O$; the other atom thus set free, unites with another molecule of oxygen to form ozone: $O_2+O=O_3$. No ozone is formed in the vicinity of the burning carbon, because each of its atoms unites directly with an entire molecule of oxygen, $C+O_2=CO_2$, producing carbon dioxyd.—*J. Pr. Ch.*, II, i, 415, June, 1870. G. F. B.

3. *On the Constitution of Narcotine and its decomposition-products.*—Matthiessen thus sums up the results of his extended research upon Narcotine:—

(1.) The analysis of various specimens of narcotine, derived from various sources, has shown that it always has the same composition, $\text{Ȼ}_{22}H_{23}N\Theta_7$.

(2.) As already noticed by previous observers, narcotine, by the action of oxydizing agents, splits up into opianic acid and cotarnin:

$$\text{Ȼ}_{22}H_{23}N\Theta_7+\Theta=\text{Ȼ}_{10}H_{10}\Theta_5+\text{Ȼ}_{12}H_{13}N\Theta_3.$$

(3.) Heated alone, to a temperature somewhat above 200°, or for a somewhat longer time with water, narcotine splits up into meconin and cotarnin:

$$\text{Ꞓ}_{22}H_{23}N\Theta_{7}=\text{Ꞓ}_{10}H_{10}\Theta_{4}+\text{Ꞓ}_{12}H_{13}N\Theta_{3}.$$

(4.) Heated for a short time (about two hours) with an excess of hydrochloric acid, methyl chlorid is formed, and one atom of hydrogen replaces the methyl in the narcotine; by a longer heating (for some days) two atoms of H may replace two of $\text{Ꞓ}H_3$; heated with fuming hydriodic acid, three atoms of the methyl are replaced by three of hydrogen; thus forming a series of homologous bases, whose decompositions are analogous to those of narcotine itself.

(5.) The formula of cotarnin has been shown to be $\text{Ꞓ}_{12}H_{13}N\Theta_{3}$, and not $\text{Ꞓ}_{13}H_{13}N\Theta_{3}$; it is capable of crystallizing with one-half and with a whole molecule of water.

(6.) Cotarnin, when heated with dilute nitric acid under certain, not yet clearly-defined conditions, is decomposed, yielding cotarnic acid and methylamine:

$$\text{Ꞓ}_{12}H_{13}N\Theta_{3}+2H_{2}\Theta=\text{Ꞓ}_{11}H_{12}\Theta_{5}+\text{Ꞓ}H_{5}N.$$

By the use of concentrated nitric acid, as already noticed by former observers, apophyllic acid is formed. The action of other oxydizing agents gives results not yet studied.

(7.) Cotarnin heated with concentrated hydrochloric acid, yields methyl chlorid and the hydrochlorate of cotarnamic acid:

$$\text{Ꞓ}_{12}H_{13}N\Theta_{3}+H_{2}\Theta+2HCl=\text{Ꞓ}H_{3}Cl+\text{Ꞓ}_{11}H_{13}N\Theta_{4}, HCl.$$

Hydriodic acid produces a similar result; only one atom of $\text{Ꞓ}H_3$ being eliminated from each molecule of cotarnin.

(8.) Opianic acid, by the action of nascent hydrogen (as by treatment with sodium-amalgam or by zinc and sulphuric acid) is reduced to meconin:

$$\text{Ꞓ}_{10}H_{10}\Theta_{5}+H_{2}=H_{2}\Theta+\text{Ꞓ}_{10}H_{10}\Theta_{4}.$$

(9.) Opianic acid by heating with potassium dichromate and dilute sulphuric acid is oxydized to hemipinic acid:

$$\text{Ꞓ}_{10}H_{10}\Theta_{5}+\Theta=\text{Ꞓ}_{10}H_{10}\Theta_{6}.$$

(10.) Opianic acid, on heating with caustic potash, splits up into meconin and hemipinic acid:

$$2\text{Ꞓ}_{10}H_{10}\Theta_{5}=\text{Ꞓ}_{10}H_{10}\Theta_{4}+\text{Ꞓ}_{10}H_{10}\Theta_{6}.$$

(11.) Opianic acid, heated with an excess of hydrochloric acid, exchanges its methyl for hydrogen. Beside methyl chlorid, two substances are probably thus produced, noropianic acid and methyl-noropianic acid; the first by a single, the second by two replacements of methyl by hydrogen.

$$\text{Ꞓ}_{10}H_{10}\Theta_{5}+2HCl=2\text{Ꞓ}H_{3}Cl+\text{Ꞓ}_{8}H_{8}\Theta_{5}.$$

$$\text{Ꞓ}_{10}H_{10}\Theta_{5}+HCl=\text{Ꞓ}H_{3}Cl+\text{Ꞓ}_{9}H_{8}\Theta_{5}.$$

Only the latter has been obtained pure, the former spontaneously decomposing. Hydriodic acid acts similarly. Methyl-noropianic acid, like opianic acid, is monobasic.

(12.) All attempts to oxydize meconin to opianic or hemipinic acid, or to any other product, were unsuccessful.

(13.) Meconin, treated with an excess of hydrochloric or hydriodic acid, yields methyl chlorid or iodid, and a body derived from meconin by replacing $ЄH_3$ by H, methyl-normeconin:

$$Є_{10}H_{10}Θ_4+HCl=ЄH_3Cl+Є_9H_8Θ_4.$$

Experiments to produce the hypothetical normeconin by substituting H_2 for $(ЄH_3)_2$, gave nothing capable of being isolated in a pure state.

(14.) Hemipinic acid, by treatment with various reducing agents, was in no case reduced to opianic acid or meconin. So, experiments to obtain opianic acid from the union of hemipinic acid and meconin, have yielded no result. Moreover, hemipinic acid could not be oxydized to any other product.

(15.) Heated with an excess of hydrochloric acid, hemipinic acid yields, beside methyl chlorid and carbon dioxyd, a new acid, methyl-hypogallic acid:

$$Є_{10}H_{10}Θ_6+HCl=ЄH_3Cl+ЄΘ_2+Є_8H_8Θ_4.$$

Heated with hydriodic acid, it affords methyl iodid, carbon dioxyd and hypo-gallic acid:

$$Є_{10}H_{10}Θ_6+2HI=2ЄH_3I+ЄΘ_2+Є_7H_6Θ_4.$$

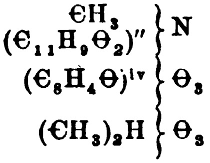
(16.) Anderson's observations proving hemipinic acid to be dibasic are confirmed, the anhydrid being obtained by simple drying:

$$Є_{10}H_{10}Θ_6=H_2Θ+Є_{10}H_8Θ_5.$$

Methyl-hypogallic acid is also dibasic.

(17.) Hemipinic acid may crystallize with various quantities of crystal water; crystals having been obtained with ½, with 1, and with 2 molecules of such water.

(18.) All the reactions of narcotine and of its decomposition products, are explained in a satisfactory manner by assuming for it the following rational formula:



$$\left.\begin{array}{r} ЄH_3 \\ (Є_{11}H_9Θ_2)'' \end{array}\right\} N \quad \left.\begin{array}{r} (Є_8H_4Θ)^{iv} \end{array}\right\} Θ_3 \quad \left.\begin{array}{r} (ЄH_3)_2H \end{array}\right\} Θ_3$$

—*Ann. Ch. Pharm.*, Suppl. Band vii, 66, Nov., 1869. G. F. B.

4. *On the size of Molecules;* by Sir WM. THOMSON. (Proc. Lit. and Phil. Soc. Manchester, March 22; Nature, May 19, p. 56.)—My occupation on the Kinetic theory of gases has led me at last to come to definite terms as to the size of molecules. Ever since about the first year of my professorship I have taught my students that Cauchy's theory of Dispersion proves heterogeneousness, or molecular structure, to become sensible in contiguous portions of glass or water, of dimensions moderately small in comparison with the wave-lengths of ordinary light. I have spoken to you also, I think, of the argument deducible from the contact electricity of metals. This, I now find, proves a limit to the dimensions of the

molecules in metals quite corresponding to that established for transparent solids and liquids by the dynamics of dispersion. In experiments made about ten years ago, of which a slight sketch is published in the Proceedings of the Literary and Philosophical Society of Manchester, I found that a plate of zinc and a plate of copper kept in metallic connection with one another (by a fine wire or otherwise) act electrically upon electrified bodies in their neighborhood, and upon one another, as they would if they were of the same metal and kept at a difference of potentials equal to about three-quarters of that produced by a single cell of Daniell's. Hence, and from my measurement of the electrostatic effects of a Daniell's battery, published in the Proceedings of the Royal Society, for February and April, 1860, I find that plates of zinc and copper held parallel to one another at any distance, D, apart which is a small fraction of the linear dimensions of their opposed surfaces, and kept in metallic communication with one another, exercise a mutual attraction equal to

$$2\times10^{-10}\times\frac{\mathrm{A}}{\mathrm{D}^2} \text{ grams weight.}$$

Hence, if they were allowed to approach from any greater distance, D′, to the distance, D, the work done by their mutual attraction is

$$2\times10^{-10}\times\frac{\mathrm{A}(\mathrm{D}'-\mathrm{D})}{\mathrm{D}'\mathrm{D}} \text{ centimeter grams;}$$

which, if D is very small in comparison with D′, is very approximately equal to $2\times10^{-10}\times\frac{\mathrm{A}}{\mathrm{D}}$.

Now suppose a pile to be made of a great number (N+1) of very thin plates alternately of zinc and copper, kept in metallic connection while they are brought toward one another. Let their positions in the pile be parallel, with narrow spaces intervening. For simplicity, let the thickness of each metal plate and intervening space be D. The whole work done will be

$$2\times10^{-10}\times\mathrm{N}\,\frac{\mathrm{A}}{\mathrm{D}}.$$

The whole mass of the pile (if we neglect that of one of the end plates) is NADϱ, where ϱ denotes the mean of the densities of zinc and copper. Hence, if h be the height to which the whole mass must be raised against a constant force equal to its weight at the earth's surface, to do the same amount of work, we have

$$\mathrm{NAD}\varrho h=2\times10^{-10}\times\mathrm{N}\,\frac{\mathrm{A}}{\mathrm{D}};$$

which gives $h=\frac{2\times10^{-10}}{\varrho\mathrm{D}}$,

or, as $\varrho=8$, nearly enough for the present rough estimate,

$$h=\frac{1}{(200000\mathrm{D})^2}.$$

Hence, if $D = \frac{1}{200000}$ centimeter,
$h = 1$ centimeter.

The amount of energy thus calculated is not so great as to afford any argument against the conclusion which general knowledge of divisibility, electric conductivity, and other properties of matter indicates as probable; that, down to thicknesses of $\frac{1}{200000}$ of a centimeter for the metal plates and intervening spaces, the contact electrification, and the attraction due to it, follow with but little if any sensible deviation the laws proved by experiment for plates of measurable thickness with measurable intervals between them. But let D be a two-hundred-millionth of a centimeter. If the preceding formulæ were applicable to plates and spaces of this degree of thinness we should have

$$h = 1{,}000{,}000 \text{ centimeters or 10 kilometers.}$$

The thermal equivalent of the work thus represented is about 248 times the quantity of heat required to warm the whole mass (composed of equal masses of zinc and copper) by 1° Cent. This is probably much more than the whole heat of combination of equal masses of zinc and copper melted together. For it is not probable that the compound metal when dissolved in an acid would show anything approaching to so great a deficiency in the heat evolved below that evolved when the metallic constituents are separately dissolved, and their solutions mixed; but the experiment should be made. Without any such experiment, however, we may safely say that the fourfold amount of energy indicated by the preceding formula, for a value of D yet twice as small, is very much greater than any estimate which our present knowledge allows us to accept for the heat of combination of zinc and copper. For something much less than the thermal equivalent of that amount of energy would melt the zinc and copper; and, therefore, if in combining they generated by their mutual attraction any such amount of energy, a mixture of zinc and copper filings would rush into combination (as the ingredients of gunpowder do) on being heated enough in any small part of the whole mass to melt together there. Hence, we may infer that the electric attraction between metallically-connected plates of zinc and copper of only $\frac{1}{400000000}$ of a centimeter thickness, at a distance of only $\frac{1}{400000000}$ of a centimeter asunder, must be greatly less than that calculated from the magnitude of the force and the law of its variation observed for places of measurable thickness, at measurable distances asunder. In other words, plates of zinc and copper so thin as a four-hundred-millionth of a centimeter from one another, form a mixture closely approaching to a molecular combination, if, indeed, plates so thin could be made without splitting atoms. Wishing to avoid complication, I have avoided hitherto noticing one important question as to the energy concerned in the electric attraction of metallically-connected plates of zinc and copper. Is there not a change of temperature in molecularly thin strata of the two metals adjoining to the opposed surfaces, when they are allowed to approach one another, analogous to the heat produced by the condensation of a gas, the changes of temperature produced by the application of stresses to elastic solids which you have investigated experimentally, and the cooling effect I have proved to be produced by drawing out a liquid film which I shall have to notice particularly below? Easy enough experiments on the contact electricity of metals will answer this question. If the contact-difference diminishes as the temperature is raised, it will follow from the Second Law of Thermodynamics, by reasoning precisely corresponding with that which I applied to the liquid film in my letters to you of February 2d and February 3d, 1858,* that plates of the two metals kept in metallic communication and allowed to approach one another will experience an elevation of temperature. But if the contact-difference increases with temperature, the effect of mutual approach will be a lowering of temperature. On the former supposition, the diminution of intrinsic energy in quantities of zinc and copper, consequent on mutual approach with temperature kept constant, will be greater, and on the latter supposition less, than I have estimated above. Till the requisite experiments are made, further speculation on this subject is profitless; but whatever be the result, it cannot invalidate the conclusion that a stratum of $\frac{1}{200000000}$ of a centimeter thick cannot contain in its thickness many, if so much as one, molecular constituent of the mass. Besides the two reasons for limiting the smallness of atoms or molecules which I have now stated, two others are afforded by the theory of capillary attraction, and Clausius' and Maxwell's magnificent working out of the Kinetic Theory of gases. In my letters to you already referred to, I showed that the dynamic value of the heat required to prevent a bubble from cooling when stretched is rather more than half the work spent in stretching it. Hence, if we calculate the work required to stretch it to any stated extent, and multiply the result by $\frac{3}{2}$, we have an estimate, near enough for my present purpose, of the augmentation of energy experienced by a liquid film when stretched and kept at a constant temperature. Taking ·08 of a gram weight per centimeter of breadth as the capillary tension of a surface of water, and therefore ·16 as that of a water bubble, I calculate (as you may verify easily) that a quantity of water extended to a thinness of $\frac{1}{200000000}$ of a centimeter would, if its tension remained constant, have more energy than the same mass of water in ordinary condition by about 1,100 times as much as suffices to warm it by 1° Cent. This is more than enough (as Maxwell suggested to me) to drive the liquid into vapor. Hence, if a film of $\frac{1}{20000000}$ of a centimeter thick can exist as liquid at all, it is *perfectly certain* that there cannot be many molecules in its thickness. The argument from the Kinetic Theory of gases leads me to quite a similar conclusion.

5. *Comparison of Mechanical Equivalents;* by PLINY EARLE CHASE, (Proc. Am. Phil. Soc., xi, 313, 1870).—The comparison of different mechanical equivalents will open a new field for investigation, which may prove to be fertile in valuable results. For

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* Proceedings of the Royal Society for April, 1858.

example, recent determinations, by the different methods of Thomsen and Farmer, fix the mechanical equivalent of light, in a wax candle burning 126½ grains per hour, at 13·1 foot-pounds per minute, the equivalent of 1 grain being 6·213 foot-pounds. According to Dulong, the heat evolved during the combustion of 1 grain of olive oil in oxygen, is sufficient to heat 9862 grains of water 1° C. According to Favre and Silbermann, 1 grain of oil of turpentine, burned in oxygen, would heat 10,852 grains of water 1° C.

It may therefore be presumed that the total heat given out by the combustion of 1 grain of wax, is about sufficient to raise 10,000 grains or water 1° C., or 18,000 gr. 1° F. This represents a mechanical equivalence of $(18{,}000 \times 772 \div 7000 =)$ 1985·143 foot-pounds, which is 319·5 times as great as the corresponding equivalent of the light given out during the combustion.

Tyndall, in his lecture on Radiation, states that the visible rays of the electric light contain about one-tenth of the total radiated heat. The relative luminous intensity of an electric lamp would therefore appear to be about 32 times as great as that of the wax candle. This ratio so nearly resembles that of solar to terrestrial superficial attraction, and the connection of electric and magnetic currents with solar radiation is so evident, that additional experiments, to furnish materials for a great variety of similar comparisons, seem desirable. While it is possible that the resemblance, in the present instance, may be accidental, the numerous harmonies between the manifestations of cosmical and molecular forces render it at least equally possible that it may have a weighty significance.

II. GEOLOGY AND MINERALOGY.

1. *On a Fossil Tooth from Table Mountain;* by Prof. William P. Blake. (Communicated by the author for this Journal.)—The fossil tooth, found by Mr. D. T. Hughes, 1,700 feet under Table Mountain, and 300 feet below the surface, I have carefully examined and compared with specimens in the Smithsonian Institution. It proves to be a back lower molar of an equine animal of the genus *Hipparion*, or a closely allied genus. This genus is one of the connecting links between the *Palæotherium* and the horse.

The specimen closely resembles a fossil in the Smithsonian museum, from the Pliocene formations of the Niobrara river in Nebraska,* not only in size but in the foldings of the enamel, and particularly in the posterior part of the tooth, but it differs enough, in several particulars, to justify the belief that it is a distinct species. Dr. Leidy does not attempt to determine, specifically, the specimen from Nebraska, but considers it closely related to, if not identical specifically with, *Hipparion gratum*, possibly *Protohippus placidus*.

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* Described by Prof. Leidy in his work upon the Extinct Mammalian Fauna of that region, p. 319, pl. xix, fig. 7,

The size of the Table Mountain specimen, which is considerably worn by attrition in the gravel, is: length, 11 lines; breadth upon the crown, 9 lines; breadth at the base, 10 lines; thickness, anteriorly, 4 lines, posteriorly, 2 lines.

This fossil is the first of the kind discovered west of the Rocky Mountains. It adds to the list of the fauna of the period antedating Table Mountain—a list which includes the mammoth (*Elephas*, from Knight's Ferry), the rhinoceros, and an animal allied to the elk. I have believed that remains of man were also found under the lava; but upon this point, after diligent inquiry, I am satisfied that the evidence is insufficient. But we now add this fossil allied to *Hipparion*, and I regard it as another indication that the Table Mountain beds are Pliocene, and homotaxial with those of the Bad Lands of Nebraska.

2. *Cause of the Descent of Glaciers.*—Rev. HENRY MOSELEY, before the Royal Society in January, 1869, in the Philosophical Magazine for the May following, and at the meeting of the Royal Institution of Great Britain, May 13, 1870, opposes the view that glaciers owe their movement mainly to gravity, and gives as the principal cause contraction and expansion due to change of temperature through the mass. He compares the movement to that of a sheet of lead on a sloping surface, in its expansion the lower edge working downward, and in its contraction the upper edge or part.

In the Phil. Mag. for July, 1870, Mr. John Ball, after alluding to the criticisms on the above theory by Mr. Wm. Mathews in the Alpine Journal for February last, shows that the supposed contraction and expansion to which Canon Moseley appeals, does not take place, and that the "crawling theory" of glacier motion is therefore unsatisfactory. He argues that the glacier is not a continuous solid mass like a sheet of metal; that the temperature of the interior, as observers have proved, is very nearly constant; that the movement is half as fast in winter as in summer; that the rate of motion is not proportioned at all to the length of the glacier as it should be by the theory; that the supposed expansion and contraction, if a fact, would exceed twenty or more times the rate of actual motion but for modifying causes,—and this is an amount of modifying intervention for the sake of the theory, sufficient to prove the theory of no value. He concludes as follows:

"If I might presume to estimate the net results of this renewed discussion of the causes of glacier-motion, I should say that they are not considerable, but yet are far from worthless. Canon Moseley's experiments have added something to our knowledge, and especially those on the tenacity of ice, which have some bearing on the origin of crevasses. Of far greater importance are the observations on ice-planks made by Mr. William Mathews. The first of these, published in the 'Alpine Journal,' gave prominence to a fact which had long been familiar to myself, and probably to many others. I have often found that long icicles placed in an inclined position, and supported only at the upper end, will gradually resume the vertical direction, and I had, perhaps too lightly, assumed that this was a particular instance of the process by which ice changes its form through fracture and regelation. In Mr. Mathew's first experiment, conducted during a thaw, a thick plank of ice supported at each end was deflected at the middle through a space of 7 inches in as many hours. Although none but very minute fissures were observed, the facts did not seem to me altogether inconsistent with that explanation. In the second series of observations, made during the severe frost of February last, Mr. Mathews found that at temperatures notably below the freezing-point a plank of ice, supported as before, subsides slowly between the points of support under the sole influence of its own weight. The deflection under these circumstances was about 1½ inch in twenty-four hours. Taking this observation in connection with a multitude of facts recently brought to light, and especially the researches of M. Tresca, we are led to admit that ice, in common with very many apparently rigid bodies, does possess a certain degree of plasticity which is exhibited by changes of form effected very slowly under the action of forces of moderate amount, rather than by the rapid action of more powerful agencies.*

"The admission of this conclusion may slightly modify, but will not materially alter, the views now generally held as to the causes of glacier-motion, which are mainly derived from the remarkable researches of Professor Tyndall. Whatever may be the final judgment of men of science, I feel quite sure that it will not confirm the opinion expressed by Canon Moseley in his latest publication: that "the phenomena of glacier-motion belong rather to mechanical philosophy than to physics." Every real advance that has been made toward the explanation of those phenomena has been due to the application of increased knowledge of the physical properties of glacier-ice; and if any thing be wanting to complete the explanation now generally accepted, it must be derived from such additional acquaintance with those properties as may be derived from continued observation and experiment."

3. *The North American Lakes considered as Chronometers of Post-glacial time;* by Dr. EDMUND ANDREWS. 24 pp. roy. 8vo. (Trans. Acad. Sci. Chicago, vol. II).—Dr. Andrews discusses in this paper the nature of the post-glacial deposits on the shores of Lake Michigan, especially in the vicinity of Chicago, their extent, the areas of the several beaches, the erosion these beaches have undergone, the width of the subaqueous plateau formed along the border of the lake out of the material removed in the erosion, and the amount of sand moved in the process; and from the elements thus obtained, arrives at the following conclusions:

(1.) The upper beach began to form immediately after the Boulder Drift period, and continued to accrete for about 900 years. No animal fossils have yet been found in it.

(2.) The waters then fell suddenly to about their present level, where they remained till a thin bed of peat accreted on the marshy slope vacated by the waves. I have not been able to collect data for a calculation of this first low-water period, but from the position of the soil-bed in the eastern dunes, I incline to think it lasted 500 or 1000 years.

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* In Dana's Manual of Geology, p. 673, this plasticity is recognized among the means of motion on the ground of observations, similar to the above, made by Kane in his "Arctic Explorations."

(3.) The water rose again, submerging for a short time the upper beach, but soon fell to the line of the middle one, where it remained about 1,600 or 2000 years. This period appears to be cotemporary with the Loess.

(4.) The water, which had already slowly fallen some feet, now retired more rapidly to near its present level, which it has maintained with only moderate fluctuations ever since.

(5.) The total time of all these deposits appears to be somewhere between 5,300 and 7,500 years.

The discussion is an interesting and important one. Some uncertainties in the calculations occur to us; but without a special examination of the region we are not at present prepared to mention any but the following. The author writes as if he supposed that sand in the course of transportation always remained sand. He observes that "the sand movement in the lake is confined to the shore line," as proved by the fact that "there is no sand in deep water," not recognizing the well-known geological fact that sands on coasts are always undergoing wear through the attrition of grain upon grain under the action of waves and currents, and that while the finer material made by this attrition is floated off to deeper waters, the coarser is left behind in such cases near or on the shores.

3. *Fossils in the Mineral veins of the Carboniferous Limestone of Great Britain.*—A paper on this subject by Mr. Charles Moore, (Rep. Brit. Assoc. for 1869, p. 360), contains notices of numerous fossils in the Lead mines of the Carboniferous limestone. In the walls of the Charterhouse Lead-mine, in the Mendip range, 270 feet from the surface, over 80 species of Liassic fossils were obtained by him, and more than 30 of Carboniferous. The Liassic included a Chara, wood in the form of jet, Rhizopods, Pentacrinites, a Cidaris and other Echinoderms, Serpula, claws of Crustacea, many Mollusks, remains of about 10 species of fishes of the genera *Acrodus*, *Hybodus*, *Lepidotus*, &c., and a tooth of an Ichthyosaur; and among the Carboniferous, there were species of Helix, Hydrobia, Planorbis, Proserpina, Valvata, Vertigo, all either land or freshwater Mollusks, also 11 species of Ostracoids, besides Mollusks, Serpulæ, Encrinites, Corals and Conodonts. A similar range of facts was observed in connection with other lead mines. We cite the following general remarks.

Whilst the various mines and mineral deposits I have examined have certain species in common, it may be said that they have each special paleontological features of their own.

In the Keld-Head Mines organic remains are very abundant at about 450 feet from the surface, amongst which are many Foraminifera, chiefly of the genus *Involutina*, of which there are six species, and univalves of about twelve genera, the freshwater species *Valvata anomala* Moore, and *Planorbis Mendipensis*

Moore, being present, and also Entomostraca of several new species.

The Fallowfield mines, although not yielding a very long list of species, have their special interest in the presence of the land and freshwater genera *Stoastoma*?, *Hydrobia*, and *Pisidium*; *Involutina*, as in the Keld-Head mines, though rarely; and a single seed of the *Flemingites gracilis* Carr. The richest samples from this mine are at 90 and 450 feet from the surface.

The Grassington mines are not only very rich in individual specimens, but have yielded the greatest number of species, among which are again freshwater remains of *Hydrobia*, *Planorbis*, *Valvata*, and *Lithoglyphus*. Entomostraca of at least ten species, Conodonts of several varieties, and fish-remains of the genera *Petalodus*, *Orodus*, &c.

The Alston mines have yielded about twelve species of univalves, though they are not in good condition. Foraminifera are present, but are rare, and fish-remains of the genera *Petalodus*.

The Weardale mines, and those of Allenheads, are comparatively not rich, the vein stuff in them being much mineralized. Conodonts occur in the former, Entomostraca rather abundantly in the latter, and also, though rarely, the genus *Hydrobia*. In these veins, and also at Alston, I have detected, for the first time, large cells of a foraminiferous shell, for which Mr. Brady suggests the generic name *Carteria*.

In the White and Silver Band mines remains are somewhat rarely distributed, the richest deposit being a friable ochreous sandstone, on the "sun" side of the Silver Band Old Mine, which yielded many specimens of *Hydrobia*, and one or two of *Valvata anomala*, several genera of Foraminifera, including *Involutina* and *Dentalina*, with Conodonts, and portions of teeth of *Psammodus*.

The Mount-Pleasant mines of Mold contain Foraminifera, and also the freshwater *Hydrobia*, though rarely, and Conodonts rather abundantly; but they are especially remarkable for the great variety of fish-remains they yield, which appear to represent at least ten different genera. Mixed with the "dowks" of the mine are occasionally small pieces of laminated stone the surfaces of which exhibit numerous traces of fish-scales.

The researches I have been making have involved very considerable labor and minute investigation; but as they will to some extent have opened up a new field of inquiry, I hope they will not be without some results. Before concluding, I desire to refer to several of the more interesting paleontological facts which have been obtained. * * * * * *

Not the least important fact in my mine explorations has been the discovery of a land and freshwater fauna. Until I obtained the three genera of *Helix*, *Vertigo*, and *Proserpina*, with the freshwater genera *Planorbis* and *Valvata*, in the Charterhouse Mine, the only known terrestrial shell below the secondary beds was the *Pupa vestusta* Daws., found by Sir Charles Lyell and Dr.

Dawson in the Coal measures of Nova Scotia. To the above genera I have now to add those of *Hydrobia*, *Stoastoma ?*, *Lithoglyphus*, and *Pisidium*, from the mines of the north of England, some of which I have little doubt are older than the *Pupa vetusta* of the coal-beds. There is thus the fact of the presence of nine genera of land and freshwater shells in the lead-veins of this country.

In addition to the list of organic remains which follows, numbering about 112 species from the north of England and North-Wales mines, eight, which are not in common, have been obtained from Weston, and to these again are to be added 89 in the list previously given from Charterhouse, so that in true and workable mineral veins I have found 209 species. In the Carboniferous Limestone of the Frome district precisely similar phenomena occur, though the fissures are not worked. These Rhætic and Liassic veins have yielded me about 70 species, so that, including the districts I have enumerated, I have obtained from vein-fissures, with their deposits of different ages, about 279 species of organic remains.

Under these peculiar circumstances, I have discovered the oldest known Mammalia, the oldest land and freshwater Mollusca, about 52 species of fish, and about 8 of Reptilia, besides the other groups to which reference has been made.

With regard to the origin of the veins, Mr. Moore observes as follows:

The chief material of all the mineral veins I find to be of marine origin; all the organic contents are fossil, and their precise geological age can be arrived at without much difficulty. Wherever they contain land shells, as on the Mendips, or freshwater shells, which occur in the veins of Alston, and are wide-spread elsewhere, they are also fossil and of contemporaneous age with the other remains. It is certain from this that the veins received their infilling when within the influence of the ocean, and before their present elevation, since which time, as I have before stated, I doubt if there could be any material alteration in their contents. * * *

It has now been established, without doubt, by the highest chemical authorities, that many of our most important minerals are present in minute quantities in the waters of the ocean. This is admitted by those who believe in segregation, the difference being that they think it was first deposited and afterward extracted from the parent rock, and redeposited in the veins, rather than originally collected in the veins themselves.

As regards the connection subsisting between the ocean and the vein-fissures, I believe it will be recognized to be the case that, in the great majority of instances, the different veins come directly to the surface, and wherever a later rock has been deposited, which is only in exceptional cases, covering up the mouth of the vein, there will still be found a break in the sequence of the strata, which might give almost unlimited time for the precipitation of the minerals therein. Wherever systems of veins occur, it is probable there will be found connected therewith an abnormal condition and considerable breaks in the deposition of the rocks. I have shown this to be the case in connection with the Carboniferous Limestone of the Mendip range, and its continuation through South Wales, in which districts it can be seen that those rocks were, through enormous periods, exposed to the influence of the ocean, possibly forming reef-like barriers around the edges of the Carboniferous basin, the fissured veins and floor of this sea-bottom receiving at some periods materials of Rhætic or of Lower or Middle Lias age, whilst an occasional capping of the beds of Inferior Oolite, left in some Carboniferous limestone trough, now and then cover up the mouths of the veins that had received all their contents prior to its deposition. Some most instructive examples are present in this district, in which it may be seen that whilst there are on the walls of the vein the usual vertical conditions of vein-stuff, such as calc-spar, sulphate of barytes, &c., with occasional hematite iron-ore, calamine, and galena, the central portion of the vein is unmistakably of Liassic or Rhætic age. In all such instances there are combined the elements of open fissures communicating with the ocean, and greater or less time in the reception of their contents.

4. *Additional note on Elasmosaurus;* by E. D. COPE.—To my preceding note on Elasmosaurus, I append the following, in consequence of the reading of another criticism by Prof. LEIDY, in the Proceedings Acad. Nat. Sci. Philad., January to April, 1870, (issued in June). [See p. 139 of this volume.—EDS.].

In this Dr. Leidy agrees with my identification of Cimoliasaurus and Discosaurus made in 1868, regarding them as the same. But he employs the name Discosaurus instead of Cimoliasaurus, to which we object for three reasons: 1, in works written subsequent to his determination, Cimoliasaurus had been exclusively used, and has therefore obtained considerable currency; 2, Discosaurus was founded upon a miscellaneous collection of species, and not defined; 3, the name refers perhaps to an individual peculiarity of one of the species, as suggested by Leidy, and conveys an erroneous impression of there being a vertebral disc characteristic of the genus, whereas the peculiarity consists of a groove. The name Cimoliasaurus is open to none of these objections.

He however unites with the above genus my Elasmosaurus, although a few pages previously he considers them distinct, on the same grounds that convinced me of the propriety of separating them, viz: the enormous neck with compressed vertebræ in the one, and the short transverse cervicals of the other. No such difference is displayed by the species of Plesiosaurus, though there is considerable variation in the genus in this respect. It cannot however be predicted, that no species combining the characters of the two will ever be found. All genera in paleontology stand open to this risk.

The *Cimoliasaurus grandis* (*Brimosaurus* Leidy) presents the shortened cervicals of *C. magnus* and therefore is not an Elasmosaurus. The *E. orientalis* is as yet but little known, and there may be doubts as to which genus it represents. In a restored figure of it which was given in an article in the American Naturalist (1868, p. 84), it is represented with a neck of the shorter type of Cimoliasaurus. Whether the shorter or longer type of cervicals belong to it will remain uncertain until more remains are found. If it be a true Elasmosaurus, the figure will represent better a Cimoliasaurus.

In his second notice Leidy mentions his having reversed the extremities of the vertebral series in the three Cimoliasauri described by him.

5. *Lias and Oolite of Australia.*—Mr. CHARLES MOORE has published an important paper on Australian Mesozoic Geology, in the Quart. J. Geol. Society for 1870, describing beds of the age of the Middle and Upper Lias and Lower Oolite from Western Australia, and Oolitic or Lower Cretaceous from Queensland (at Wollumbilla). Out of 50 species of Mollusks in Western Australia 20 are found to be identical with British species, viz:

Ammonites Aalensis, var. *Moorei* Lycett, Upper Lias; *A. radians* Rein, Upper Lias; *A. Walcottii* Sow., Upper Lias; *A. macrocephalus* Schloth, Oolite; *A. Brocchii* Sow., Oolite; *Nautilus semistriatus* D'Orb., Upper Lias; *Belemnites canaliculatus* Mill, Oolite; *Gresslya donaciformis* Goldf., Upper Lias; *Maycites liassianus* Quenst., Middle Lias; *Cucullæa oblonga* Sow., Oolite; *Pholadomya ovulum* Ag., Oolite; *Avicula Münsteri* Goldf., Oolite; *A. echinata* Sow., Oolite; *Pecten cinctus* Sow., Oolite; *P. calvus* Münst., Oolite; *Lima proboscidea* Sow., Oolite; *L. punctata* Sow., Oolite; *Ostrea Marshii* Sow., Oolite; *Rhynchonella variabilis* Schloth, Oolite; *Cristellaria cultrata* Montfort, Oolite. *Lima proboscidea* and *Ostrea Marshii* appear to have been as abundant as in the hills around Bath; and *Pecten cinctus* from Australia attains the same large proportions as in this country. The Corn brash is apparently represented in Australia by *Ammonites macrocephalus* and *Avicula echinata*, and the Middle Lias by the *Mya cites liassianus* before mentioned.

The paper describes many new species.

6. *Plants of the Coal formation of Langeac, Haute-Loire;* by Dr. H. B. GEINITZ.—The plants mentioned in this paper as occurring at Langeac and remarked upon are *Calamites cannæformis* Schl., *C. Cisti* Bgt., *C. Suckowi* Bgt., *Annularia longifolia* Bgt., *Cyatheites arborescens* Sch., *C. dentatus* Bgt., *C. Miltoni* Artis, *Alethopteris pteroides* Bgt., *Cardiocarpus emarginatus* Göpp. & B., *C. Gutbieri*, *Noeggerathia palmæformis* Göpp., *Rhabdocarpus ovalis* Göpp. & F., *Cordaites principalis* Germ., *Trigonocarpus Nœggerathi* Sternb., *T. ventricosus* Göpp. & F. The several kinds of fruit here described give special interest to the memoir. The accompanying plate contains figures of several forms under six species, two species of Cardiocarpus, one of Rhabdocarpus, one of Cordaites, and two of Trigonocarpus.

7. *Sivatherium in Colorado.*—Dr. Leidy refers a fragment of a fossil from Colorado (received through Dr. Hayden from Dr. Gehrung), with a query to a genus near Sivatherium, which he calls *Megacerops.* He regards the fossil as corresponding to that portion of the face which comprises "the upper part of the nose, together with the forehead and anterior horn cores." The animal to which it belonged was nearly as large as the Sivatherium. The distance between the center of the two horn cores is 10½ inches; the length of the horn cores above the intervening space 5 inches; the breadth of the face where narrow below the horn cores 7¼ in. He names the species *Megacerops Coloradensis.*—*Proc. Acad. Nat. Sci. Philad.*, 1870, p. 1.

8. *Megalonyx Jeffersoni in Illinois.*—A metacarpal bone of this Megalonyx is announced by Dr. Leidy (Proc. Acad. Nat. Sci., 1870, p. 13) as having been found in a crevice in the lead-bearing rocks near Galena, 130 feet below the surface, along with a last lower grinder of the extinct Ox, *Bison antiquus.* The Museum of the Academy contains other bones from the same locality, being remains of a large extinct Peccary, *Platygonus compressus*, an extinct Raccoon, *Procyon priscus*, and a large insectivore, *Anemodon Snyderi*, all of which were probably cotemporaries of the Megalonyx.

9. *Mineralogical Contributions of G. vom Rath* (Pogg. Ann. cxxxviii, 449).—VOM RATH describes and figures here crystals of the following Vesuvian minerals: twins of Anorthite, crystals of Oligoclase, Wollastonite, Orthite and Humite; besides giving an account of a new mineral from Laach which he names *Amblystegite*, and a description of certain twins of orthoclase.

Amblystegite occurs in small crystals of a brown to reddish-brown color, a gray streak slightly greenish; adamantine luster. The crystals are orthorhombic; $i\text{-}\bar{i}:I=135°\ 50'$, $i\text{-}\breve{i}:1\text{-}\bar{2}=119°\ 26'$, $\frac{1}{4}\text{-}\breve{i}:\frac{1}{4}\text{-}\breve{i}=163°\ 47'$, $i\text{-}\breve{i}:\frac{1}{4}\text{-}\breve{i}=98°\ 6\frac{1}{2}'$. B.B. very fusible and thus differs from humite and chrysolite. Composition,

$$\text{Si}\ 49{\cdot}8\quad \overline{\text{Al}}\ 5{\cdot}05\quad \dot{\text{Fe}}\ 25{\cdot}6\quad \dot{\text{Mg}}\ 17{\cdot}7\quad \dot{\text{Ca}}\ 0{\cdot}15=98{\cdot}30,$$

corresponding to the oxygen ratio for $\dot{\text{R}}$, $\overline{\text{R}}$, Si, 12·81 : 2·36 : 26·56. Only half a gram was here used. The mineral is so mixed with magnetic iron as to be separated with difficulty for analysis.

10. *Laxmannite* of Nordenskiöld (J. f. pr. Ch., cvii, 491) according to Hermann (ib. II, i, 447) is probably Vauquelinite; Nordenskiöld made the crystals monoclinic with the inclination of the vertical axis 69° 46′; G. = 5·77, H. = 3, color olive-green; and obtained for the composition (mean of two analyses)—

$$\ddot{\text{P}}\ 8{\cdot}31\quad \overline{\text{Cr}}\ 15{\cdot}91\quad \dot{\text{Pb}}\ 61{\cdot}16\quad \dot{\text{Cu}}\ 11{\cdot}64\quad \dot{\text{Fe}}\ 1{\cdot}06\quad \dot{\text{H}}\ 1{\cdot}10=99{\cdot}18$$

Hermann observes that Berzelius probably analysed the same mineral in his examination of the species he named Vauquelinite, and that if the pure oxyd of chrome of Berzelius be taken as a mixture of phosphoric and chromic acids, the two analyses closely agree, as follows:

	$\overset{\cdot\cdot\cdot\cdot\cdot}{\bar{\text{P}}}$	$\dddot{\text{Cr}}$	$\dot{\text{Pb}}$	$\dot{\text{Cu}}$	$\dot{\text{Fe}}$	$\dot{\bar{\text{H}}}$
Berzelius,..........	8·31	15·85	60·87	10·80	1·10	1·10=98·03
Nordenskiöld, ...	8·31	15·91	61·16	11·64	1·06	1·10=99·18

both leading to the formula $2\dot{\text{Cu}}^3\overset{\cdot\cdot\cdot\cdot\cdot}{\bar{\text{P}}}+5\dot{\text{Pb}}^2\dddot{\text{Cr}}+2\dot{\bar{\text{H}}}$.

11. *Phosphorchromite.*—Hermann (J. f. pr. Ch., II., i, 450) thus names a chromophosphate of lead and copper from Beresofsk. It occurs concretionary, crystalline or massive within, with a blackish-green color, pistachio-green streak, H. =3, G. =5·80. An analysis afforded:

$\overset{\cdot\cdot\cdot\cdot\cdot}{\bar{\text{P}}}$ 9·94 $\dddot{\text{Cr}}$ 10·13 $\dot{\text{Pb}}$ 68·33 $\dot{\text{Cu}}$ 7·36 $\dot{\text{Fe}}$ 2·80 $\dot{\bar{\text{H}}}$ 1·16=99·72,

leading to the formula $3\dot{\text{Cu}}^2\overset{\cdot\cdot\cdot\cdot\cdot}{\bar{\text{P}}}+5\dot{\text{Pb}}^3\dddot{\text{Cr}}+3\dot{\bar{\text{H}}}$.

Vanadiolite of Hermann (J. f. pr. Chem., ib. 445) occurs in small crystals, partly in druses, of a dark or blackish-green color and gray-green streak; dark emerald-green color and transparent, in small grains; of strong vitreous luster, with G. = 3·36. B.B. melts on the edges to a black blebby slag. His analysis afforded

$\dddot{\text{Si}}$ 15·61 $\dddot{\text{V}}$ 44·85 $\dddot{\bar{\text{A}}}\text{l}$ 1·10 $\dot{\text{Fe}}$ 1·40 $\dot{\text{Mg}}$ 2·61 $\dot{\text{Ca}}$ 34·43=100

and he regards it as a subvanadate combined with augite like the Lavroffite of the same locality, Slŭdănka, near Lake Baikal. He writes the formula $3\dot{\text{R}}\dddot{\text{Si}}+\dot{\text{Ca}}^6(\text{VO}^4+2\text{VO}^5)$.

12. *Wolframite.*—Descloizeaux suggested in 1850 (Ann. Ch. Phys., III, xxviii) that Wolfram was oblique rhombic in crystallization, after some measurements of crystals. He has recently confirmed this conclusion (ib., IV, xix, 168, Feb. 1870) by both optical and crystallographic observations. He finds the obliquity 90° 38′, and the prismatic angle 100° 37′. His crystals were from Bayewka, near Ekatherinenbourg, in the Urals, a variety which afforded Mr. Koulibine,

$\dddot{\text{W}}$ 74·32 $\dot{\text{Fe}}$ 2·11 $\dot{\text{Mn}}$ 20·90 $\dot{\text{Ca}}$ 1·30 $\dddot{\text{Si}}$ 0·28=98·91,

agreeing nearly with Hubnerite, of Nevada. G.=7·357.

13. *Namaqualite, a new ore of Copper;* by Prof. CHURCH (J. Ch. Soc., II, viii, 1).—Namaqualite is named after the region in which it occurs, Namaqualand, S. Africa. It occurs in thin layers of silky fibers, which alternate with chrysocolla and are sparingly mixed with small crystals of "magnesia mica." Color pale blue; H.=2·5; G.=2·49; isolated fibers transparent under the microscope. In the closed tube yields much water and blackens; at 100° C., or over sulphuric acid in vacuo, no loss of weight. Mean of analyses:

$\dddot{\text{Si}}$	$\dddot{\bar{\text{A}}}\text{l}$	$\dot{\text{Cu}}$	$\dot{\text{Mg}}$	$\dot{\text{Ca}}$	$\dot{\bar{\text{H}}}$
2·25	15·29	44·74	3 42	2·01	32·38=100·09.

The oxygen ratio for the protoxyds, alumina and water is 4 : 3 : 11. Prof. Church remarks that its crystallized condition, and its definite and constant composition show that it is a true species; and the presence of a sesquioxyd that it is related in composition to hydrotalcite and pyroaurite.

14. *Contributions to the Mineralogy of Victoria;* by G. H. S. ULRICH, F.G.S. 32 pp. 8vo. Melbourne, 1870. (Reprinted from

the Report on the Mineral Statistics of Victoria for the year 1869, presented to Parliament.)—These "contributions" contain detailed descriptions of many species, with figures of crystals, and some analyses, the latter by Mr. C. Newbery. The following are among the new or more interesting species noted by Mr. Ulrich.

Maldonite, or Bismuthic Gold, from the Nuggety reef, of the Alliance Company, Maldon. It occurs in particles in the granite veins affording gold. It has a pinkish white color, but tarnishes easily on exposure to a dull copper color, and ultimately to black. Cleavage apparently cubic. H.=1·5—2. Malleable and very sectile. Formerly in larger pieces in the upper workings of the Alliance Company's mine.

Native Bismuth, Bismuth Glance and Bismuthite occur in a quartz vein at Linton, Ballarat district.

Selwynite of Ulrich (Dana's Min., p. 509*), a chrome-bearing mineral, has been further examined. Mr. Newbery obtained in new analyses—

$\dddot{\mathrm{Si}}$	$\dddot{\bar{\mathrm{Al}}}$	$\dddot{\bar{\mathrm{Cr}}}$	$\dot{\mathrm{Mg}}$	$\dot{\mathrm{Na}}$	$\dot{\mathrm{H}}$
47·25	35·28	7·82	2·42	—	5·67
48·42	34·72	6·94	2·11	2·03	4·83
48·23	38·16	6·14	1·21	3·12	2·90

the mean of which affords very nearly the oxygen ratio for $\dot{\mathrm{R}}$, $\dddot{\bar{\mathrm{R}}}$, $\dddot{\mathrm{Si}}$, $\dot{\mathrm{H}}$, 1 : 16 : 22 : 3½. Mr. Ulrich observes that the mineral is probably an altered feldspathic mineral, and related to the Pinite group, or especially to gieseckite and dysyntribite. When polished it has some resemblance to nephrite.

Talcosite is a new mineral occurring in thin seams in the selwynite—the seams lamellar-columnar. Resembles talc in feel. H.=1; but transverse to lamination 1·5–2·0. G.=2·46–2·5. Color silver-white, faint greenish or yellowish. Luster pearly. Scales flexible, not elastic. B. B. exfoliates, whitens, gives off water, and fuses at 4 to a blebby enamel; with cobalt solution a fine blue. A mean of two analyses by Mr. C. Newbery—

$\dddot{\mathrm{Si}}$ 49·04 $\dddot{\bar{\mathrm{Al}}}$ 45·98 $\dddot{\bar{\mathrm{Cr}}}$, $\dot{\mathrm{Fe}}$, $\dot{\mathrm{Mg}}$, $\dot{\mathrm{Na}}$, *tr.* $\dot{\mathrm{H}}$ 4·30=99·32,

giving the oxygen ratio of $\dddot{\bar{\mathrm{R}}}$, $\dddot{\mathrm{Si}}$, $\dot{\mathrm{H}}$=5 : 6 : 1.

Herschelite is found at a quarry of basalt near the river Yarra at Richmond. Several interesting figures of its crystals are given. It occurs with phillipsite, analcite and calcite. It is also found in basalt at the shaft of the Ballarat and Clunes Gold-Mining Co., Clunes.

Struvite in crystals occurs in guano, in the Skipton Caves near Ballarat. The depth of the guano is about 20 feet, and it has been derived from the *excrements of bats.*

Tetrahedrite, Stibnite, Molybdenite, Brookite, Cassiterite, Scheelite, Sapphire, Topaz are among the other minerals of which mention is made in the memoir.

15. *Lavroffite* (Lawrowit), has been analyzed by Hermann (J. f. pr. Ch., 1870, II, i, 444) and shown to be a vanadiferous diopside.

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* In the analysis cited in the Mineralogy, $\dot{\mathrm{Mg}}$ 4·36, should be $\dot{\mathrm{Mg}}$ 4·56.

He obtained $\ddot{\mathrm{S}}\mathrm{i}$ 53·65 $\dddot{\bar{\mathrm{A}}}\mathrm{l}$ 2·25 $\dot{\mathrm{F}}\mathrm{e}$ 2·48 $\dot{\mathrm{M}}\mathrm{g}$ 16·00 $\dot{\mathrm{C}}\mathrm{a}$ 23·05 = 100, with 2·57 p. c. of hypovanadic acid.

16. *On Sellaite, a new native fluorid*, by Dr. Struver, Atti della R. Accad. di Torino, iv, 1868, 35.—This mineral was detected on a specimen of anhydrite from Geibroulà, in the State of Maggiore, in Piedmont. On the anhydrite there were also crystalline sulphur, dolomite, and rare twins of albite. It is tetragonal in crystallization, with I on 1 (∞P on P) = 123° 30′, and *i-i* on *i*-3 (or ∞P∞ on ∞P3) = 161° 34′. Cleavage parallel to I and *i-i* perfect. H. = 5. G. = 2·972 at 24° C. Fracture conchoidal. Luster vitreous. Colorless. Transparent. Powder white.

It is insoluble in water; also in acids, except concentrated sulphuric, which causes an evolution of fluohydric acid. Small fragments melt in the flame of a candle with intumescence. From the similarity of its reactions to those of fluorite, the author concludes as not improbable that the mineral is a monofluorid of magnesium, which contains 38·71 of magnesia and 61·29 of fluorine. In treating a small fragment of it with concentrated sulphuric acid, he obtained for the proportion of magnesium 39·64 p. c. The small amount of the mineral in hand prevented his making a complete analysis. The species is named after the distinguished crystallographer, Quintino Sella.

17. *Ambrosite.*—C. U. Shepard describes under this name, in The Rural Carolinian, Feb., 1870, p. 311, a resin resembling amber, from the phosphatic formation near Charleston, S. C. It is yellowish-brown externally, and clove-brown within; feebly translucent; sinks slowly in water; and melts into a clear liquid at 460° F., after softening at a much lower temperature. It gives off much succinic acid long before it melts, and a dense yellow oil is volatilized on its fusion. It is very combustible, burns with a bright yellowish-white light and pleasant odor, and leaves no ash behind. The name is made from the two words *amber* and *rosin*.

18. *On the Guanape Island Guano and its minerals;* by Prof. C. U. Shepard, (ib. May, p. 469).—Guanape island is two miles northeast of the Chincha Islands. A mineral occurring in balls and veins in the Guanape Guano, having H. = 1·5, G. = 2·3, and resembling a little the red Cheshire rock-salt, but rhombic instead of cubic in cleavage, is called *Guanapite* by the author. He found it to consist of sulphate of potash 67·75, sulphate of ammonia 27·88, oxalate of ammonia 3·75 = 99·38. It loses ammonia slightly on exposure to the air.

Another substance from the Guano is named *Guanoxalite.* It contains sulphate of potash 40·20, oxalate of ammonia 29·57, water 30·46 = 100·23. It occurs as pseudomorphs of a bird's egg, having the size and shape of that of a domestic duck. Color white exteriorly, but from the presence of the altered shell. Within, the material is foliated with a rhombic cleavage; a cream-white color; somewhat pearly luster; feeble translucence; hardness below 2, and G. = 1·58. When heated it swells up, blackens, partially fuses, gives off copious fumes of ammonia, and leaves a white residue of sulphate of potash.

In the cavities of the spinal column and the stomach of the remains of birds from the Guano, yellow crystals of the mineral *Taylorite* occur, a species first noticed by W. J. Taylor in the guano of the Chincha Islands. Along with the Taylorite there are numerous minute scaly crystals of *Aphthitalite;* also oxalate of ammonia, which Prof. Shepard names *Oxammite*. *Oxalate of lime* is occasionally observed in crystals; also phosphate of ammonia (*phosphammite* of Shepard) in crystals and lumps; and, as an efflorescence from this mineral, biphosphate of ammonia (*Biphosphammite* S.).

III. BOTANY AND ZOOLOGY.

1. *Miscellaneous Botanical Notices and Observations.*

The Code of Botanical Nomenclature, as digested by M. Alph. DeCandolle and adopted by the International Congress at Paris in 1867, while on the whole approved by botanists, has been variously criticised in certain particulars, some writers naturally objecting to one article, some to another. To the more important of these criticisms DeCandolle made reply at a meeting of the Botanical Society of France on the 26th of February, 1869. The number of the *Bulletin* which contains his communication was issued, we believe, at the beginning of the present year. M. DeCandolle intimates that he passes by the still controverted question as to the proper mode of citing the authority for species where they have been transferred to another than the original genus, as being one upon which further discussion could hardly be more than iteration. He merely remarks that the more experience we have of the working of the system of double citation of authors, whether with or without parenthesis, the more grave do the inconveniences appear. Since we cannot readily make a brief abstract of the several points which he does discuss, we will now notice only those upon which this Journal ventured to differ from M. DeCandolle and the Congress. The chief serious objection was raised upon article 50, which prescribed the mode in which unpublished names, taken up by an author, should be subsequently cited. We remarked that: "For instance, there may be no necessity for taking up one of Commerson's names affixed by him to his plants in herbaria; but if taken up, simple verity would seem to require this botanist's name to be cited. We should feel bound to write '*Flacourtia* Commerson,' although published by L'Heritier or Jussieu, who probably supplied the character. The rule as proposed would apply to names communicated with manuscript characters by one botanist to another, as well as to named specimens. Now, no botanist is bound to do the work of publication for another; but if he chooses to do so, the maxim *qui facit per alias*, etc., must [conversely] fairly apply, and succeeding writers should not be required to take the godfather for the father. If we rightly understand the editor, he proposes that we should write *Eulophus*, *Leptocaulis*, and *Trepocarpus* DC., although the elder DeCandolle, accepting these names with the specimens from Nuttall, scrupulously attributes them to 'Nutt. in litt.' To us, all such names, which the elder DeCandolle has, at his own discretion, published for Nuttall, are of *Nutt. in DC. Prodr. &c.*"

To which M. DeCandolle now rejoins: .... "But see the inconvenience which results. All the catalogues or dictionaries, and all indexes have *Leptocaulis* Nutt. So we search the works of Nuttall, and perhaps even the small papers scattered in the journals, and lose much time and pains; for Nuttall never published this name. It may be that he would not have desired to publish it if he had examined the question subsequently. The date of the genus is that of the publication; and the publication is really the one essential thing; for what are the most important discoveries if not published? In writing *Leptocaulis DC. ex Nutt. litt.* the primitive author is equally indicated; but then one will see in the books *Leptocaulis DC.*, and any one will readily find in the works of DeCandolle the origin of the genus and the date of publication."

We are disposed to add that the information sought would be as readily found when written "*Leptocaulis Nutt. in DC.*," and that this more strictly as well as more tersely represents the fact than "*Leptocaulis DC. ex Nutt. litt.*" The full reference is "*Leptocaulis* Nutt. in DC. Prodr. 4, p. 107;" and the question is, whether in mere enumeration this is to be abbreviated into "*Leptocaulis Nutt*," or "*Leptocaulis DC.*" Either way, the work, as well as volume and page, has to be looked up, and the trouble of finding the origin of the name in the pages of the Prodromus may probably be far less than if Nuttall had published it directly in one of his various scattered papers. But let us in our turn test the new rule by the results which would ensue from its consistent application. To the first volume of the Flora of North America Nuttall contributed a large number of new species and genera, which were published for him by the authors of that work; but for which, if cited according to the new rule, they would seem to have incurred a responsibility much beyond what was counted on. They might well insist that the new mode of citation was misleading. Moreover, if "*Centrostegia* A. Gray, mss.," so published in Bentham's *Eriogoneæ* contributed to DeCandolle's Prodromus, is briefly to be cited "*Centrostegia Thurberi* Benth. in DC. Prodr," must not *Eriogonum Douglasii* and a score of other such species on the same principle be cited as *E. Douglasii* Alph. DC., &c.? for we should search the works of Bentham in vain for these names, losing much time and pains. It will of course be said that the "auctor Bentham" at the top of the page explicitly indicates the actual author; but so equally does the "A. Gray mss." at the head of that article. To take another instance: upon this plan we may be required to write "*Eliottia* Elliot" (Sketch, 1, p. 448). Elliott himself wrote "*Elliottia* Muhlenberg," adding, "I have inserted it, as requested by Dr. Muhlenberg, under this name." He adds to the specific name "Muhl. Cat." indeed. But the context shows that the second edition of Muhlenberg's Catalogue was not then published; it is doubtful if it was so when that part of Elliott's first volume was issued; and if it were, the enumeration in the catalogue, according to the Code, is not tantamount to publication. There are other well-known cases of a similar sort, in which an author would become liable to the imputation of dedicating a genus to himself.

When we consider how punctilious the elder DeCandolle was in this respect and others of a similar sort, and how generally he has been followed, we must regard the change proposed in the code and exemplified in some (but not all) of the later volumes of the Prodromus as an innovation rather than the declaration of an existent common law, or a natural development of it. The laudable end sought, however, in this and in article 42, is to obviate or diminish grave and growing inconveniences which arise from imperfect publication and merely recorded names in collections and herbaria. And here we do not complain that the rights of publication through the distribution of specimens, conceded in art. 42, are seemingly very much restricted by the new commentary. The duty, or perhaps comity, of respecting imperfectly published or unpublished names is only one of imperfect obligation, as the moralists would say, and therefore not to be fixed by law but governed by discretion. Legal rights begin only with publication, no matter by whose agency. We would only urge that subsequent citation, purporting to indicate the origin, should not in effect misquote the record.

Baillon's Histoire des Plantes, briefly noticed in our January number, goes on well. We have received two more monographs published since the year began; one of the *Proteaceæ*, the other and much larger one of the Papilionaceous *Leguminosæ*. The paper and print are most excellent and attractive: so also are the wood-cuts, as is usual in French publications of this kind. We notice that an English translation is announced. Considering the high price and generally limited sale of botanical books in England we should not expect it would pay. But, if it could ever be finished upon the plan adopted, it would be a proper companion and complement to LeMaout and Decaisne's *Traité General de Botanique*, of which an English edition is now in preparation under the best auspices. The latter copiously illustrates the families, and barely enumerates some of the principal genera. The former illustrates the tribes, &c., by a general history of the structure of some leading representatives, cast in a popular and readable form, and adds the characters of the genera, in the ordinary technical form and in the Latin language.

An attempted Improvement in the Arrangement of Ferns and in the Nomenclature of their Subdivisions, is the title of a pamphlet by the Rev. Prof. Hincks of the University of Toronto and President of the Canadian Institute; a popular account of the structure, fructification, and fertilization of this group of plants, which he regards as an alliance (*Filicales*), containing the three orders, *Osmundaceæ*, *Cyatheaceæ*, and *Polypodiaceæ*, founded on the sporangia, the *Ophioglorsaceæ* being excluded by their "straight æstivation" [vernation] and referred, as is natural, to the *Lycopodiaceæ.* The tribes of the first two filical orders three each, of the *Polypodiaceæ* four times three, or three for each division or subdivision, founded on the sori. For genera he is disposed to make more of the venation than is the present mode, and less of stipes and articulation, and so discards *Phegopteris.* Finally, he takes up the questions relating to *Aspidium*, both as to generic comprehension and nomenclature, and proposes to keep *Lastrea* and *Polystichum* as genera.

The Genus Hydrolea is reviewed by A. W. Bennett in the 53d number of the Journal of the Linnean Society (May, 1870), and the species are brought up to thirteen, of which three are here established. That *H. affinis*, described in the last edition of Gray's Manual, should have been overlooked is not to be wondered at; but specimens of it doubtless occur in the Kew herbaria, probably confounded with *H. quadrivalvis.* Mr. Bennett sides with Choisy in keeping up the order *Hydroleaceæ*, as distinct from *Hydrophyllaceæ*, "most clearly," on account of "the bilocular many-seeded ovary, axile placentation, and leaves invariably simple." These characters come down to two, the indefinitely numerous ovules and the two-celled ovary. But the new Californian genus *Draperia* Torr. stands in the gap, having the two-celled ovary as well as the habit of *Nama*, with the geminate ovules of proper *Phacelia.*

Tampico Jalap has within the last few years come into the markets as a distinct sort, and "although less rich in resin and less purgative than true jalap, yet on account of its lower price it has found a ready sale." Mr. Daniel Hanbury has traced the article to its source, in the root of a new *Ipomœa*, *I. simulans* Hanbury, described and figured in the 53d number of the Journal of the Linnean Society, which inhabits the Sierra Gorda, etc., in Mexico.

A Revision of the Flora of Iceland is the title of an extensive paper in the same Journal, by Professor Babington. The catalogue of Phænogamous and Acrogenous plants reaches 467 species. Although Iceland touches the arctic circle and is largely occupied by mountains, "many of which rise to the height of 6000 feet and are covered through fully their upper half with perpetual ice and snow, from whence extensive glaciers descend almost to the level of the sea," yet the climate is rendered so mild comparatively by the influence of the Gulf Stream which washes the coast, and the rain-fall and prevailing cloudiness in summer are so great, that, while on the one hand there is no proper forest, on the other "only 62 species are found which do not form part of the British Flora," and not more than three are decidedly arctic, viz., *Gentiana detonsa*, *Pleurogyne rotata*, and *Epilobium latifolium.*" All three of these come down to comparatively low latitudes and low levels in North America. A plant of much interest, and which should be thoroughly compared with that so named in North

America, is *Platanthera hyperborea*, of which the present writer craves Icelandic specimens.

The Fertilization of various Flowers by Insects is the title of an original paper, contributed by Dr. Wm. Ogle to the April number of the Popular Science Review. Of the various mechanical contrivances described, as also of the views suggested, some are not novel, the writer, as he states, not being familiar with the botanical literature of the subject (and this is most evident): but the whole article is well worthy of much attention, and would have been most interesting to the general reader—the narrative being remarkably clear—except that it is so disfigured by typographical errors as to be almost unintelligible to ordinary readers; indeed, the proofs would seem not to have been revised at all. The most interesting and original portion is that which relates to the fertilization in the Heath family, and to the use of the awns or appendages on the back of the anthers in Heaths, Uva-Ursi, Vaccinium, &c., by which the bee in sucking the flower topples or moves the anther, so that the pollen falls out of the terminal openings upon the insect's head. Bean-flowers and their fertilization, as also in related papilionaceous blossoms, are well described; and it is noticed that, while some bees have learned to get at the nectar feloniously by making a hole in the side of the calyx-tube, and others enter the regular way, and so do their proper work, an individual bee, visiting a succession of bean-flowers, keeps persistently to the one or the other plan. "It would thus appear that the habit is not an instinct, belonging by inheritance to the whole species, but is in each case the result of individual experience. As with the same experience some bees have acquired the habit and others have not, we must admit not only that these insects are intelligent, but that they differ from each other in their degrees of intelligence, some being slow in acquiring knowledge, others quicker." Perhaps the knowing ones have inherited the knack, in which case it is instinct in them, after all. Instinct, briefly defined, is congenital habit.

Elements de Botanique et de Physiologie Végétale, suivis l'une petite Flore simple et facile pour aider à découvrir les noms des Plantes les plus communs du Canada: par l'Abbé Ovide Brunet, Prof. Bot. à l'Université Laval. Quebec, 1870.—A very neat little book, prepared by Prof. Brunet, as a sort of first book in Botany for Lower Canada, where French is still the language of instruction. It is illustrated by wood-cuts mostly from very good drawings by the author's own hands; and the structural and physiological part, and also the little flora, seem to be as clear as they well can be in the diminutive space they occupy; excepting that *Abies Canadensis* is put with Balsam-Fir into a genus in which the scales are said to fall from the axis, which is an obvious oversight. It is curious to see what the plants are which are taken as the commonest in Lower Canada, and what the popular names are. For instance, *Sarracenia* is known as "Petit-cochons;" Wood-sorrel as "Alléluia;" *Impatiens fulva* as "Chou-sauvage;" *Hama-*

melis as "Café-du-Diable;" Cranberry as "Atoca," but *Vacinium Vitis-Idœa* as "Pommes-de-terre."

The Herbarium of the late Von Martius has been purchased for 30,000 francs by the Belgian Government, to form the nucleus of a national collection at Brussels.

The Michaux Grove Oaks.—This name is to be applied to a collection of Oaks to be planted in Fairmount Park, Philadelphia, "in which, if practicable, shall grow two oaks of every kind that will endure the climate," in commemoration of the younger (F. A.) Michaux, author of the Sylva Americana, and his father, author of *Histoire des Chênes de l'Amerique.* The grove is to be maintained by the income of a legacy of $6000 bequeathed by the younger Michaux to the American Philosophical Society, in trust for arboricultural purposes, which legacy is now devoted by the Society to this use.

Otto Bœckeler is publishing in the current volume of the *Linnæa*, a full account of the Cyperaceæ of the Royal Hebarium of Berlin, which now, in the fourth fasciculus reaching the *Scirpeæ*, is becoming of consequence to United States botanists. He describes several supposed new species of *Cyperus* and a few of *Eleocharis*, &c., and identifies several of ours with older species, making some changes of nomenclature. By what may be a good hit he refers the inner squamula of *Hemicarpha* to an abortive stamen, and so refers the species to Scirpus, ours becoming *S. micranthus* Vahl.

Generis Astragali Species Gentogææ pars altera.—The first part of this monograph by Prof. Bunge of the Old-World species (almost a thousand in number) of the great genus *Astragalus*, was mentioned in our January number. We have now received the completion, containing the systematic enumeration of the species, with characters and descriptions of most of them, and the synonymy. The work is published by the Imperial Academy of St. Petersburgh, this part being the initial number of the 15th volume of its *Mémoires*, pp. 254.—The succeeding number, of over 300 pages, consists of part first of

Flora Caucasi, by Rupprecht, with six plates. It carries the work from *Ranunculaceæ* down to *Vitaceæ.* The species are described or annotated at large, but not by definite technical characters, and vast observation and learning are exhibited, along with a liking for multiplication of genera. Dr. Rupprecht considers the name *Viola umbrosa* of Fries (1828) to have precedence of *Viola Selkirkii* Pursh ex Goldie, 1822, on the ground that the latter name was not published until 1833 by Hooker. But in Goldie's paper (which indeed was drawn up by Hooker) there is a good Latin diagnosis of the species followed by almost half a page of detailed description in English; in fact, hardly any species has been more effectually published.

Mr. Bentham's Presidential Address at the anniversary meeting this year is somewhat briefer than usual, but not less likely to attract attention. It is mainly devoted to two topics: 1, the results obtained from the recent explorations of the deep-sea faunas; and

2, those from the investigation of the tertiary deposits of the arctic regions, "tending both of them to elucidate in a remarkable degree one of the most important among the disputed questions in biological history, the continuity of life through successive geological periods." After briefly indicating where the principal data are recorded, Mr. Bentham continues:

"It would be useless for me here to retrace, after Dr. Carpenter and Prof. Verrill, the outlines of the revolution which these marine discoveries have caused in the previously conceived theories, both as to the geographical distribution of marine animals, and the relative influences upon it of temperature and depth, and as to the actual temperature of the deep seas, or to enter into any details of the enormous additions thus made to our knowledge of the diversities of organic life; and it would be still further from my province to consider the geological conclusions to be drawn from them. My object is more especially to point out how these respective dips into the early history of marine animals and of terrestrial forests have afforded the strongest evidence we have yet obtained, that apparently unlimited permanency and total change can go on side by side, without requiring for the latter any general catastrophe that should preclude the former.

"There was a time, as we learn, when our chalk-cliffs, now high and dry, were being formed at the bottom of the sea by the gradual growth and decay of Globigerinæ and the animals that fed on them —amongst others, for instance, *Rhizocrinus*, and *Terebratulina caput-serpentis;* and when, at a later period, the upheaval of the ground into an element where these animals could no longer live arrested their progress in that direction, they had already spread over an area sufficiently extensive for some part of their race to maintain itself undisturbed; and so, on from that time to the present day, by gradual dispersion or migration, in one direction or another, the same *Rhizocrinus* and *Terebratulina* have always been in possession of some genial locality, where they have continued from generation to generation, and still continue, with Globigerinæ and other animals, forming chalk at the bottom of the sea, unchanged in structural character, and rigidly conservative in habits and mode of life through the vast geological periods they have witnessed. So also there was a time when the hill-sides of Greenland and Spitzbergen, now enveloped in never-melting ice and snows, were, under a genial climate, clothed with forests, in which flourished *Taxodium distichum* (with *Sequoiæ*, *Magnoliæ*, and many others); and when at a later period these forests were destroyed by the general refrigeration, the Taxodium already occupied an area extensive enough to include some districts in which it could still live and propagate; and whatever vicissitudes it may have met with in some parts, or even in the whole, of its original area, it has, by gradual extension and migration, always found some spot where it has gone on and thriven, and continued its race from generation to generation down to the present day, unchanged in character, and unmodified in its requirements. In both cases, the permanent animals of the deep-sea bottom and the permanent trees of the terrestrial forests have witnessed a more or less partial or complete change in the races amongst which they were commingled. Some of these primitive associates, not endowed with the same means of dispersion, and confined to their original areas, were extinguished by the geological or climatological changes, and replaced by other races amongst which the permanent ones had penetrated, or by new immigrants from other areas; others, again, had spread like the permanent ones, but were less fitted for the new conditions in which they had become placed, and in the course of successive generations have been gradually modified by the Darwinian process of natural selection, the survival of the fittest only among their descendants. If, in after times, the upheaved sea-bottom becomes again submerged, the frozen land becomes again suited for vegetation, they are again respectively covered with marine animals or vegetable life, derived from more or less adjacent regions, and more or less different from that which they originally supported, in proportion to the lapse of time and extent of physical changes which had intervened. Thus it is that we can perfectly agree with Dr. Duncan, that 'this persistence (of type and species through ages, whilst their surroundings were changed over and over again) does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods;' but we can, at the same time, feel that Dr. Carpenter is in one sense justified in the proposition, that we may be said to be still living in the Cretaceous period. The chalk formation has been going on over some part of the North Atlantic sea-bed, from its first commencement to the present day, in unbroken continuity and unchanged in character."

A portion of this address will probably astonish the vegetable paleontologists, excepting perhaps, Mr. Lesquereux, whose cautious language, and his statement in this Journal that, properly speaking, no *species* can be established from leaves or mere fragments of leaves, are commended. For the President of the Linnæan Society avows himself wholly skeptical as to the "New Holland in Europe" of eocene times, denying the existence of a single specimen out of the nearly one hundred supposed tertiary species which a modern systematic botanist would admit to be Proteaceous, unless received from a country where *Proteaceæ* were otherwise known to exist. The grounds of this opinion are given in detail.

The Student's Flora of the British Islands, by J. D. Hooker, C.B., &c., just issued by Macmillan & Co., comes to take the place of Hooker & Arnott's British Flora, now out of print and antiquated, as that took the place of Sir William's British Flora which did such good service in its day and in its five editions. In these a goodly octavo, then a thick duodecimo, it now becomes a compact 16mo, a very "handy book," which may be carried in the pocket; but this compactness is not at the expense of either clearness or fullness. "The object of this work," so the preface opens, "is to supply students and field-botanists with a fuller account of the plants of the British Islands than the manuals hitherto in use aim at giving." The body of the volume, exclusive of Synopsis and Index, is contained in 474 pages; the species appear nearly to average three to the page. We are glad to see that orthodox specific characters are kept up, and that they are specific characters, diagnoses, not descriptions. Descriptive matter follows and, with habitat, range in elevation, and distribution in general, forms a secondary paragraph of much greater extent than the diagnosis; and then "*sub-species*" are numerous and fully characterized, as are occasionally varieties of second order under them. The species are naturally arranged, under sections when needful; but keys to the species are not given, the author "finding from experience that such keys promote very superficial habits amongst students,"—which they certainly do. As the book is wholly new, and the descriptions all at first hand, from living plants and dried specimens, a good many slips and oversights are inevitable. With the longest experience and great painstaking these cannot be avoided, but have to be weeded out at leisure. Here where a British Flora is only incidentally used, we have no call to point out such as we have noticed, most of which are sure to be rectified in the new edition, which must needs be promptly called for. But we will venture to find fault with one typographical blemish, as we think it, viz., the insertion of the accent mark between the letters in generic and specific names, so as to display an unsightly cleft in the middle. Surely the London printing-houses have, or can have, the vowels with accents cast upon the type; and there are ways of indicating whether the vowel be long or short, if that be worth the while. This whole business of accentuation is, no doubt, of secondary consequence, and plainly is not so much considered by English botanists of the present day as by those of a past generation. But ordinary students and amateurs may fairly ask for guidance; and those who have occasion to coin many botanical names are bound to consider in advance how they must needs be pronounced. Artificial keys to the natural orders are liable in some degree to the same objection as are keys to species; but they are almost indispensable to young students; only they must be made with extreme care, and every deviation or exception provided for. A synopsis of the natural orders, such as Dr. Hooker has prepared, guarded by mention of exceptions and qualifications, has its own advantages, and where the number of orders is not larger than in Britain may perfectly serve the purpose. A. G.

The American Entomologist, now published at St. Louis, has this year added the words "and Botanist" to its title, and Dr. George Vasey has taken charge of the Botanical Department. He has entered upon his duties with promising vigor, and is producing a series of popular botanical articles which are likely to be widely read throughout the western country and to be very useful.

In the combined July and August number the editor has a spirited article upon the Origin of Prairie Vegetation, which is mainly a criticism of the writings of Prof. Winchell upon this subject in his "Sketches of Creation." If it is seriously maintained in that work that seeds buried in diluvial deposits may have retained their vitality during the glacial period, and by their germination after it "reproduced the flora of the præ-glacial period," we can only wonder that this immense extension of the hypothesis of the indefinite vitality of buried seeds should have been made at a time when most biologists probably doubt whether any seed ever preserved the power of germination for a century or two. A. G.

2. *Carbolizing Birds;* by H. W. PARKER. (Communicated by the Author.)—The following methods, carefully studied for two years, with results noted, are recommended for the saving of birds in warm weather until the operator finds time to skin them; for the permanent preparation of drawer specimens, where the student needs a large series of individuals to determine the variations and limits of species; and for mounting small birds, at least as temporary representatives, when neither the time nor the expense involved in the old methods can be afforded.

The viscera are removed, to effect which neatly the legs are pinned widely apart, and a paper several times folded is pinned over the tail in the direction whither the viscera are drawn out. With proper care, the sex is readily observed. A wad of cotton absorbs the fluids remaining in the cavity. The leg is then grasped close to the body, and a knife or wire is introduced into the cavity and run down into the flesh of the leg, working the instrument around, but not so as to break the skin. For a small bird, five to ten drops of the commercial fluid preparation of carbolic acid is made to anoint the whole interior, and to penetrate the leg by stretching and relaxing the same in proper position. The application is repeated after the first drops are absorbed; and a wad of cotton, wet with the acid, may be left close under the breastbone next to the neck. The cavity is then filled with cotton and the skin drawn back into place. The inside of the mouth is well anointed, and a saturated wad of cotton pushed down the whole length of the neck. The eyes are removed by a hooked wire inserted into the ball, the head being so held that the humors of the eye will drop without soiling the lids. The moist lids are left as open as possible, and the specimen placed in a cool cellar till the next day, when the lids are dry enough to take their open shape. Then a nail is inserted through the lids and pushed through the bone at the back part of the orbit into the brain, and so worked as to make a good opening. A tightly rolled bit of cotton, saturated with the acid, is pushed into the brain and worked around in it, care being taken not to wet the eyelids. If by chance the feathers are wet, the acid can be removed by powdered chalk, repeatedly applied.

Specimens so prepared in warm weather, can be skinned a week or two after, if kept boxed in a cellar. No smell of decomposition is observed; the acid gradually and completely penetrates the pectoral muscles; the skin is strong and the feathers not loosened.

For permanent preparation, the skin should be laid open from the abdomen to the neck, the pectoral muscles removed and replaced by cotton, and the incision sewed up. The throat, neck and orbits are also filled with cotton. The specimen should then be suitably arranged, encircled by a slip of paper, and placed on a bed of cotton. Before this, the flesh of the wings should be laid open and arsenic applied in the usual manner.

For mounting, it only needs to run one wire through the foot, tarsus, and so on through the neck to the forehead, and another wire through the other foot to any point in the back or breast where the end of the wire catches firmly. Papers or strings for keeping the feathers in place should remain long. Some shrinking about the head and neck will eventually follow in the case of many birds, particularly those of the smallest size or of scanty, or close, plumage; but in other instances no shrinking whatever can be noticed after more than a year of drying. The cabinet in which they have been set up is made insect-proof by means of pasted cloth and paper, putty and paint, fifteen inches passage way being left in front of the shelves and the only access being through a tight door at one end, fastened by a screw.

Travelers, who desire to collect a large number of birds for comparison, will find this method one of great advantage; and the specimens will be better for study than skins, inasmuch as the proportions will be better preserved. Small mammals can be kept some days for skinning by a similar process, and an opening into the brain may be made through the roof of the mouth, if preferred. A Fox Squirrel, so treated, was in good condition for skinning after four day's preservation, in very warm weather.

Iowa College, July, 1870.

This with all similar methods of preparing permanent specimens, without skinning, has been found to be of comparatively little use in the damper air of the Eastern States, especially near the coast, where all dried preparations are so liable to mould and decay.—Eds.

3. *A Synopsis of the Family Unionidæ;* by Isaac Lea, LL.D., Vice-Pres. Amer. Phil. Soc., &c. 4th ed., very greatly enlarged and improved. 184 pp. 4to. Philadelphia, 1870. (Henry C. Lea).—Dr. Lea has given further completeness to his labors on the Unionidæ by preparing and publishing this fourth edition of his Synopsis. During the eighteen years that have elapsed since the issue of the preceding edition, the number of known species has much increased, and various corrections of former determinations have been made. The subject of the arrangement of the species in genera is discussed in the earlier part of the volume, and then tables are given, with very full synonymy and numerous annotations. The table of geographical distribution, which next follows, is very much enlarged. The volume closes with an Index of all the names of species and a statement of the place of publication of each, and finally a long Bibliography.

4. *Commensalism among Animals.*—**Van Beneden**, at the meeting of the Royal Academy of Belgium, on the fifth of March last, continued his observations on commensalism, or the living together of different species of animals. He first cited the following facts which he had received from Mr. Al. Agassiz. A species of *Lepidonotus*, of California, is always found attached on an *Asteracanthion* (*A. ochraceus* Brandt) near its mouth, on different parts of the ambulacral rays, sometimes five of them on a single individual. A small fish of the genus *Clupea* is often found lodged in the folds of the fringes of a species of Pelagia (*Dactylometra quinquecirra* Al. Ag.) A species of *Hirudinea* lives within the cavity of a Beroë (*Mnemiopsis Leydii*) of Buzzard's Bay, the Beroë never being seen without four or five of these worms, and often harboring seven or eight. Between the buccal fringes of the large Medusa *Cyanea arctica* (as stated in the Seaside Studies), a species sometimes 7½ feet in diameter, there lives an Actinia (*Biccidium* of L. Ag.); three to five of these Actiniæ reside commonly on each Cyanea. In an *Aurelia* of our coasts, a large number of Crustacea of the family Hyperina are often harbored. Another interesting fact is the commensalism of the young Comatulæ on the adult. The young of the species of the coast of South Carolina attaches itself to the basal cirri, and they live like a small colony of young Pentacrini. A species of Planaria, the *P. angulata* Müller, lives always in free "commensal" on the under surface of the American Limulus, near the base of the tail.

Van Beneden next speaks of the relation of the siliceous *Hyalonema* to the associated polyps (a species of *Polythoa*), and makes it as a case of commensalism of polyps on a sponge. He adds the fact that Mr. Oscar Schmidt has found in the Adriatic a *Polythoa* living on a species of sponge of the genus *Axinella*. He closes with repeating an opinion he had before expressed (in the *Zool. Médicale*, published long since by him in connection with *P. Gervais*) that sponges are only polyps of extreme simplicity, in which the active part is reduced to a membranous tube without tentacles about the orifice.—*L'Institut*, July 20.

IV. ASTRONOMY.

1. *Discovery of a new Asteroid, the* 111*th;* by Dr. C. H. F. **Peters**. From a communication by the author to one of the editors, dated, Litchfield Observatory of Hamilton College, Clinton, N. Y., Aug. 16, 1870.—I take pleasure in forwarding the first observation made upon an asteroid, the 111th of the group, discovered here night before last, viz:

	h m s	h m s	° ′ ″	
1870. Aug. 14.	12 38 31 H. C. m. t.	A.R.=21 25 20·21	Decl.=−13 10 5·4	10 Comp.
" 15.	9 5 40 " "	21 24 32·88	−13 12 29·6	5 "

The planet was of about 11½ magnitude.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Nineteenth Meeting of the American Association for the Advancement of Science, held in Troy, New York, August* 17–25, 1870.—The meeting of the American Association which has just closed at Troy, was, in point of interest and value, one of the best in its history. Owing to the continued illness of President William Chauvenet of St. Louis, the duties of President were performed by the Vice President, Dr. T. Sterry Hunt, of Montreal. And in the absence of Professor C. F. Hartt of Ithaca, the General Secretary, now in Brazil, Professor F. W. Putnam of Salem was elected to that office for the session. The attendance at the meeting was good, the names enrolled upon the Treasurer's book at the close of the session being about two hundred. The titles of papers entered with the Permanent Secretary numbered one hundred and forty-three. And, though there was no one subject of great general interest under discussion, as at Chicago and Salem, yet the absolute value of the papers read at Troy was quite equal to that of those presented at either of the meetings mentioned.

Of the papers read in General session, that by Lieut. C. E. Dutton, U. S. A., upon the Chemistry of the Bessemer Process, deserves particular mention. It was an admirable analysis of the process itself, and also of the successive stages which are noticed in the conversion. An abstract of the paper, by Professor Barker, is deferred to our next number.

The address of Col. J. W. Foster, the retiring President, was delivered on Thursday evening in the First Presbyterian church. It was upon "The Latest Investigations in Geology and Archæology, with special reference to the condition of Pre-historic man."

Among the various objects of interest in and about Troy, none attracted more attention than the various Iron-works. The proprietors of the Burden, the Albany, and the Rensselaer Iron-works, and of the Bessemer Steel-works provided special facilities for their inspection. Mr. A. L. Holley, to whose mechanical skill the wonderful machinery of the latter establishment is due, was particularly attentive and courteous. The U. S. Arsenal at Watervliet, the Rensselaer Institute at Troy, and many other places of interest received also their share of attention.

Two excursions were made from Troy; one to Albany by invitation of the Albany Institute, the other to Saratoga by invitation of the citizens of Troy. The time in Albany was divided between the Dudley Observatory and the Geological Hall. A sumptuous entertainment at the State Library closed the day's festivities. The Saratoga excursion occupied an entire day, dinner being served at Congress Hall at 4 P. M. The Association was the recipient also of various private hospitalities.

The Association voted to hold its next meeting in Indianapolis, Indiana,—in accordance with an invitation from that city—on the 17th of August, 1871. A committee was also appointed to arrange for the following meeting in San Francisco in 1872, upon invitation of the California Academy of Natural Sciences. The following officers were elected for the next meeting:

President, Prof. ASA GRAY, of Cambridge; Vice-President, Prof. George F. Barker, of New Haven; General Secretary, Prof. F. W. Putnam of Salem; Treasurer, Mr. Wm. S. Vaux, of Philadelphia.

The following is a list of the papers presented:

1. IN GENERAL SESSION.

1. On the Chemistry of the Bessemer Process; CLARENCE E. DUTTON.

2. Mrs. Willard's Theory of Circulation by Respiration: Mrs. A. L. PHELPS. (Read by J. G. Morris.)

3. Last Winter's occupation of Moosilauke Mountain in New Hampshire; J. H. HUNTINGTON.

2. IN SECTION A.

Mathematics, Physics, and Chemistry.

1. Some new applications of the Graphical Method; EDWARD C. PICKERING.

2. On Dispersion and the possibility of attaining perfect Achromatism; EDWARD C. PICKERING.

3. An Examination of the Doctrine of Atomicities; F. W. CLARKE.

4. The Isothermals of the Lake Region; A. WINCHELL.

5. A Description of a new apparatus for illustrating the Precession of the Equinoxes; JAS. BUSHEE.

6. The Magnetic Wells of Michigan; A. WINCHELL.

7. To whom is due the credit of the most important application of Steam as a Motive Power; CLINTON ROOSEVELT.

8. On Methods of illuminating optical Meteorology, particularly the formation of Halos and Coronæ. according to the theory of Bravais; JOSEPH LOVERING.

9. Researches in Electro-Magnetism; ALFRED M. MAYER.

10. Abstract of a research on a simple method of measuring Electrical Conductivities by means of two equal and opposed Magneto-electric Currents or Waves; ALFRED M. MAYER.

11. Unpublished experiments of Prof. W. B. Rogers on the Influence exerted by the presence of Carbonic Acid in gas, on its Illuminating Power; FREDERICK E. STIMPSON.

12. A Graphical Discussion of the various formulæ proposed for the relation between the quantity of light produced by the combustion of Illuminating Gas and the volume of gas consumed; FREDERICK E. STIMPSON.

13. Aurora Borealis; L. BRADLEY.

14. Cosmogony; L. BRADLEY.

15. The Northers of Texas; SOLOMON SIAS.

16. The Connection between Solar Spots, Terrestrial Magnetism, and the Aurora Borealis; ELIAS LOOMIS.

17. A Theory of the Constitution of the Corona of the Sun; SIMON NEWCOMB.

18. On the Assumption that Matter is Impenetrable; H. F. WALLING.

19. Elasticity, a mode of Motion: H. F. WALLING.

20. The conditions of Stable Equilibrium in Atomic Orbits: H. F. WALLING.

21. Spectroscopic examination of the Bessemer Flame; J. M. SILLIMAN.

22. Description of a new Meteorograph, for the automatic registration of Meteorological phenomena; G. W. HOUGH.

23. Remarks on the total fluctuation of the Barometric Column; G. W. HOUGH.

24. Relations existing between temperature, pressure, wind, and rain-fall, as indicated by automatic registering instruments; G. W. HOUGH.

25. On the rate of the Dudley Observatory Sidereal Clock for two years; G. W. HOUGH.

26. On a new form of mercurial horizon, by which vibrations are extinguished; J. H. LANE. (Presented by J. E. Hilgard).

27. Description of Batchelder's Arctic Tide-Gauge: J. E. HILGARD.

28. On proposed improvements for Common Roads; S. D. TILLMAN.

29. On a new Musical Notation; S. D. TILLMAN.

30. On improvements in Inland Navigation; S. D. TILLMAN.

31. On improved facilities for transmitting heat from one fluid to another; S. D. TILLMAN.

32. Gaseous and Liquid Rings; E. N. HORSFORD.

33. On the possibility of a Limit of Visible Magnitude; F. A. P. BARNARD.

34. On the brightness appearing on the limb of the Moon's Image, in Photographs of Solar Eclipses; F. A. P. BARNARD.

35. On the testimony of ancient Eclipses in regard to the Uniformity of the Earth's Rotation; J. N. STOCKWELL.

36. The Discovery of the force which originally imparted all their motions to all the stars; JACOB ENNIS.

37. On the Survey of the Northern Heavens instituted by the German Astronomical Society; T. H. SAFFORD.

38. On a New Method of determining the Level-error of the axis of a Transit instrument; C. A. YOUNG.

39. On Solar Prominences and Spots observed with the Spectroscope during the past year; C. A. YOUNG.

40. Some account of progress in the investigation of the laws of Winds; J. H. COFFIN.

41. Abstract of a paper on Temperature for twenty-five years; O. W. MORRIS.

42. The solvent power of anhydrous liquid ammonia; CHARLES A. SEELY. (Read by Prof. Walling).

43. The inadequacy of the prevailing Baconian system by Induction, and the fallacy of the too exclusive use of the *a priori* method; F. L. CAPEN.

44. On an improved form of Solar Eyepiece; S. P. LANGLEY.

45. The universal method of approximation; THOMAS HILL.

46. Note on the involute of a circle, and the analytical value of the hyperbolic base; THOMAS HILL.

47. Molecular classification; GEO. F. BARKER.

48. On the latest discoveries in regard to the manufacture of Ice by mechanical power; P. H. VANDER WEYDE.

49. Further improvements in the method of transmitting, audibly, musical melodies by the electric telegraph wire; P. H. VANDER WEYDE.

The following papers were read only by title:—

50. On Elasticity as a Feature in Physics; S. J. WALLACE.

51. On the Advancement of the Sciences; CLINTON ROOSEVELT.

52. On the present aspects of Organic Physics; HENRY HARTSHORNE.

53. Suggestions for systematizing chemical nomenclature; A. M. EDWARDS.

54. The relation between the bands of the Spectroscope and the musical scale; P. H. VANDER WEYDE.

55. The most important result obtained from the researches during some years of travel, consists in the establishment of the following theory:
The law of gravity is not the motive power of cosmic bodies; A. HABEL.

56. Gravity is not the principal motive power on terrestrial bodies, but acting very secondary; A. HABEL.

57. Past and Future Astronomy; JOSEPH TREAT.

58. Demonstration of the Perturbation of Uranus, which discovered Neptune; JOSEPH TREAT.

59. Universal Mathematical Demonstration of the impossibility of Gravity, or the Attraction of Matter as Matter; JOSEPH TREAT.

60. Corollaries of the Milky Way; JOSEPH TREAT.

61. An account of an Experiment upon the Physiological Action of Nitrous Oxyd; F. W. CLARKE.

62. Acid reaction of Tribasic Phosphate of Lime; E. N. HORSFORD.

3. IN SECTION B.

Geology and Natural History.

1. On the Early Stages of Discina; E. S. MORSE.

2. On the Organization of the Brachiopoda, (Discina and Lingula); E. S. MORSE.

3. The Brachiopoda as a Subdivision of the Annulata; E. S. MORSE.

4. Observations on Seedling Compass-plants (Silphium laciniatum); THOS. HILL.

5. The Development of Limulus Polyphemus; A. S. PACKARD, Jr.

6. The Terrace Epoch in Michigan; A. WINCHELL.

7. On the Relation of Organic Life of the several continents to the Physical Character of those land areas; N. S. SHALER.

8. On the character of the observations necessary to interpret the record of the last Glacial Period; N. S. SHALER.

9. On a method of collecting certain Geological facts adopted by the Social Science Association; N. S. SHALER.

10. Notes on the Condors and Humming birds of the Equatorial Andes; JAMES ORTON.

11. On the evidence of a Glacial Epoch at the Equator; JAMES ORTON.

12. On the Homologies of the Cranial Bones of the Primary Types of the Reptiles; EDWARD D. COPE.

13. On the Reptiles of the Triassic formations of the United States; EDWARD D. COPE.

14. On the existence of two classes of male flowers in the common sweet chestnut, and the influence of nutrition on the sex; THOMAS MEEHAN.

15. On objections to Darwin's theory of Fertilization through Insect agency; THOMAS MEEHAN.

16. On the law of fasciated branches, and its relation to the law of sex in plants; THOMAS MEEHAN.

17. The supposed Elevation and Depression of the Continent during the Glacial period; J. B. PERRY.

18. The Boulder-Trains of Berkshire county, Mass.; J. B. PERRY.

19. The Development and Old Age of the Tetrabranchiate Cephalopods; ALPHEUS HYATT.

20. The Genetic Relations of the Arietes; ALPHEUS HYATT.

21. The porphyries of Marblehead; ALPHEUS HYATT.

22. Geology and Topography of the White Mountains, N. H.; C. H. HITCHCOCK.

23. Description of a new Trilobite from New Jersey; C. H. HITCHCOCK.

24. The Distribution of Maritime Plants a proof of Oceanic Submergence in the Champlain Period; C. H. HITCHCOCK.

25. On the young of Orthagoriscus; F. W. PUTNAM.

26. On the salt deposits of Western Ontario; T. STERRY HUNT.

27. On Iron Sand Ores; T. STERRY HUNT.

28. Notes on Granitic Rocks; T. STERRY HUNT.

29. On the oil-bearing limestone of Chicago; T. STERRY HUNT.

30. On the Lignites of West America, their Distribution and Economic Value; J. S. NEWBERRY.

31. On the Sequence and Chronology of the Drift Phenomena in the Mississippi Valley; J. S. NEWBERRY.

32. On some new relics and traces of the Mound Builders; J. S. NEWBERRY.

33. On the relation of the Onedonta Sandstone and Montrose Sandstone of Vanuxem to the Hamilton and Chemung Groups; JAMES HALL.

34. Notice of the Fossil Plants of the Hamilton and Chemung Groups, with reference to the source of the sediments of these Formations; JAMES HALL.

35. Note upon the Rocks of the Huronian System on the Peninsula of Michigan; JAMES HALL.

36. Remarks on the occurrence of the genus *Dithyrocaris* in the Hamilton and Chemung Rocks of New York; JAMES HALL.

37. On the Geology of the Delta, and the Mudlumps of the Passes of the Mississippi. I. Geological Structure of the upper Delta plain. II. The lower Delta, and the Mudlumps of the Passes; E. W. HILGARD. (Read by J. R. Walker.)

38. Apatite Deposits of Lanark Co., Ontario, Canada; GORDON BROOME.

39. On some new generic forms of Brachiopoda, with remarks on some points of their structure; W. H. DALL.

40. On the order Docoglossa of Troschel; W. H. DALL.

41. On the nature of the foliage of Pines, etc.; a criticism; A. GRAY.

42. On the local Glaciers of the White Mountains; L. AGASSIZ. (Read by J. B. Perry.)

43. Notes on the relative age of the Niagara and so-called Lower Helderberg series; A. H. WORTHEN.
44. Probable Origin of the South Carolina Phosphates; W. C. KERR.
45. On some points in the Stratigraphy and surface Geology of North Carolina; W. C. KERR.
46. A point in Dynamical Geology; W. C. KERR.
47. Lava-ducts in Washington Territory; R. W. RAYMOND.
48. The Salt Marsh at Silver Peak, Southern Nevada; R. W. RAYMOND.
49. On Abnormal Vertebræ of the Elephant; SANBORN TENNEY.
50. On a new locality of Kyanite; SANBORN TENNEY.
51. On some points in the Geology of Eastern Mass.; SANBORN TENNEY.
52. Brief Notices on Hoosac Mountain and Tunnel; JAMES HYATT.
53. On the Relations of the orders of Mammals; THEODORE GILL.
54. On the subdivisions of the Branch Mollusca; THEODORE GILL.

The following papers were read by title only:—

55. Sketch of the researches made during seven years' travel through the five states of Central America, part of New Granada, the Republic of Ecuador and Peru; then on the Island of Chincha, and the Galapagos Archipelago; M. A. HABEL.
56. On the occurrence of native iron, not meteoric; H. B. NASON.
57. On the composition of the American Opium; H. B. NASON.
58. On parallel striæ in quartz crystals; L. FEUCHTWANGER.
59. Geology of the Cottonwoods Mining District of the Utah Territory; P. A. CHADBOURNE.
60. The Geysers of Iceland and California; P. A. CHADBOURNE.
61. Guano, the origin of the Apatite of Rideau, Canada; E. N. HORSFORD.
62. Fresh Water Pond Overlying a Salt Water Pond in Middlesex county, Mass.; E. N. HORSFORD.
63. Evidence of glacial action in the placer and gulch gold of California; E. N. HORSFORD.
64. Lakes and Lake Regions; S. J. WALLACE.

4. IN SUB-SECTION C.

Microscopy.

1. Microscopic Circuits of Generation: *a.* Of Zymotic Fungus; *b.* Of the (nominal) Genera of Fresh Water Algæ, as development-phases of Bryaceæ, etc.; *c.* Of Vorticello-Planarians; T. C. HILGARD. (Read by J. E. Hilgard.)
2. On a new form of Binocular Microscope; F. A. P. BARNARD.
3. On the Structure of the scale of Podura plumbea; F. A. P. BARNARD.
4. On the Illumination of Binocular Microscopes, with proposal for a new Diaphragm-stop; R. H. WARD.
5. Remarks on Stereoscopic vision as applied to the Microscope; R. H. WARD.
6 Some remarks on Nobert's lines, with particular reference to Dr. Woodward's photographs; R. H. WARD.
7. Some remarks on a Pocket Microscope and Telescope combined; JOSIAH CURTIS.
8. Some remarks on two deposits of Diatomaceous earths recently thrown up by the Sea; EDWIN BICKNELL.
9. Remarks on a method of producing very low power with the microscope, with demonstration; EDWIN BICKNELL.

5. IN SUB-SECTION E.

Archæology and Ethnology.

1. Observations of the stone used by the Indians within the limits of Massachusetts in the manufacture of their implements, with some remarks on the process of manufacture; JAMES J. H. GREGORY.
2. The Substitution of General Laws for Special Legislation; GEO. A. LEAKIN.
3. On some peculiarities of the Grammar of the Eskimo Dialects; W. H. DALL.

4. Sign-Language as illustrative of the Laws of Written and Vocal Language; G. W. Samson.

5. Remarks on customs of the Waribiara and Blanco Indians in the vicinity of Chiriqui Lagoon, Central America; J. A. McNiel.

2. *On "Pure Scarlet;"* in a letter from Wm. H. Dall (letter to Prof. Silliman dated Smithsonian Inst., Washington, D. C., May 3, 1870.)—In 1863, being engaged in making anatomical drawings from living microscopic animals, I purchased among other colors, a cake made by Windsor and Newton of England stamped with the name "Pure Scarlet." It was of the finest and most vivid scarlet, far superior in brilliancy to any of the various shades of vermillion, &c., which are commonly sold in the shops.

I had, however, no occasion to use the color until 1865, when I colored a lithographic plate (on ordinary paper used for such purposes) of *Pentstemon Cerrosensis* Kellogg, putting in the darker shades with carmine.

In the summer of 1865, at Sitka, Alaska Terr., I made some drawings of Æolis and other nudibranchiate mollusks, which had streaks of bright scarlet upon various parts of their bodies and tentaculæ. These were sent by mail to the Smithsonian Institution where they remained until my return from the north in 1868.

In 1866 being in San Francisco I chanced to examine the plate which I had colored the winter before, and noticed a fading of the colors, but not considering the matter particularly, I recolored the plate with the same pigment and left it in perfect condition.

Having returned to Washington, during the spring of 1869, I used the cake of "pure scarlet" again, for coloring a map (upon stout, thin, map-paper) to show the distribution of the Indian tribes. The color was laid on thickly and uniformly over the entire surface of two or three of the divisions of the map. As this lay on my table for convenient reference I noticed that it began to fade rapidly, and in the course of a few months the paper was left perfectly white and clean, not a trace of the heavy, opaque pigment remaining. This recalled the fading of the colored plate to my mind, and upon examination I found that the latter was in the same condition, not a particle of scarlet remaining, and the only color upon the plate being the streaks of carmine which had been put on by way of shading and which were unchanged.

I had believed the drawing of the nudibranchs to have been lost, as I could find it nowhere, but now a more careful search revealed the fact that the scarlet having entirely faded away, I had failed to recognize it. It was made on the best rough drawing paper of English make.

An examination of the substance showed it to be soluble in iodid of ammonium and hyposulph. soda, and with nitrate of silver in solution it gave iodid of silver, hence I suppose it to consist wholly or in part of iodid of mercury. I enclose a fragment for examination. The supposition that it was acted upon by hyposulph. soda in the paper is doubtful, in view of the fact that three kinds of paper, one of them made expressly for water color drawings, gave the same results.

As the pigment is an exceedingly attractive one, liable to be used by naturalists in valuable drawings, it may not be out of place to call their attention to its ephemeral properties.*

3. *Professor Marsh's Rocky Mountain Expedition. Discovery of the Mauvaises Terres formation in Colorado.*—Prof. Marsh and party left New Haven on the 30th of June for Omaha and beyond. By a letter from him dated Cheyenne, August 4th, we learn that he had already made a successful expedition to the Loup Fork, and obtained large collections of specimens, but they were prevented by the Indians from going on to the Niobrara region. The following is the copy of a later letter to J. D. Dana, dated Pine Bluffs, Wyoming Territory, Aug. 12th:

The Scientific Expedition from Yale College, while recently examining the geology of Northern Colorado, discovered an extensive outcrop of the true Mauvaises Terres or White River formation, at a point nearly 200 miles south of the region where it had been previously identified. The locality first detected, which contained all of the characteristic fossils of this deposit, was on one of the branches of the Little Crow Creek, about five miles south of the Wyoming State line. The strata there observed consisted of at least 150 feet of light-colored clays, overlaid by sandstones and conglomerates about 200 feet in thickness. The lower portions of the clays are the true Titanotherium beds, containing many remains of *Titanotherium Proutii.* Above these are similar clay deposits corresponding closely in age with those on the White River, and marked by abundant remains of *Oreodon Culbertsoni*, *Testudo Nebrascensis*, *Helix Leidyana*, and many other fossils, which characterize that horizon. Associated with these were found several new species of mammals and birds.

This interesting series of fresh-water Tertiary strata lies almost horizontal, dipping apparently, but very slightly, toward the northeast. It probably forms the southwest border of the great Miocene lake-basin, east of the Rocky Mountains, which is so remarkable for its extinct animal remains. Our party traced the same formation, with its more common fossils, about thirty miles northeast into Wyoming, along the hills known as Chalk Bluffs, and still farther north in the Pine Bluff range. We hope soon to examine it at other points.

4. *Glaciers of Scotland.*—Prof. Croll, in a memoir on the "Boulder-clay of Caithness," in the Geological Magazine for June, 1870, takes the ground that the boulder drift of Scotland is due mainly to glaciers, and not to icebergs, and points out the direction of the movement of the great glacier, not only over Scotland, but over the seas north and east, illustrating the subject by a map. He finds an argument for its glacier origin in the fact that the drift is *unstratified*, remarking that depositions from icebergs would necessarily be more or less stratified by the waters in which they fall. The argument is one that cannot be set aside,

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* The color sent by Mr. Dall is, as he supposes, *mercuric iodid*, well known to chemists as very volatile, changing at a gentle heat from scarlet to nearly white, and the latter by friction returning again to scarlet.—S.

and holds as well for New England and the rest of North America as for Scotland. Prof. Dana, in his recent paper on New Haven Geology, draws the same distinction between the drift that was deposited by glaciers over the land, and by glaciers over waters.

Dr. Robert Brown also presents the glacier theory for Scotland in an article read before the Geological Society of London on June 22, entitled—On the Physics of Arctic Ice as explanatory of the Glacial remains in Scotland.

5. *Meteoric Irons.*—The Museum of Vienna has recently received an iron meteorite from Atacama having a maximum diameter of a third of a meter, and weighing 51 kilograms. The iron contains about 6 per cent of nickel.

Tschermak has studied the meteoric iron which fell at Lodran, near Moultan in India, on the 1st of October, 1868, a fragment of which was sent to the Vienna museum by Mr. Oldham. Niccoliferous iron constitutes about 32 per cent of it; and but for the iron, the meteorite is identical with terrestrial chrysolite rocks. It contains crystals of bronzite and bluish-gray chrysolite, and of magnetic pyrites and chromic iron. The chrysolite contains 12 per cent of protoxyd of iron.—*L'Institut*, July 20.

6. *Fall of a Meteorite in Stewart Co., Georgia.*—Prof. J. LAWRENCE SMITH states in a letter to Prof. Brush, dated Louisville, Ky., July 18th, 1870, that a meteoric stone fell in Stewart county, Georgia, in October, 1869. He adds that a description of its fall and phenomena by Prof. Willet, and an analysis of it by himself, will be sent for the next number of this Journal.

7. Prof. HELMHOLTZ takes the chair of Physics at Berlin made vacant by the death of Magnus. He will enter upon his new duties in April next.—*Nature*, July 28.

Prof. J. WATSON has been awarded, by the Paris Academy of Sciences, the Astronomical Prize, Lalande foundation, for the discovery of eight new asteroids in one year.

8. *On the occurrence of a Peat bed beneath deposits of Drift in Southwestern Ohio.*—The author of the article on this subject, at p. 54, is Professor EDWARD ORTON.

9. *Woodward on the Magnesium and Electric Lights for Photo-micrography.*—The illustrations of this article, on pages 298 and 301, of the last volume of this Journal, should be transposed.

OBITUARY.

JACOB P. GIRAUD, Jr., the ornithologist, died at Poughkeepsie on Tuesday, July 19th, in the fifty-ninth year of his age. A particulr friend of Audubon, Bachman and Wilson, and a member of the New York Lyceum of Natural History since 1840, he made his favorite science of ornithology his special study. In 1841 he published "A Description of Sixteen New Species of Texas Birds," and in 1844, a more valuable work on "The Birds of Long Island." His collection of North American Birds, one of the most complete in the country and containing many of Audubon's types, he presented to Vassar College. In addition he bequeathed to Vassar

College the sum of thirty thousand dollars to endow the professorship of Natural History, and two thousand dollars to complete his cabinet of North American Birds.

Mr. A. H. **Haliday**, of Antrim, Ireland, an entomologist of distinction died on the 13th of July, near Lucca, Italy. He graduated at Trinity College, Dublin, in 1822, at the age of 15, and five years afterwards as a gold medalist.—*Nature*, July 21.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *The Andes and the Amazon, or across the Continent of South America;* by **James Orton**, Prof. Nat. Hist. in Vassar Coll., Poughkeepsie. 356 pp. 12mo. New York, 1870. (Harper & Brothers).—Prof. Orton made his journey through South America in the years 1867–68, under the auspices of the Smithsonian Institution, with Messrs. P. V. Myers and A. Bushnell of Williams College, and others. His course from Paita, Peru, was by Guayaquil to Quito, and thence over the mountains to Napo; thence along the Napo river to the Amazon and Para at its mouth. The book of travels which he has given to the public is pleasantly written, and is full of facts and animated descriptions relating to the interesting region passed through—its general features and people, its climate and resources, its mountains and volcanoes, its landscapes and tropical productions. It contains a map of the part of South America from the Pacific to the Atlantic in the latitudes of the Amazon and its tributaries, which appears to have been prepared with care, and also several excellent woodcuts of scenery, etc. The party had not the time or instruments for extended physical investigations. Some of the observations of Prof. Orton have appeared in this Journal (vol. xlvii). Specimens were collected in zoology and botany, and some of fossils, and these have been put into the hands of different naturalists for description. Among the geological facts we observe the discovery of tertiary fossils at Pebas, on the Amazon, in long. 72° W., at a height of 345 feet above the sea; the fossils have been described by Mr. Gabb in the American Journal of Conchology.

2. *The Chemical History of the Six Days of Creation;* by **John Phin**, C.E. 96 pp. 12mo. New York, 1870.—This work is not, what its title seems to imply, a discussion by a chemist of the chemical history of the earth's genesis. It treats briefly of the bearing of some of the general principles of science, chemical, physical, geological and astronomical, on the interpretation of the first chapter of Genesis. The aim of the work is good. The views with regard to the "works" of the successive days accord in the main with those that have been presented by Prof. Guyot, and may be read with profit. There are also ideas peculiar to the author which we cannot commend. For example, he says that the "evening" of the "second day" resulted "from the cooling of the ashes of the great combustion;" that the "evening" of the "fifth day" resulted from the separation of Venus from the sun;

and the "morning" from "the restoration of the brilliancy of the sun;" the "evening" of the sixth day "from the separation of Mercury from the sun," etc. The author by bringing more exact science to bear on the subject and a wider philosophy will be enabled to prune his work of some manifest defects.

3. *The American Chemist: a Monthly Journal of Theoretical, Analytical and Technical Chemistry.* Edited by CHAS. F. CHANDLER, PH.D., and W. H. CHANDLER. New York, 1870. (William Baldwin & Co., 434 Broome St.)—The American reprint of the Chemical News having been given up, the subscription-list and stock of that reprint have been purchased by Mr. Baldwin, who starts the American Chemist to take its place, putting the editorial charge into the hands of Prof. Chandler and his brother. While we regret that a journal of the recognized ability of the Chemical News cannot be continued in this country, we believe that the well-known scientific reputation of the editors of the American Chemist is a guarantee that their new enterprise will not be second to it in value. The first two numbers have been received; i. e., those for July and August. They contain 32 articles, of which 17 are taken from the Chemical News, and eight are original. For the present, the admirable "Chemical Notices from Foreign Sources" printed in the Chemical News, will be retained. It is announced that after the close of the current year, the American Chemist will be conducted as an entirely independent journal. The subscription price is $5.00.

4. *On the Gulf Stream, and the Thermometric knowledge of the Atlantic Ocean and adjoining lands in the year* 1870; by Dr. A. PETERMANN. 64 pp. 4to, with three large colored temperature charts. Gotha, June, 1869. (Justus Perthes. From Petermann's Geogr. Mittheilungen, 1870, Heft 6–7.)—Dr. Petermann has here given an admirable exposition of the former and present state of knowledge of the temperature of the Atlantic and the Arctic seas and the lands adjoining, including the later results of the most recent expeditions. Deep sea temperatures, as well as the saltness of the ocean, and much detail with regard to special localities, come within the wide range of subjects under review. The first two charts contain the isotherms for every 2° Reaumur for the region between the meridians of 75° W. and 60° E., north of the parallel of 35°; the first, the isotherm for July and the second for January; and the third includes 5 charts of the arctic regions, giving the isotherms severally for January, July, the winter, the summer, and the isotherms of absolute minima for every 10° R. Eight pages are devoted to the instructions given to the second German North-polar Expedition, 1869–1870.

5. *Roasting of Gold and Silver Ores, and extraction of their respective metals without Quicksilver;* by G. KUSTEL. 145 pp. 12mo. San Francisco, 1870. (Dewey & Co.)—This little treatise contains an introductory chapter on the classification of silver ores, which is followed by a chapter on *roasting*, divided into sections, treating, respectively, of the chloridizing and the oxyd-

izing roasting of silver ores. The author then proceeds to give the different methods for the extraction of silver by lixiviation and subsequent precipitation; and, finally, a brief chapter on the extraction of gold by the chlorination process. The book contains a large amount of valuable facts expressed in a very compact manner, besides embracing much original matter, the results of the experience of the author and his friends in the Pacific States and in Mexico. It forms an excellent supplement to Mr. Kustel's previous work on the "Nevada and California processes of Silver and Gold extraction."

Bulletin of the National Association of Wool Manufacturers, founded Nov. 30, 1864. No. 1 of this Quarterly Bulletin appeared on January of 1869, and the first number of vol. ii in Jan. 1870. The numbers contain about 100 pages.

Report on the Coals and Iron Ores of Pictou County, Nova Scotia; by Edward Hartley, F.G.S., Mining Engineer to the Geological Survey; being an Appendix to Reports on the Pictou Coal-Field. From the Report of the Geological Survey of the Dominion of Canada, for 1867–69. 78 pp. 8vo. Montreal, 1870.

Index to the Literature of Uranium, by H. Carrington Bolton. Ann. Lyc., New York, Feb. 1870., pp. 362-377.

Uebersicht der im Königreiche Sachsen zur Chausseeunterhaltung verwendeten Steinarten; zusammengestellt von Dr. H. B. Geinitz, K. Professor, and C. Th. Sorge. K. Oberbaurath. 116 pp. 4to. Dresden, 1870.

Paris Universal Exposition.—The following additions have been made to the list of Publications of Reports connected with the Paris Universal Exposition, given on page 258 of the last volume of this Journal:

Extracts from the Report of the International Committee on Weights, Measures and Coins, with a notice of the use of the Metric System in the United States, and its relations to other systems of Weights and Measures.

Report on Clothing and Woven Fabrics, being Classes Twenty-seven to Thirty-nine of Group Four; by Paran Stevens, U. S. Commissioner. 86 pp. 8vo.

Report on Education; by J. W. Hoyt, U. S. Commissioner. 398 pp. 8vo.

Bibliography of the Paris Universal Exposition of 1867; by Wm. P. Blake. 40 pp. 8vo.

Introduction, with selections from the Correspondence of Commissioner-General Beckwith and others, showing the organization and administration of the United States Section.

Proceedings of the Acad. Nat. Sci. Philadelphia, No. 1, 1870.—Page 1, Supposed Sivatherium from Colorado; *J. Leidy.*—p. 3, 4, 5, Reptile remains from Colorado; the Alabama Cretaceous; Fort Bridger Tertiary (a turtle named Baptemys Wyomingensis); *J. Leidy.*—p. 5 and 11, Fossil Birds of the U. S. Cretaceous and Tertiary; *O. C. Marsh.*—p. 6, Analysis of the nickel ore of the Gap mine, Lancaster Co., Pa.; *Wharton.*—p. 8, Mylodon from Central America; *J. Leidy.*—p. 9, Note on the Triassic Dromatherium sylvestre of Emmons; *id.*—p. 9, Note on Elasmosaurus; *id.*—p. 11, Dicotyles antiquus, from N. J. Miocene; *O. C. Marsh.*—p. 12, Ichthyodorulites; *J. Leidy.*—p. 13, Megalonyx Jeffersoni and Bison antiquus in Illinois; *id.*—p. 13, Growth of wood in Yucca: *J. Meehan.*—p. 14 Cross fertilization and the law of sex in Euphorbia; *id.*—p. 15 Relations of Synocladia of King to Septopora of Prout; *Meek* and *Worthen.*—p. 18, On Discosaurus and its allies; *J. Leidy.*—p. 22, New fossils from Illinois; *Meek* and *Worthen.*—p. 56, New fossils collected by the U. S. Coast Survey under Clarence King; *F. B. Meek.*

Proceedings of the California Acad. Sci., Vol. IV, Part ii, 1870.—Page 48, Discovery of a nearly entire skeleton of a Mastodon near Petaluma.—p. 50, Shells of Antioch, Cal.; *H. P. Carlton.*—p. 57, Shells of Truckee River and vicinity; *id.*—p. 61, Fauna of California and its geographical distribution; *J. G. Cooper.*—p. 82, 83, Indian mounds.—p. 86, Shell mounds; *L. Ransom.*—p. 88, Section of rocks in Hamilton, Nevada; *J. G. Clayton.*—p. 89, Rise of water in Mono Lake.—p. 90, Explorations in the Rocky Mountains; *J. D. Whitney.*—p. 92, the West-coast Fresh-water Univalves, No. I; *J. G. Cooper.*

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[SECOND SERIES.]

ART. XXVIII.—*On the Examination of the Bessemer Flame with Colored Glasses and with the Spectroscope;* by J. M. SILLIMAN, M.E., Adj. Prof. of Metallurgy, Lafayette College, Easton, Pa.

[Read at the Troy Meeting of the Am. Association for the Advancement of Science.]

I. *Examination with Colored Glasses.*

IN the Bessemer process, the progress of the decarbonization is determined chiefly by the appearance of the smoke, flame and sparks which are emitted from the apparatus. Owing to the rapidity with which the reactions take place, it is highly important to catch the exact moment when the blast should be turned off. This is indicated by the color and brightness of the stream of gas issuing from the converter, and by this the moment of total decarbonization can generally be accurately determined by the naked eye. When, however, pig-iron of certain qualities is used (manganiferous iron, for example) this determination is very difficult; even those who have had much experience make frequent mistakes and find it impossible to produce the same quality of steel at every blow.

In order to intensify these flame-indications, use has been made of the spectroscope, and also of various combinations of colored glasses. The former was first attempted by Dr. Roscoe, and the latter by Mr. Rowan at the Atlas Works.

Mr. Rowan experimented with a great variety of colored glasses and obtained the best results by using three glasses, two of ultramarine blue and one of dark yellow. This little instrument, or chromopyrometer, as he terms it, is now in daily use at the Atlas Works, its indications being so marked and unmistakable as to render its use safe in the most inexperienced hands.

The following experiments were made at the Bessemer Steel Works of John A. Griswold & Co., in Troy, while pursuing the chemical course in the Winslow Laboratory of the Rensselaer Polytechnic Institute. In my observations on the flame I made use of the spectroscope, and also of a combination of colored glasses. This combination consisted of two light-yellow glasses and a blue one, through which the sunlight appeared of a deep purplish-blue tint; and as it differed slightly from Rowan's, it gave somewhat different results.

In order to reproduce the appearance of the flame at the different stages of the process, I prepared a plate consisting of about a hundred varieties of colors and tints, all of which were numbered and thus referred to a table which indicated their composition. They were also arranged to be seen with either a light or dark background. The use of this plate was of necessity limited to daylight, but the illustration and description are given as occurring at night in order to show its illuminating power.

At the beginning of the process that which issues from the converter does not appear to be a true flame, but only an illumined stream of gas carrying with it innumerable red-hot pellets of iron. This gas has scarcely any illuminating power, extends but a short distance from the mouth of the converter, and is sometimes sheathed with a whitish smoke. Seen through the glasses the flame and sparks have a deep crimson color, the converter is invisible, and at the base of the flame is a crimson band which continues throughout the process.

As the reaction continues, this stream of gas grows brighter and more elongated, and after a few minutes a small pointed whitish flame appears, which suddenly increases in size. At this instant the blast-pressure falls from twenty to eighteen pounds.

When viewed through the glasses the upper part of the converter comes dimly into view, and the flame and pellets of iron appear of a lighter color, while the fragments of slag which begin to be thrown out are of a deep red. This difference in shade between the iron and slag thrown out is probably entirely owing to the lower temperature of the latter, for the reason that while the iron is discharged from the metallic bath the slag is washed up on the sides of the converter, and can be seen clinging around its mouth in a spongy mass until detached and thrown out by the blast. The greater porosity of the slag and its consequent more rapid cooling would also cause a difference of temperature.

In the second period the discharge of slag increases, and the flame is very bright and illuminating, with occasional dark streaks. Through the glasses at the beginning of this period the flame is of an ashy blue color with streaks and flashes of crimson; the edges being sometimes of a purplish hue. At this point surrounding objects are illuminated, and the converter becomes distinctly visible. A wreath of crimson is seen surrounding the flame where it strikes the chimney. By the middle of this period the crimson almost entirely disappears from the body of the flame, leaving only a slight cone at its base, and a border of greenish hue makes its appearance, and gradually grows more decided. Streaks of a dark blue color are also seen in the body of the flame.

The beginning of the third period is scarcely indicated to the naked eye, though the flame becomes somewhat weakened, and after a few minutes shows dark streaks running through it. Through the glasses at the commencement of this period the rose-colored cone begins to expand and deepen, the greenish sheath is more decided, while streaks of dark and green are visible. After a few minutes the change becomes very rapid, a few seconds only being required to reduce the flame from rose-color to the deep crimson non-illuminating gas, as at first, and again the converter is lost to view, by which time the blast should have been turned off.

The gradual fading of the crimson from the beginning of the blow and its deepening at the termination of the process, as well as the crimson band at the base of the flame and the wreath of crimson surrounding the flame at the chimney, tend to confirm Mr. Rowan's views, which are, that the different shades of crimson are due to changes of temperature. The stream of gas which comes from the mouth of the converter at the beginning of the process being illumined from within, derives its color from the metallic bath, the temperature of which, owing to the combustion of silicon, increases more rapidly during this period than at any other.

The crimson band at the base of the flame and the wreath of crimson at the chimney might also be accounted for by this theory. The flame rushing from the mouth of the converter has a tendency to create a vacuum at its base around the converter's edge, and thus to cause a wreath of flame to pass over this surface and by consequent cooling produce the crimson band. The wreath of crimson at the chimney may be also due to the cooling of the flame consequent upon deflection.

It is true we have a seeming contradiction to this theory in the rose-colored cone extending from the base at the center, which we would naturally consider the hottest part of the flame; but, as in the flame of the Bunsen burner, the hottest part is in its outer sheath, the conditions of combustion in both being similar, it is probable that that part of the flame occupied by the cone is at a lower temperature than that surrounding it.

The green streaks in the flame are most intense when the manganese spectrum is brightest; and as the color of the flame when the spiegeleisen is added is also green, we are led to suppose them due to the presence of manganese.

On two occasions simultaneous observations were made with the spectroscope and the colored glasses; but with the exception of that just mentioned, and the changes at the commencement and termination of the blow, no striking coincidence was noticed.

II. *Examination with the Spectroscope.*

The science of spectrum analysis is yet in its infancy, and there has been no scientific investigation, perhaps, which has been more contradictory in its results than that of the Bessemer flame. The first application of the spectroscope to the analysis of the Bessemer flame was made in 1862 by Dr. Roscoe at the works of Messrs. John Brown & Co., in Sheffield. Soon after this it was in constant use in Brown's works for controlling the process. It was next introduced at Crewe, and from there said to have been taken to Seraing, in Belgium, in 1865.

Roscoe's account of the general appearance of the spectrum has not altogether been verified by subsequent observers. His not having seen any line beyond 80° indicates an imperfection in his instrument. He, also, is the only one who claims to have seen the sodium line as an absorption band, or who professes to have detected the lines of nitrogen and hydrogen in the Bessemer spectrum. His spectroscope was so arranged that the spectrum of the Bessemer flame was seen in the upper half of the field of view, while the spectrum with which it was to be compared was seen immediately below. The spectrum of the flame was thus compared with the following spectra:—

1. Spectrum of electric discharge in carbonic oxyd vacuum.
2. “ “ strong spark between silver poles in air.
3. “ “ “ “ “ iron “ “ “
4. “ “ “ “ “ “ “ “ hydrogen.
5. Solar spectrum.
6. Carbon spectrum—oxyhydrogen blowpipe supplied with olefiant gas and oxygen.

The coincidences observed were very few, and totally failed to explain the value of the Bessemer spectrum. The lines of the well-known carbon spectrum did not occur at all, either as bright lines or absorption bands, nor was any coincidence observed between the lines of the Bessemer spectrum and those of the carbonic oxyd vacuum tube. The lines of lithium, sodium and potassium were strongly marked and identified with certainty. He found that three fine, bright lines between E and *b*, shown on the plate at 66½°, 67°, 67½°, coincided with those of iron; and in place of the red hydrogen line C, he discovered a black band which he considered an absorption-band, and states that it is better defined in wet than in dry weather.

In Austria, Prof. Lielegg followed up this subject with great perseverance, and gave more extended accounts of the varying character of the Bessemer spectrum during the different stages of the process. His experiments were made at Gratz, where the spectroscope was afterward used with great success in controlling the Bessemer process; but at Königshütte, where dark gray manganiferous iron was used, it was found that the indications which in other works so plainly determined the moment of decarbonization were unreliable. In this case, the lines whose disappearance is to indicate the exact point of time for ending the process, disappear too soon. During the period in which the spectrum is brightest, among the glowing vapors and gases that stream from the converter, carbonic oxyd next to nitrogen is most abundant, and it is for this reason that the first investigator, Roscoe, expressed himself as confident that the numerous lines of the spectrum were caused by this gas, although he could obtain no coincidence.

Brunner* states that "no part of the Bessemer spectrum is ever visible in the flame when the converter is heated for the first time after being re-lined, but that when the lining is not new, Lielegg's group of green lines ($CO\gamma$) appears in the spectrum, which then contains also the lines of potassium, sodium, and lithium." From which he concludes that this spectrum is not to be identified with carbonic oxyd, but must be produced by other constituents of pig-iron. Others state that the Bessemer spectrum is sometimes visible while the converter is being heated after a blow. I made an observation of the flame from the converter while it was being heated the first time after being re-lined, and obtained with great distinctness the potassium, lithium, and sodium lines, but have not under any circumstances detected any other lines while the converter was being reheated.

Lichtenfels, by a series of simultaneous comparisons of the manganese with the Bessemer spectrum found the lines in the blue and green fields to completely harmonize in the two spectra. The violet manganese line which had been seen by some he could not detect in either of the spectra. I have never observed it, but Dr. Wedding, who has summed up the observations of others, states that he has repeatedly seen it. Its position is at 135½°.

The instrument used in my investigations was constructed by Alvan Clark of Cambridge, and consists of an equiangular flint-glass prism, in a metallic box, into the sides of which at the requisite angles are screwed an inverting telescope with a magnifying power of six, and a tube containing the adjustable slit and lens for rendering the rays parallel; also a tube with a scale, which is placed at such an angle that it is reflected from the surface of the prism through the telescope to the eye; it can be so adjusted as to appear along the upper edge of the spectrum. I was provided with Bunsen's plates of spectra on a large scale, and in order to adapt them to the scale in my instrument, I took the spectrum of the sun and obtained Fraunhofer's lines with great distinctness. Two characteristic lines in the solar spectrum were then noted, one of which appeared at 37° and the other at 117°, and a space measured equal to their distance apart as given on Bunsen's scale. This was divided into eighty equal parts, and the division extended in both directions. By the application of this scale to Bunsen's, I found that the remainder of Fraunhofer's lines in my instrument exactly coincided with their position on his plates. The correctness of the new scale was also proved by other coincidences. By moving the prism, Fraunhofer's lines will vary slightly in their relative distances apart, but in no possible position in which I could place the prism could I obtain the sun-spectrum as given by Wedding in connection with the Bessemer spectrum; if the spectrum given by him was obtained by the use of bisulphid of carbon in his prism, that substance causes a greater variation than I had supposed.

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* Van Nostrand's Eclectic Eng. Mag., vol. i, page 508.

I have recorded the results of twenty-five observations on the Bessemer flame, most of which were taken at a distance of about thirty feet from the flame, though I have stationed myself at intermediate points between that and the flame; at one time sitting so close as to be almost scorched. Nearly all my observations were made at night and the lines obtained much better defined than when seen in diffused sunlight.

The record of my observations was kept as follows:—Five columns were ruled, headed—

Degree.	Color.	Brightness.	Time.	Remarks.

Note was made of the dark bands as well as the bright ones, both of which were classed according to their distinctness, as very bright, bright, faint, and very faint. In the time-column was noted the number of minutes after the commencement of the blow at which the lines appear.

At the first two or three observations I attempted to make a thorough note of the changes as they occurred throughout the whole spectrum, but afterward abandoned it as utterly impossible, as at the beginning of the second period, the lines come in so fast and the changes are so rapid that they can not be accurately noted at the exact moment of their occurrence. I therefore confined myself to a few degrees at each observation, and by this method was enabled to note accurately, and at the exact moment of their occurrence slight changes which otherwise might have escaped notice. Note was also taken of the changes in the general appearance of the whole spectrum during the successive stages of the process. After having made half a dozen observations, while viewing the spectrum of the flame from the converter while it was being heated for another charge, it was discovered that a movement of the eye before the eye-glass occasioned a similar movement of the lines of the spectrum along the scale, on which their position could thus be made to differ more than half a degree. I have seen no notice of this in the statements of others, and it may account for some of the apparent discrepancies. Thereafter, when taking the readings of any of the lines, the position of the eye was so adjusted as to bring the sodium line exactly at 50°. Owing to the extreme brilliancy of the flame the aperture may be made exceedingly narrow, and thus the many lines of the spectrum which with a duller light and broader gauge would be blended together, may be separated.

At the beginning of the blow, the spectrum is continuous and very faint, and generally extends from 35° to 120°, covering about three-fourths of the length attained in the second period. This increases slightly in extent and brightness until the appearance of the sodium line. This line appears at the end of the first period at the beginning of a more decided flame. It comes flashing through from one extremity to the other for an instant, and then disappears only to return the next instant in brighter flashes which are continued for about a minute, by which time the line becomes permanently established. On one occasion the sodium line, instead of flashing and disappearing as usual, continued visible after a few seconds, and expanded and contracted in width almost isochronously until it became permanently established. The appearance of this line indicates the termination of the first period. This period I have found to vary in extent from three to seventeen minutes in blows lasting from thirteen to twenty-seven minutes. None of the other lines make their appearance in vivid flashes as does the sodium. The lithium line becomes visible three or four minutes after the first flash of the sodium. It is very faint at first but soon becomes quite distinct and lasts through the blow. The vivid flashing of the sodium line may be accounted for by the exceedingly small amount of sodium required to produce its spectrum—an amount not exceeding $\frac{1}{180000000}$ of a grain. The slightest momentary combustion taking place in the stream of gas from the converter, would at that instant render glowing a sufficient amount of the vaporized sodium to produce its spectrum, and thus occasion the flashes so characteristic in the first appearance of that line. Lithium exists in a much smaller quantity and requires $\frac{1}{6000000}$.

of a grain, or thirty times that given for sodium. By the time the lithium line is established the red potassium line at 23½° and occasionally the violet line at 135° appear, and the blue and green fields become divided into bands which are so rapidly resolved into bright and dark lines, that it is difficult to note the exact time of the appearance of each. The spectrum increases to a dazzling brightness, and extends itself in both directions until it reaches from 23½° to 140°.

During the third period the spectrum becomes more brilliant, and the lines more distinct. Several new lines make their appearance in different parts of the spectrum, of which the ones at 51½°, 57°, and 67° are well defined, while others are faint and not always visible; some of them appearing only toward the close of the last period. In viewing the lines in the most refracted part of the spectrum, it has been repeatedly observed both by myself and others, that these lines were more strongly marked when entering the eye at an angle than when viewed directly. That this was not imagination is proved by repeated identification of lines at the same point on the scale.

At the termination of the blow the lines are rapidly swept away, sometimes in the inverse order of their appearance, but more generally they disappear within the space of two or three seconds, leaving a continuous spectrum as at first, though somewhat brighter. Sometimes the sodium and lithium lines are swept away with the others, and at other times they remain visible. In either case the change is very decided, and does not generally occupy more than three seconds. In the course of my observations, thirty-three lines have been detected, as given in the table below.

Some of the lines given by Lielegg I have failed to find, but have detected others not given by him.

1st Period, 23½, 35, 50, 135.
2d Period, 23½, 35, 43, 44, 44½, 45½, 46, 47½, 48½, 50, 52, 53, 56, 56½, 61½, 62, 62½, 63, 65, 66½, 67½, 70, 72, 120, 135.
3d Period, 23½, 35, 43, 44, 44½, 45½, 46, 47½, 48½, 50, 51½, 52, 53, 56, 56½, 57, 61½, 62, 62½, 63, 65, 66½, 67, 67½, 70, 72, 100, 102, 103, 105, 108, 135.

Among the dark bands detected, the most intense occurred at 44–46, 51–55, 56–58, 62–64½; others were found at 33–34½, 36½, 37½, 38½, 40, 68–72.

Many of the dark bands were crossed by bright lines.

I have repeatedly observed the dark band considered by Roscoe to be a hydrogen absorption line, but have not noticed that its intensity varied with the dampness of the weather. Whether it is an absorption band or not can be determined by a series of observations continued through wet and dry weather. If this proves to be a hydrogen line, the Bessemer spectrum will be found more complicated than is generally supposed. It has been thought by some that the dark bands in the spectrum are absorption lines due to the cooling of the outer sheath of flame, but it is more probable, that although the pellets of iron and slag tend to produce a faint continuous spectrum; yet in contrast with the very brilliant lines it appears discontinuous, the dark bands being merely intervals between the bright ones. The iron spectrum has not been satisfactorily identified. It has been suggested that the brightness and size of the lines of the Bessemer spectrum do not allow the iron lines to appear. In comparing the Bessemer spectrum with Bunsen's spectra of nickel, cobalt and calcium, no coincidences were observed except two or three in the latter spectrum. The brightest calcium line, however, was not visible in the Bessemer spectrum. The Bessemer spectrum contains yet many mysteries to be solved, among which is the cause of the non-appearance of the lines of the spectrum at the beginning and termination of the blow.

This was readily solved when the numerous lines of the spectrum were attributed to carbon, but in proving them to be caused principally by manganese, their disappearance is not so readily accounted for.

One theory to account for it is that the luminous power of the flame is too small at the beginning and end of the process to produce a spectrum. In regard to this it may readily be shown that the brilliancy of the spectra of incandescent metallic vapors does not depend upon the illuminating power of a flame but upon the heat of the flame into which they are introduced. For instance, the spectra are more distinct in the non-luminous flame of a Bunsen lamp than in the ordinary luminous gas-flame. If we take the theory as referring to the feebleness of light given off by those substances in the flame which produce the spectrum, it will resolve itself into the one of change of temperature, notwithstanding the fact that the illuminating power of flames of the same temperature varies with the composition of the gas, because there is evidently enough sodium in the flame to give its characteristic line; hence, whatever might be the illuminating power of the flame, if the heat is sufficiently intense the sodium line *will* show itself.

Dr. Wedding adopts the theory that the absence of the spectrum at the beginning and termination of the blow is because the absolute quantity of the bodies volatilized producing the spectrum is at these times too small. His reasons for holding this view are as follows:—"A trace of sodium will give its characteristic line, but, according to Simmler, a much larger quantity of manganese is needed to obtain a recognizable reaction than that which can be detected by the well known blow-pipe reaction with carbonate of soda. Consequently, spectrum analysis does not depend alone upon the *presence* of a body but also upon the presence of a certain quantity. And although manganese is always left in the iron, it may not be left in sufficient quantity at the termination of the blow to produce the spectrum, and for this reason the lines disappear."

To this theory there are same some strong objections. *1st.* If we take manganese in sufficient quantity and hold it in a flame the spectrum will increase in brightness until a uniform temperature is attained; but when the amount of manganese vaporized begins to diminish, its spectrum will gradually decrease in brightness until it disappears. Now, if the disappearance of the manganese lines in the Bessemer spectrum is owing to the diminution of the quantity of manganese, we should infer that these lines would gradually grow more indistinct and then fade away; but on the contrary, the manganese spectrum increases in brilliancy from its first appearance, and is more intense just before being swept away than at any other time. The analysis of the smoke, which appears when the flame ceases, proves that a considerable quantity is still volatilized, and it is notable that in manganiferous iron this quantity increases towards the close of the blow. *2nd.* It would be more difficult to account by this theory for the non-appearance of the sodium line at the beginning of the blow, as sodium then in all probability exists in the issuing gas in sufficient quantity to produce its spectrum at a high temperature, as it is only by special precaution that we can keep it out from any flame. *3rd.* A still greater difficulty would arise in applying this theory to the spectra of sodium and lithium at the close of the blow. As has before been stated, these lines sometimes disappear at the moment of complete decarbonization, and sometimes remain. In the former case, to say that our friend sodium had given out would be doing great injustice to that element, as it has never given us reason for bringing so grave a charge against it. Dr. Wedding in attempting to demonstrate that the non-appearance of the manganese lines is owing to the lack of sufficient quantity volatilized to produce its spectrum, makes the following statements:—

From analyses made by Brunner we find that the manganese contained in the iron falls from 3·460 per cent in the raw material, to 1·645, 0·429, and finally to 0·113 per cent in the decarbonized product; and that the protoxyd of manganese in the slag first increases from 37·00 per cent to 37·90 per cent, and then sinks to 32·23 per cent, and furthermore, that a certain quantity of manganese is to be found in the smoke. How much manganese is really lost by volatilization cannot be determined, since data are wanting as to the absolute quantity of slag and iron, consequently we cannot determine how much manganese has been lost by means of the eruptions.

But since the manganese contained in the pig-iron decreases constantly, and that contained in the slag after the termination of the boiling period also decreases, a considerable volatilization of this body is probable just at the time when the spectrum is best developed. Comparing with this the experiments that can be made in the laboratory we arrive at the hypothesis, that the oxydized manganese which has entered into the slag is not volatilized but is retained by the slag; it can, therefore, get into the flame only in the shape of solid or fluid combinations.

In the above statements the results of the analysis prove that some of the manganese in the slag is volatilized. We cannot consider the manganese spectrum during the entire process as due wholly to the volatilization of the manganese directly from the iron, for while the amount eliminated from the iron grows continually less, the manganese spectrum grows brighter. Owing to the intimate mixture by the blast of the iron and slag, the manganese oxyd contained in the latter, is brought in contact with the melted iron and vaporized. This mixing of the slag and iron would cease at the termination of the process, and this would account for the sudden diminution of smoke.

If there was a sufficient carbonic oxyd flame to render the escaping gases glowing it is evident they would not issue from the converter as dark smoke, but as incandescent vapor having its characteristic spectrum. The lack of sufficient flame may, therefore, account for the disappearance of the manganese spectrum. The Bessemer flame presents other problems, and opens an intensely interesting field for scientific investigation; and by the use of more delicate instruments than have yet been employed for this purpose, discoveries may be made which will throw new light upon the subject of spectrum analysis.

ART. XXIX.—*On a simple method of measuring Electrical Conductivities by means of two equal and opposed magneto-electric currents or waves;* by ALFRED M. MAYER, Ph.D.

[Read before the Troy meeting of the American Association for the Advancement of Science.]

1. *General description of the Method.*

A MAGNET is firmly supported in a horizontal position with a portion of its length projecting beyond a fixed stop (see fig. 2); over this free end of the magnet, and resting against the stop, are placed two similar flat spirals, formed of the same quality of copper wire, and having the turns of one spiral in a direction the reverse of those of the other. The spirals are clamped together and their four terminal wires are carried vertically downward into four separate cavities containing mercury; these mercury-cups are so connected with a reflecting-galvanometer that, when the spirals are together slid off the magnet, the two equal electric currents, thus generated, simultaneously tend to traverse the galvanometer in opposite directions, and therefore its needle remains stationary. If we now introduce into the circuit of one of the spirals a resistance equal to that introduced into the circuit of the other, the needle will still remain at rest when the spirals are slipped off the magnet; but, if the resistance placed in one circuit is greater or less than that placed in the other, there will be a deflection of the galvanometer needles when the spirals are removed. Thus, by introducing wires of different metals into the circuits we can readily determine their relative conductivities, by making them of such length that their resistances are equal; which condition is attained when, on sliding off the spirals, the needle remains absolutely at rest. If, in the latter case, the wires have equal diameters then their conductivities are directly and their resistances are inversely as their lengths.

A modification of the above method is discussed in the conclusion of this paper; in which the magnet is replaced by the terrestrial magnetic force and the spirals and the wires by two similar coils, from two to three feet in diameter, formed of the two wires whose conductivities are to be compared. These coils contain equal lengths of the same sized wires and the same number of turns; the direction of the turns being opposed in the two coils. The coils having been bound together are placed in a plane at right angles to the line of "the dip," and the four terminal wires are so connected with the reflecting-galvanometer that the two induced currents tend to traverse it in opposite directions. The coils are now quickly rotated through 180°, around an axis at right angles to the line of the dip, and if the wires present equal resistances the needle remains at rest; if it is deflected, the direction and the amount of the deflection shows which coil has the lesser resistance and affords a means of estimating the same.

After this general description of the method I will present, in order, a description of the apparatus used, and of the actions which take place in it; the degree of precision of the method; examples of the determinations of electrical-conductivities, and experiments on the modification of the method.

2. *Description of the Apparatus.*

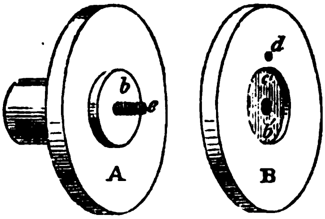
The magnet was formed of a combination of three steel bars, separated from each other by slips of wood ·2 in. thick. The middle bar was 10·4 in. long and its ends projected ·25 in. beyond the two side magnets. Each bar was ·27 in. thick and ·9 in. wide. About three months before this investigation was undertaken they had been magnetized to saturation by the following process. The axis of a helix, 8·7 ins. long and containing 558 feet of $\frac{1}{16}$ in. copper wire, was placed in the line of "the dip" and a current so sent through it from ten Bunsen cells that its N. pole was toward the earth. The separate bars were then drawn through the helix until they ceased to acquire an increase of magnetism. This method gives a uniform and powerful magnetization, and probably may be improved by causing the bars to vibrate as they pass through the helix; which can be accomplished by means of a tuning fork furnished with a long brass stem. After the magnets were combined, as described above, a weight of 1·5 lb. was sustained at the end of the middle bar.

The magnet was supported in an E. and W. line, 15·5 in. above the surface of the mercury in the cavities of the wooden "connecting-block" placed below it; and 2·5 in. of its S. end projected beyond the wooden clamp which held it.

The Spirals were formed of $\frac{1}{26}$ inch "double-covered" Lake Superior wire. Each spiral contained 176·06 in. of wire coiled in 20 turns, and the terminals were 15·5 in. long, thus making 207·06 in. of wire in each spiral. The greatest diameter of the spirals was 3·9 in. and each had a central opening of 1·7 in. Their thickness after they were covered with paraffined paper and varnished was ·06 in. The covering of the two terminal wires of each spiral was saturated with melted paraffin; they were then firmly tied together with silken cord to about ·4 in. of their ends where they separated and formed forked terminations.

The spirals were formed in this manner. An iron plate A, which screws on to the mandrel of a lathe, has cemented on to its face a disc of hard wood *b*, 1·7 in. in diameter and ·1 in. thick. From the center of the plate A projects a screw *e* which enters the wooden disc B at *e'*. When the plate B is screwed "home" the disc *b* fits into the cavity *b'* and the plates A and B are separated to a distance a little greater than the diameter of the covered wire, while the disc *b* forms a cylinder between them on which to wrap the spiral.

1.



The end of the wire to be coiled is passed through a hole *d* in the plate B, which is then screwed home on to A. The lathe is then turned so that the wire is coiled over the center disc from A to *b*. After the space between the disc is filled with coils, the free end of the wire is secured and the plate B unscrewed, while the wire slides through *d* and the coil is not unwrapped; which would have taken place if it had been coiled in the direction from *b* to A. The spiral is now saturated with very fluid paraffin and has cemented on to it, with a hot chisel, a paper disc previously saturated with paraffin. The spiral is now removed, the covered side placed against the disc and its other surface treated in the same manner. The spiral is then taken off the chuck, and on holding it up to the light the copper wire is distinctly seen through the translucent covering of the wire and the paraffined paper cover of the spiral. The insulation thus obtained is very perfect and the coils are firmly cemented together. The terminals are now led radially from the spirals, and are tightly bound together as described above. To still further strengthen the spirals, both they and their terminals are covered with a firm layer of shellac varnish.

I have thus minutely described the process of making these spirals for they are of inestimable value in many electrical researches; having been used in my recent investigations in electro-magnetism, and will be again used and referred to in a subsequent communication.

The galvanometer I specially constructed for this research, but experience has shown that a coil of shorter and thicker wire, (say, $\frac{1}{16}$ in. wire in 6 turns) offering less resistance, would have been better than the one employed. The wire of the galvanometer was $\frac{1}{32}$ in. thick, and was wrapped around the lower needle in two layers of 22 turns each; the opening of the coil being ·15 in. in width. The needles are 1·65 in. long and ·03 in. diam. The upper needle is ·6 in. above the lower with a thick copper plate intervening; it was not much affected by the current in the coil, and was under the influence of a feeble magnet, 13 in. long and ·18 in. diam., which was placed with its similar poles over the upper needle and 8·2 in. above it. Under these conditions the simple oscillations of the system were exactly 4½ per minute, and by lowering or raising the magnet I could render it more or less astatic. The needles were hung by a frame of fine copper wire to a plane mirror 1·2 in. square, formed of thin glass silvered by Foucault's process, and the whole was suspended by a few fibers of unspun silk. The instrument was enclosed in a cover, the front of which was made of a carefully selected piece of plate glass.

The S. end of the magnet used in inducing the electric currents in the spirals was 14 in. to the left of the magnetic meridian line drawn through the point of suspension of the galvanometer needles and 6 ft. 11 in. distant from the same. In this position the magnet caused a deflection of 52′·5 in the needles of the galvanometer and they were brought back into the meridian by means of the damping-magnet.

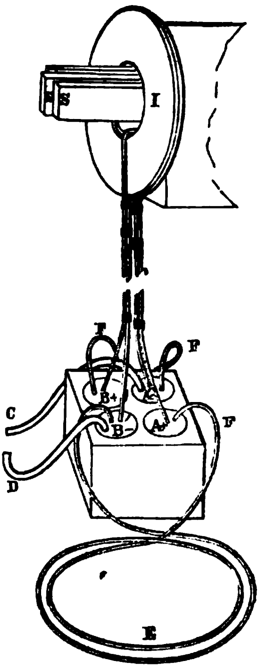
The deflections of the needles were read off by the beautiful method invented by the illustrious Gauss, using a telescope and scale placed as described below.

The telescope was of ·7 in. aperture and 12 in. focus; just under its object glass was placed, at right angles to its axis, a rod of wood 1 in. square and 1 meter long, covered with drawing paper. This rod was divided off into centimeters by lines 1^{mm} thick; thus, the division lines were $\frac{1}{10}$ of the distance between two similar sides of the centimeter divisions. A thick spider thread was selected which just covered a division line, and therefore was also, apparently, 1^{mm} thick. By this simple device,—using one and the same side of the spider line as point of reference,—we can accurately estimate deflections of the needles corresponding to $\frac{1}{10}$ of a division of the scale.

The scale was 2·285 meters distant from the center of the mirror, and therefore a motion of 1 division of the scale over the spider thread corresponded to an angular deflection of 7′ 30″, and as we have seen that $\frac{1}{10}$ of a division can be accurately read, it follows that we can determine a deflection of 45″. In this paper I will give the deflections in divisions of the scale, which can be converted into minutes of arc by multiplying them by 7·5.

Connecting-block is the name I give to the block of wood, placed under the projecting end of the magnet; it has four cavities containing mercury, by means of which we make the various electrical connections required in the experiments. Fig. 2 gives a view of this block and shows the manner of making the connections when the object is the measurement of relative electrical resistances. Four holes, 1 in. in diam. and 1 in. deep, separated by walls ·1 in. thick are bored out of a block of wood, and then coated with thick shellac varnish. A, A′ are the terminal wires of one spiral, B, B′ those of the other. The wires to be compared are at E and F. If E represent the standard wire of a fixed length, then the wire F has to allow of its length being altered so that its resistance may be made equal to that of E. This is arranged by sliding one end of this wire through a heavy copper clamp (not shown in the fig.) which is fixed in the mercury-cup B, while the other end, previously well amalgamated, dips into cup A′.

2.



The two spirals I are firmly clamped together, so that when slid quickly off the magnet S, they both have the same direction of motion. By referring to fig. 2, the directions in which flow the currents, thus produced, can be readily followed. Taking spiral A A′, the current flows down the terminal A+ through the wire E into B′, thence by the wire D through the galvanometer and back by the wire C to A′−, the other leg of the spiral, thus completing the circuit. In the case of the spiral B B′, the current flows down the terminal B+ through the wire F, thence by the wire C through the galvanometer; returning by D to B′, thus forming the circuit.

It is evident that if the spirals by themselves generate equal currents, and the resistances E and F are equal, no deflection of the needles will ensue, for equal and contrary currents will tend simultaneously to traverse the galvanometer.

The manner in which contacts are made in these experiments is of great importance. The double silk covering of the wires is unwrapped to ·2 in. from their ends; the unwrapped silk is then firmly wound over the end of the silk covering and saturated with thick shellac varnish. The uncovered end of the wire is now scraped, rubbed with nitrate of mercury, and well amalgamated, up to the silk covering. The iron wires were amalgamated by dipping their uncovered ends into sodium-amalgam. Thus, even if the end of the wire should dip deeper than ·2 in. into the mercury, the point of contact will yet remain at that distance from the end, as the shellac prevents contact above the amalgamated portion of the wire. The terminals of the spirals and of the galvanometer coil were formed in the same manner. One end of the wire whose resistance was to be compared to the standard copper wire, was uncovered and well cleaned for some portion of its length, so that it could be drawn through the heavy copper clamp until its length equalled in resistance the standard wire. The wires were then removed and their lengths accurately measured.

3. *Investigation into the actions which take place in the apparatus.*

In the general introductory description given of the method, I have, for simplicity of illustration, assumed that when the two spirals,—similar as to form, length of wire and resistance,—are slid off the magnet, no current would be sent through the galvanometer. But this cannot be, for the hinder spiral is further on the magnet than the other by ·06 in. and therefore cuts more "lines of magnetic force," and also, the two spirals traverse simultaneously portions of the field differing in magnetic intensity.

The following experiments will exhibit the above action. I will call the back and front spirals respectively A and B.

They were reversed on each other, clamped together and had their terminals dipping in A and A′ so that the two induced currents would tend to traverse the galvanometer coil in opposite directions.

The mean of three experiments shows that when the spirals are slid off there is a deflection of 2·43 div. in favor of spiral A. But, on making spiral B the back spiral, the needle moved 3·9 div. in its favor; thus showing that spiral B offers less resistance than A, though both lengths were contiguous pieces taken from the same sample of wire. On again placing A against the stop, I found that a resistance of 2·9 in. of $\frac{1}{32}$ in. wire, when attached to one leg of this spiral, reduced its action to equal the forward spiral and the needle remained absolutely unaffected when the spirals were quickly removed from the magnet.

The following experiments show the effects of separating the spirals. The balanced spirals remaining, in other respects, as in the last experiment, I separated them ·05 in. by intervening card-board; the needle was now deflected 1·05 div. in favor of the back spiral, and on increasing the separation to ·125 in. the action of the back spiral equalled 3·6 div. of the scale.

I have said above that the two opposed currents *tend* to traverse the galvanometer coil, because theoretic considerations induce me to hold the opinion that two currents cannot simultaneously traverse a wire in opposite directions, and that only the excess of the intensity of one current over the other is really propagated through the wire.

The next point to be considered is the mutual inductive action of the spirals. The directions of the turns in the spirals being opposed, and as the current in each, on sliding them off the magnet, rises rapidly to a maximum intensity and as quickly comes to 0, it follows that they must exert a mutual inductive action. But although the current in one spiral during the rise to its maximum causes an induced current in the other spiral in the same direction as that induced in it by the magnet, yet, as the current *decreases* as quickly to 0 after it has reached its maximum, it follows that a current in the opposite direction to that induced by the magnet in the other spiral will now quickly follow it, and as these currents, +, and − are equal, there will be no increased outside effect arising from their interaction; and many experiments showed that whether a copper disc was placed between the spirals or an equally thick disc of paper, the action at the galvanometer was the same.

The following experiments on this subject appear to confirm the above view. The two spirals were placed on the magnet, but only the front one was connected with the galvanometer, while the terminals of the back spiral were separated so that no current went through it when the spirals were together slid off the magnet. The action of one spiral alone was sufficient to deflect the galvanometer needle about 60°. This deflection was reduced to 1° 18′ by placing in its circuit a helix of 735 ft. of no. 18 copper wire; the mean of six experiments (the *range* of which was only $\frac{1}{10}$ div.) giving 10·4 div. The ends of the back spiral were now so connected that an equal current flowed through it in a direction the reverse of the other. The mean of six deflections, produced by sliding together the spirals off the magnet, equalled 10·4 div., the same as in the previous experiment; thus showing that the mutual inductive action of the spirals had no effect on the intensity of the induced magneto-electric currents.

It was also found that on passing the induced current from a spiral through another spiral on which rested a third spiral whose ends were connected with the galvanometer, that no deflection ensued when the magneto-electric current was passed through the inducing spiral.

However, the magneto-electric currents were of such low intensity that probably they were not able to produce an induced current in the second spiral capable of deflecting the needle, and that therefore the experiments here narrated are of little value; nevertheless, I think the reasoning given above will be supported by experiments made with more powerful magnets and with larger spirals.

4. *The degree of Precision of the method.*

The degree of precision of this special apparatus was determined in the following manner. A copper wire 123 ins. long had opposed to it a resistance which was about equal to 120 ins. of its length and the mean deflection of the galvanometer-needles was carefully determined. The copper wire was now shortened 1 in. and the deflection again determined; this was repeated,—determining the amount of deflection produced after each shortening of 1 in.,—until 6 in. had been cut off. These experiments showed that a diminution or increase of resistance of $\frac{1}{120}$ part in one of the wires caused a deflection of ·4 div. of the scale, or of 3′ of arc, in the galvanometer-needles. But we have seen that ·1 div. can be read on the scale, therefore, we can, with this special apparatus, detect and measure an increased or diminished resistance of $\frac{1}{480}$ part. But as the galvanometer can be removed to even twice the distance at which we read its deflections, I think I am safe in saying that with this method, as applied with the above apparatus, we can measure a difference of resistance in two conductors of $\frac{1}{500}$ part; which is far within the variations observed in different samples of wires of the same lengths and diameters.

If a galvanometer formed of 6 or 8 turns of ·1 in. wire were used in connection with a powerful magnetic battery and larger spirals of thicker wire, while the galvanometer is placed at a greater distance, I have no doubt that a variation of $\frac{1}{1000}$ part can thus be detected and measured.

5. *Examples of the determinations of electrical conductivities by this method.*

The object of these determinations was not to furnish science with new and accurate data,—for that would have required a careful personal supervision of the operations of preparing chemically pure metals,—but it was to give examples setting forth the practice of the method.

I had prepared "hard-drawn" wires, of No. 18 B. W. G. (=·049 in. diam.), of copper, silver, iron, and German silver. These wires were found to have the same diameter. They were all covered with a double wrapping of silk.

Silver.—The spirals were balanced, by the introduction of an increased resistance in the back-spiral, so that no deflection took place on sliding off the spirals. A length of 120 in. of the silver wire having been placed in the circuit of one spiral, it was found that 127 in. of copper wire were required in the other circuit, in order to equal it in resistance. Taking the copper wire as the standard of comparison, at 100, we have

$$127 : 120 :: 100 :: 94{\cdot}48.$$

Matthiessen (Phil. Trans., 1858, 1862) makes the ratio of the conductivity of silver to copper, both hard-drawn, as 100: 99·95 or about equality; but in my determination the silver is 5·5 per cent below the copper. I therefore suspected impurities in the silver, and an examination of the wire kindly made by my colleague, Dr. Wetherill, showed that it contained about ·01 per cent of gold and a trace of iron. This accounts for the low number found, and affords a good illustration of Pouillet's remark, that the purity of a metal is most readily determined by a measure of its electrical conductivity. The electrical test of purity, however, exceeds in delicacy the chemical examination; for a very minute percentage of alloy causes a great increase of resistance, and if we could be sure that the wires we compared were in the same physical condition as to annealing or hardness, we could probably use this method as a means of determining the percentage of a *known* metal which formed the alloy.

Pouillet shows (Traité de Physique, 1856, vol. i, p 606) that silver whose conductivity is 100 when pure, is only 51 when it contains ·037 of alloy, and is 47, 42 and 39 when it contains respectively ·100, ·143, and ·253 of alloy. Pure gold gave 39, but ·049 of alloy reduced its conductivity to 13; and Jenkin has found that an alloy of 1 part of silver and 2 of gold presents almost as much resistance as German silver.

Iron.—The three following determinations were made of the conductivity of the best quality of iron wire relatively to the standard copper wire.

(1) The resistance of 240 in. of copper wire = 36·7 in. of iron wire.
(2) " " " 111·6 " " " " = 16·16 " " " "
(3) " " " 60 " " " " = 8·67 " " " "

Giving for the relative conductivity of iron,

(1) 240 : 36·7 = 100 : 15·29
(2) 111·6 : 16·16 = 100 : 14·48
(3) 60 : 8·67 = 100 : 14·45

14·74 = Mean.

E. Becquerel (Ann. de Ch. et Phys., III, xvii, 266) gives 13·6 for the conductivity of iron, copper being 100; and both wires hard-drawn; while Matthiessen (Phil. Trans. 1858, 1862) determines 16·81 as the conductivity of iron, copper being 100, and both hard-drawn.

The mean of Becquerel and Matthiessen = 15·20
My determination = 14·74
Difference = ·46

The copper and iron wires in (3) were cut off from the lengths used in (2); but the wires used in (1) were taken from parts of the coils removed from the lengths (2) and (3). This accounts for the close agreement of (2) and (3) and the higher number obtained in (1).

My determination therefore appears to compare favorably with those made with different methods by these experimentalists. I say "appears," because although the copper was of excellent quality and the iron the best procurable, yet they were not chemically examined as to their purity.

Another series of determinations was obtained by comparing the lengths of copper and of iron wires which would equal in resistance one and the same length of German silver wire, used as a term of comparison. The result agreed with the above determinations.

6. *On a modification of the method.*

As long ago as 1832 Faraday (Exp. Res. 170–180) first obtained an electric current, directly induced by the earth's magnetism, by rotating a closed wire circuit around an axis at right angles "to the line of the dip;" an experiment whose theoretic beauty has ever been the admiration of natural philosophers.

A length of 38 ft. of $\frac{1}{16}$ in. insulated copper wire was wound into a coil of 3 ft. in diameter, containing 4 turns. The terminals of this coil were connected by binding screws with the wires leading to the galvanometer and the coil placed in a plane at right angles to the line of the dipping needle. On quickly rotating the coil through 180° the needles were deflected 25°, and by making the rotations correspond in direction and time with the oscillations of the needle, I found that six rotations brought the deflection to over 45°. Faraday (Exp. Res. 202–213 and 3145 *et seq.*) has shown that the intensities of the magneto-electric currents induced in wires of different metals are as their electrical conductivities, therefore a coil of iron wire similar in all respects to the above copper coil will give a deflection of about 4° for the first rotation; but by increasing the number of turns of the coil to 10 or more and by using a galvanometer with a shorter and thicker wire coil, the angle of deflection can no doubt be doubled.

The above facts show that we can substitute for the steel magnets, previously used, the magnetism of the earth, and can replace the spirals by two similar coils made of the two specimens of wire to be compared. The coils are placed on each other so that their convolutions are in opposite directions; and having been firmly tied together their plane is made to coincide with a direction at right angles to the dipping needle, while their terminals are so connected with the galvanometer that the currents induced in the two coils tend to traverse it in opposite directions.

Things being arranged as above, it is evident,—as the wire coils are similar in all other respects,—that if the conductivities of the wires are the same, there will follow no deviation of the galvanometer needle when the coils are quickly rotated through 180°; but if the wire of one coil offers a greater or less resistance than that of the other the needle will be deflected. By ascertaining what differential actions correspond to known differences of conductivity of coils of a certain diameter, number of turns and thickness of wire, we can, by always using similar coils in these relative measures, ascertain what difference in relative conductivity corresponds to a certain angle of deflection; the chords of these angles, or, the sines of half of the angles, being to each other as the intensities of the currents.

Minute differences of resistance in the two coils may be made to cause a deflection in the galvanometer needle by knowing the time of its oscillation, and by reversing the motion of rotation of the coils so as to correspond to the swing of the needle; thus after several reversals a motion is given to the needle which could not have been observed after a single rotation.

In point of ready application,—and especially in reference to the determination of the resistances of lengthy conductors,—I doubt whether this method will be generally adopted; but after the conception of the idea it appeared worth investigating; this

I have done, and have thus developed at least any value it may possess. It certainly presented an interesting problem and the pleasure afforded in its solution has repaid me for the considerable labor which it required.

S. Bethlehem, Pa., July 15, 1870.

ART. XXX.—*On the supposed absence of the Northern Drift from the Pacific Slope of the Rocky Mountains;* by DR. ROBERT BROWN, M.A., F.R.G.S., etc., Edinburgh, Scotland.

IN some interesting remarks addressed to the California Academy of Sciences on the 4th of June, 1868, and published in their 'Proceedings' for that year (vol. iii, pp. 271, 272), Professor J. D. Whitney denies that there is any true Northern Drift within the State of California. "Our detrital materials," the learned Professor remarks, "which often form deposits of great extent and thickness, are invariably found to have been dependent for their origin and present position on causes similar to those now in action, and to have been deposited on the flanks and at the bases of the nearest mountain ranges by currents of water rushing down their slopes. While we have abundant evidence of the former existence of extensive glaciers in the Sierra Nevada, there is no reason to suppose that this ice was to any extent an effective agent in the transportation of the superficial detritus now resting on the flanks of the mountains. The glaciers were confined to the most elevated portions of the mountains, and although the moraines which they have left as evidences of their former extension are often large and conspicuous, they are insignificant in comparison with the detrital masses formed by aqueous erosion. *There is nothing anywhere in California which indicates a general glacial epoch during which ice covered the whole country, and moved bodies of detritus over the surface* independently of its present configuration, as is seen through the Northeastern States."

Mr. Whitney goes on to observe that the same condition of things prevails in Nevada and Oregon, the detritus seeming always to be accumulated at the base of the mountains. Further, from the observations of Messrs. Ashburner and Dall, he remarks that "it would appear that no evidences of a Northern Drift have yet been detected on this (Pacific) coast, *even as far north as British Columbia and Russian America* (Alaska). Neither of these gentlemen has observed any indication of a transportation of drift materials from the north toward the south, or any condition of things similar to that which must have existed in the Eastern States during the diluvial epoch." Mr.

W. H. Dall, the gentleman referred to in the foregoing extract, (and well known to the readers of this Journal as one of the most active and observant of the staff of Naturalists attached to the Collins Overland Telegraph Expedition), follows suit to these observations of Professor Whitney by declaring, in a paper published in this Journal for January, 1868, that though he had carefully examined the country over which he had passed, in Alaska, for glacial indications, he had not found any effects attributable to such agencies. His own opinion, indeed, from what he had seen of the west coast, *though yet unproved*, was that the glacier-field never extended in these regions to the westward of the Rocky Mountains, though single glaciers have existed and still exist between spurs of the mountains which approach the coast. No boulders, according to Mr. Dall, such as are common in New England, no scratches or other marks of ice action had been observed by any of his party, though carefully looked for.

It is this general theory of the absence of the Northern Drift in Northwestern America that I propose combating in the remarks which follow, and I do so with extreme diffidence, knowing well from old experience the care and caution with which Prof. Whitney has proceeded in his remarkable geological survey of California, as well as in his earlier work on the shores of Lake Superior. For this reason I will speak only of what I know from personal knowledge of the districts visited by myself, calling in, however, the observations of others as corroboration of my statements.

As far as Alaska and California, and even Oregon and Washington Territory, are concerned, I must leave the question of glacial remains within their boundaries, to observers more intimately acquainted with their country than I am, though I have a strong inclination to believe that what I say about other portions of the Pacific coast will hold equally good regarding them also. I have certainly visited and traveled through California, and have been in some portions both of Oregon and Washington Territory, and on the borders of Alaska, yet my knowledge of these countries does not entitle me to dispute statements so explicitly made by such excellent observers as those cited. But with the coast of British Columbia and the whole of Vancouver Island I am very intimately acquainted—perhaps more intimately than any other single individual—and can speak positively regarding the marked presence of true Northern Drift there, so that with every respect to the opinion of so distinguished a geologist as Prof. Whitney, I am compelled to dissent from his theory regarding the entire absence of glacial remains proper, from the Pacific slope of the Rocky Mountains.

Between 1863 and 1866—nearly four years—I traveled on foot and in canoes, through the forests, over the mountains, on the rivers, the lakes and the prairies of the whole of the region indicated, as Commander and Government Agent of the First Vancouver Exploring Expedition, and as Botanist of the British Columbia Expedition. Again I have three times visited the Arctic Regions, passing a whole summer in Greenland, studying these and other similar phenomena, and have for many years been very familiar with the remains of the Northern Drift in Scotland, the north of England and portions of the north of Europe. These personal particulars are mentioned to show that I am in a position to know glacial remains when found, and to distinguish them from the ordinary terrestrial debris accumulated by causes now in action in the temperate countries where formed. The result of these extended observations has, therefore, been to confirm me in an opinion entirely contrary to that expressed by Messrs. Whitney, Ashburner and Dall, viz: that so far from the Northern Drift being absent from Vancouver Island and British Columbia, it is present in as marked a manner as ever I saw it in countries celebrated for the presence of such remains. This opinion I casually expressed in 1869 in a memoir entitled *Das Innere der Vancouver Insel*, published in the volumes of Petermann's *Geographische Mittheilungen* for that year, and more recently and more explicitly, in another, *On the Physical Characteristics and Geographical Distribution of the Coal Fields of Northwest America* (Transactions of the Geological Society of Edinburgh, 1868–69). As that statement has been inclined to be called in question—scientific sceptics not unreasonably considering that a doctrine promulgated by so eminent a geologist as Prof. Whitney is entitled to further consideration, than a mere curt denial of its truth, I have considered it proper to present in a concise manner in this place the facts on which I base my disbelief in its truth. As early as 1860, Mr. Henry Bauermann, geologist of the British Northwest Boundary Commission, made many observations on this subject; and subsequently, in 1862, in a *Prize Essay on Vancouver Island, its resources and capabilities as a Colony* (Victoria, 1862), Dr. Charles Forbes, R.N., published similar facts, which my own researches have only tended to confirm and enlarge, over a greater area. Dr. Forbes showed, what is familiar to every one visiting that section, that in the whole southern portion of the Island, though from the open prairie-like character of some portions of the southeastern section it is there earlier observed than in the wooded districts, the scooping, grooving, and scratching of the rocks by ice action is very marked. The chief rock *in situ* there is a dense, hard, feldspathic trap, and this is ploughed in many places into furrows six to eight inches deep, and from six to eighteen inches wide. The ice action is also well shown in the sharp peaks of the erupted, intruded rocks, having been broken off and the surface smoothed and polished, as well as grooved and furrowed, by the ice action on a sinking land, giving to the numerous promontories and outlying islands which here stud the coast, the appearance of rounded bosses between which the soil is found to be composed of sedimentary alluvial deposits, containing the debris of tertiary and recent shelly beaches, which have, after a period of depression, been again elevated to form dry land, and to give the present aspect to the physical geography of Vancouver Island.

The whole surface of the country is strewn with erratic boulders. Great masses of 60 to 100 tons in weight,—chiefly of various igneous and crystalline, as well as sedimentary rocks, sufficiently hard to bear transportation,—are found scattered everywhere over the island from north to south, and through the region lying on the western slope of the Cascade Mountains. Some of these syenitic or granitic boulders are of a fine grain and accordingly some of the chief buildings in Victoria are built from them. I am not aware that any rock of a similar description is found *in situ* anywhere in Vancouver Island; it appears to have drifted in icebergs from the north. I am cordially of opinion with Dr. Forbes, that though the last upheaval of the land, which might have taken place at a geologically recent period, failed to connect Vancouver Island with the mainland of North America; it was at all events sufficient to effect to a great extent, the junction of numerous insular ridges, and thus to form a connected whole of what *was*, and might have continued to be, only an archipelago of scattered islets. The upheaving force elevated and connected these and brought to the surface, the great clay, gravel and sand deposits of the northern Drift which had swept over, and been deposited on, the submerged land. These sands, gravels and clays, were now to form the soil of land, prepared for the habitation of man. These constituents of the drift remain, in many parts, thinly covered by a coating of vegetable mould; but much has been washed away. The clay remains most generally and widely spread out, as a retentive sub-soil, having resting upon it a thick coating of vegetable mould. The most valuable soil is found sweeping down the sides of gentle slopes, filling up hollows and swampy bottoms, and, mixed with the rich alluvial deposits of such districts as Saanich, Cowitchan, Delta of Nanaimo, and Comax, forms an inexhaustible source of agricultural wealth. The true glacial or boulder clay is found in various portions of Vancouver Island. Bauermann has described it as seen near Victoria, and I am glad to be able to vouch for the correctness of his description: it is extensively developed not only there but on the opposite coasts of Washington Territory and British Columbia. In the neighborhood of Esquimault and Victoria, the rocks are deeply scored and grooved along the shore, and large boulders are scattered irregularly over the surface of the country, as already described. The other rocks observed as erratics were black cherty conglomerate, similar to that underlying the secondaries,* dark laminated mica schist with well defined garnet-crystals, hornblende rock and largely crystalline greenstone, and rarely and in small masses vesicular obsidian and pitchstone.

The following section is given to show the general character of the drift, at Esquimault Harbor.

Black sandy and peaty ground with broken shells	2 to 6 feet
Yellowish sandy clay with casts of shells (Cardium and Mya) and a few pebbles and boulders	6 to 8 "
Gravel of scratched pebbles resting on rock	2 to 3 "

The rocks are grooved and scratched at the junction; the direction of the glacial markings is between N.–S. and N.N.W–S.S.E. In a well sinking at Esquimault Barracks (for the boundary Commission) the lower gravel was reached at 42 feet, after going through a sandy blue clay without shells or boulders. The section in the cliff between Albert Head and Esquimault is as follows:—

Blue drift clay with boulders; junction with rock not seen,	70 feet
Fine sand and gravel, passing upward into coarse quartzose gravel,	100–120 "

—*Quart. Journ. of Lond. Geol. Soc.*, 1860, p. 202.

Mr. Bauermann is, or at least was, at the period his observations were made, a member of the Geological Survey of the British Islands, and therefore might be supposed to know what he was speaking about. I say so because though I have been able to confirm all his descriptions, yet it is satisfactory in a subject of controversy involving so many important matters to have the support of an additional qualified witness.

As already remarked, I cannot speak so confidently of Washington Territory, Oregon, and the interior of British Columbia to the east of the Cascade Mountains, being less familiar with that section of the Pacific slope. However, through western Oregon wherever I visited the country, down at least to the Umpqua river, and in Washington Territory to the very base of the Cascades, whatever further, I observed glacial remains not less marked than in the neighboring region of Vancouver Island. Some of the erratic blocks are scattered over the prairies of that region, standing on the stoneless grassy plains in marked contrast to their surroundings. These boulders and erratic blocks* have even attracted the notice of the Indians—otherwise so stolid in regard to the natural objects among which they live. One huge angular block, on the Snoqualami Prairie, has a tradition attached to it, to the effect that at one time it was suspended from the sky but was cast adrift to earth on account of the wrath of the Supreme Being, being roused at the licentiousness of a minor god and his myrmidons who for the time being were disporting themselves on it! Not far from the corner of the Peninsula of Saanich off the coast of Vancouver's Island, there are several large boulders—(apparently rounded by the waves and not by ice action?) which aboriginal tradition assert to have been some old witches turned into stone. My canoe-men in passing them used not unfrequently to stop there, and throw water on them, shouting: "Give us a wind, you old jade!" and as occasionally an afternoon breeze does not spring up in that region after the midday summer calms, the superstition obtained with them a semblance of belief, and so got handed on to posterity clothed in all the hoary sanctity of antiquity.

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* "Tertiaries," Mr. Bauermann says, but if he refers to the Northern coal fields of the island, then there can be but little doubt that these beds are as I have given them, here and elsewhere, of secondary age.

Groovings and other unequivocal marks of *general* ice action are not wanting in Washington Territory either. Even with the superficial glance we were enabled to give the subject in hurried journeys over that region, for other purposes, we observed not a few of such deep unmistakable ice planings. And in a note received recently from my friend and former traveling companion, Mr. Edmund T. Coleman, (well known as the author of the folio "Scenes from the Snow Fields of Mont Blanc," and who may therefore be supposed to know ice markings) he states, though with no view to combating the theory in hand, which indeed he knew nothing about:—

"I saw at Seahome (near Bellingham Bay) in the cuttings made for a tramway, the finest instances of fluting and grooving, evidences of glacial action, that I have ever seen on this coast. They were 90 feet in length, running N. and S. according to the theory of Professor Agassiz."

I have not been in Alaska proper, but in 1866 in a visit to the Queen Charlotte Islands lying some thirty or forty miles off the northern coast of British Columbia, close to the southern boundary of the former territory,† marks of the northern Drift quite as marked as in Vancouver's Island were found there.

Indeed in crossing the "spit" at the entrance of Skidegate

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* By "boulders" I mean to designate rounded worn blocks of stone carried along in the *moraine profonde* of the ice sheet; by "erratic blocks," angular fragments of rock. apparently conveyed to their present resting place in ice (field ice or bergs) without having been subject to erosion. The necessity of the division is apparent. The first is very rare on the Pacific coasts and I suspect part of local moraines; the other is universal.

† I have described their geography in the Proceedings of the Royal Geographical Society of London, for 1869. Some remarks on their geology will likewise be found in the same place.

Sound, we had a dangerous reminder of the fact, having lost the keel of our schooner, on one of the great boulders which cover that locality. Masset Spit and other shoal localities are equally dangerous on that account. Here they present themselves disagreeably to the seaman's senses, but on *land*, though less visible on account of the dense vegetation concealing them, yet to one accustomed to search for such things, traveled blocks and ice groovings are sufficiently abundant. Boulder clay is also not wanting to complete the tale of the glacial period in Northwest America.

All throughout this paper I have sedulously avoided touching upon the modern local glaciers which are found scattered all throughout the northern portion of the Cascade and Coast Ranges of Mountains, in some places (as in some of the northern inlets on the coast of British Columbia) approaching to within a short distance of the sea; and in the southern part of the latter range they are found in most of the high mountains, such as Mt. Baker, Diamond Peak, etc. In another place, "On the formation of Fjords, etc."* I have shown that in all likelihood these British Columbian inlets were at one time the site of glaciers, and though the marks of local glaciers are evident here and there where none are now found, yet the appearances described are due to a totally different set of causes from these, or any now in existence on the American continent, unless indeed Greenland be included under that geographical division. These local glaciers in the limits assigned to a paper of this nature do not therefore require to be further touched upon.

Am I therefore in error, when I think that the case I have submitted, makes good the thesis with which I commenced these remarks, viz:—that whatever may be said of California and Alaska (and Messrs. Whitney and Dall are quite capable of holding their own in reference to their assertions about these regions), the Northern drift is certainly not absent from British Columbia, Vancouver's Island, Washington Territory and the Queen Charlotte Islands? With every respect to the observations of the gentlemen named, my more extended opportunities of investigation have, I think, enabled me to answer, with some degree of certainty, this question in the negative. Perhaps I would not have been so particular in discussing this question at length, had not Prof. Whitney's and Mr. Dall's idea been taken up in this country, and in America by geologists of no mean eminence,† and a disposition been shown by others less capable to build thereon theories, where no theories ought to be built.

4 Gladstone Terrace, Hope Park, Edinburgh, June 23d, 1870.

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* Journal of the Royal Geographical Society, vol xxxix.

† Foster in "Mississippi Valley," p. 338, and A. Geikie in "Nature," vol. i, p. 436.

ART. XXXI.—*Extracts from the Address of* GEORGE BENTHAM, Esq., President of the Linnean Society, on the 20th of May, 1870.

IT had been my intention on the present occasion to carry on the sketches of the general progress of biological science which I had attempted in 1862, 1864, 1866, and 1868; but I have, from various causes, been unable to devote so much time as usual to the preparation of my Address, and feel obliged to confine myself to a few points connected with subjects of special interest to myself, which, within the last two or three years, have made considerable advances.

The most striking are, without doubt, the results obtained from the recent explorations of the deep-sea faunas, and from the investigation of the tertiary deposits of the arctic regions, which, although affecting two very different branches of natural science, I here couple together, as tending, both of them, to elucidate in a remarkable degree one of the most important among the disputed questions in biological history, the continuity of life through successive geological periods.

An excellent general sketch of the first discovery and progressive investigation of animal life at the bottom of the sea at great depths, up to the close of the season of 1868, is given by Dr. Carpenter in the Proceedings of the Royal Society, vol. xvii, No. 107, for Dec. 17, 1868. The results of the still more important expedition of the past year have as yet been only generally stated by Mr. Gwyn Jeffreys, in the numbers of 'Nature' for Dec. 2 and 9, 1869, and by Dr. Carpenter, in a lecture to the Royal Institution, published in the numbers of 'Scientific Opinion' for March 23 and 30 and April 6 and 13 of the present year; and further details, as to the Madreporaria, are given by Mr. Duncan in the Proceedings of the Royal Society, vol. xviii, No. 118, for March 24 of the present year; whilst, in North America, the chief conclusions to be drawn from these researches into the deep-sea fauna are clearly and concisely enumerated by Prof. Verrill, in the American Journal of Science for January last; and some of the more detailed reports of the American explorations, by Louis and Alexander Agassiz and others, have been published in the Bulletin of the Museum of Comparative Zoology at Harvard College, Nos. 6, 7, and 9 to 13. For the knowledge of the data furnished by the tertiary deposits of the arctic regions we are indebted almost exclusively to the acute observations and able elucidations of Prof. O. Heer, in his 'Flora Fossilis Arctica,' in his paper on the fossil plants collected by Mr. Whymper in North Greenland, published in the last part of the Philosophical Transactions for

1869, and in the as yet only short general sketch of the results of the Swedish Spitzbergen Expeditions, contained in the Genevese 'Bibliothèque Universelle, Archives Scientifiques,' for Dec. 1869.

It would be useless for me here to retrace, after Dr. Carpenter and Prof. Verrill, the outlines of the revolution which these marine discoveries have caused in the previously conceived theories, both as to the geographical distribution of marine animals, and the relative influences upon it of temperature and depth, and as to the actual temperature of the deep seas, or to enter into any details of the enormous additions thus made to our knowledge of the diversities of organic life; and it would be still further from my province to consider the geological conclusions to be drawn from them. My object is more especially to point out how these respective dips into the early history of marine animals and of terrestrial forests have afforded the strongest evidence we have yet obtained, that apparently unlimited permanency and total change can go on side by side, without requiring for the latter any general catastrophe that should preclude the former.

There was a time, as we learn, when our chalk-cliffs, now high and dry, were being formed at the bottom of the sea by the gradual growth and decay of Globigerinæ and the animals that fed on them—amongst others, for instance, *Rhizocrinus* and *Terebratulina caput-serpentis;* and when, at a later period, the upheaval of the ground into an element where these animals could no longer live arrested their progress in that direction, they had already spread over an area sufficiently extensive for some part of their race to maintain itself undisturbed; and so, on from that time to the present day, by gradual dispersion or migration, in one direction or another, the same *Rhizocrinus* and *Terebratulina* have always been in possession of some genial locality, where they have continued from generation to generation, and still continue, with Globigerinæ and other animals, forming chalk at the bottom of the sea, unchanged in structural character, and rigidly conservative in habits and mode of life through the vast geological period they have witnessed. So also there was a time when the hill-sides of Greenland and Spitzbergen, now enveloped in never-melting ice and snows, were, under a genial climate, clothed with forests, in which flourished *Taxodium distichum* (with *Sequoiæ*, *Magnoliæ*, and when at a later period these forests were destroyed by the general refrigeration, the Taxodium already occupied an area extensive enough to include some districts in which it could still live and propagate; and whatever vicissitudes it may have met with in some parts, or even in the whole, of its original area, it has, by gradual extension and migration, always found some spot where it has gone on and thriven, and continued its race from generation to generation down to the present day, unchanged in character and unmodified in its requirements. In both cases, the permanent animals of the deep-sea bottom and the permanent trees of the terrestrial forests have witnessed a more or less partial or complete change in the races amongst which they were commingled. Some of these primitive associates, not endowed with the same means of dispersion, and confined to their original areas, were extinguished by the geological or climatological changes, and replaced by other races amongst which the permanent ones had penetrated, or by new immigrants from other areas; others, again, had spread like the permanent ones, but were less fitted for the new conditions in which they had been placed, and in the course of successive generations had been gradually modified by the Darwinian process of natural selection, the survival of the fittest only among their descendants. If, in after times, the upheaved sea-bottom becomes again submerged, the frozen land becomes again suited for vegetation, they are again respectively covered with marine animals or vegetable life, derived from more or less adjacent regions, and more or less different from that which they originally supported, in proportion to the lapse of time and extent of physical changes which had intervened. Thus it is that we can perfectly agree with Mr. Duncan, that "this persistence (of type and species through ages, whilst their surroundings were changed over and over again) does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods:" but we can, at the same time, feel that Dr. Carpenter is in one sense justified in the proposition, that we may be said to be still living in the Cretaceous period. The chalk formation has been going on over some part of the North Atlantic sea-bed, from its first commencement to the present day, in unbroken continuity and unchanged in character.

If once we admit as demonstrated the coëxistence of indefinite permanency and of gradual or rapid change in different races in the same area, and under the same physical conditions, according to their constitutional idiosyncracies, and also that one and the same race may be permanent or more or less changing, according to local, climatological, or other physical conditions in which it may be placed, we have removed one of the great obstacles to the investigation of the history of races, the apparent want of uniformity in the laws which regulate the succession of forms. We may not only trace, with more confidence, such modifications of race through successive geological periods as Prof. Huxley has recently exhibited to us in respect of the Horse, but we can understand more readily the absolute identity of certain species of plants inhabiting widely dissevered areas, of which the great majority of species are more or less different. One of the arguments brought forward against the community of origin of representative species in distant regions, such as temperate Europe and the Australian Alps, the Arctic Circle and Antarctic America, the Eastern United States and Japan respectively—an argument which has long appeared to me to have considerable weight—was this:—if disseverance and subsequent isolation result necessarily in a gradual modification by natural selection, how is it that when all are subjected to the same influences, the descendants of some races have become almost generically distinct in the two regions, whilst others are universally acknowledged as congeners, but specifically distinct, and others again are only slight varieties or have remained absolutely identical? To this we can now reply, with some confidence, that there is no more absolute uniformity in the results of natural selection than in any other of the phenomena of life. External influences act differently upon different constitutions. Were we to remove the whole flora and fauna of a country to a distant region, or, what comes to the same thing, change the external conditions of that flora and fauna, as to climate, physical influences, natural enemies, or other causes of destruction, means of protection, &c., we should now be taught to expect that some of the individual races would at once perish; others, more or less affected, might continue through several generations, but with decreasing vigor, and, in the course of years or ages, gradually die out, to be replaced by more vigorous neighbors or invaders. Others, again, might see amongst their numerous and ever varying offspring some few slightly modified, so as to be better suited for the new order of things; and experience has repeatedly shown that the change once begun may go on increasing through successive generations and a permanent representative species may be formed. And some few races might find themselves quite as happy and vigorous under their new circumstances as under the old, and might go on as before, unchanged and unchanging.

Taking into consideration the new lights that have been thrown upon these subjects by the above investigations and by the numerous observations called forth by the development of the great Darwinian theories, amongst which I may include a few points adverted to in a paper on Cassia which I laid before you last year, but which a press of matter has prevented our yet sending to press, it appears to me that, in plants at least, we may almost watch, as it were, the process of specific change actually going on; or at least we may observe different races now living in different stages of progress, from the slight local variation to the distinct species and genus. As the first step we may take, for instance, those races which are regarded by the majority of botanists as variable species, such as *Rubus fruticosus*, *Rosa canina*, *Zornia diphylla*, *Cassia mimosoides*, &c. We shall find in each some one form, which we call typical, generally prevalent over the greater part of the area of the race, whilst others, more or less aberrant, are more or less restricted to particular localities, the same varieties not occurring in disconnected stations with precisely the same combinations of character and in the same proportions; local and representative varieties and subspecies are being formed, but have not yet obtained sufficient advantages to prevent their being kept in check by their intercommunication (and, probably, cross-breeding) with their more robust type. The British Batologist or Rhodologist transported to the south of France or to Hungary will still find one, or perhaps two or three forms of Bramble and Dog-rose with which he is familiar; but if he wishes to discriminate the thirty or forty varieties or subspecies upon which he had spent so much labor and acuteness at home, he must recommence with a series of forms and combinations of characters quite new to him. The species is still the same; the varieties are changed. As examples of what we may call a second stage in the formation of species, we may adduce such plants as *Pelargonium australe* or *grossularioides* and *Nicotiana suaveolens* or *angustifolia*, to which I alluded in the above-mentioned paper on *Cassia*. Here we have one race, of no higher than specific grade in the ordinary acceptance of the term, inhabiting two countries which have long been widely dissevered (in the one case South Africa and Australia, in the other Chili and Australia), which, if originally introduced by accident from one country to the other, have been so at a time so remote as thoroughly to have acquired an indigenous character in both; in both they are widely spread and highly diversified: but among all their varieties one form only is identical in the two countries (*Pelargonium australe*, var. *erodioides*, and *P. grossularioides*, var. *anceps; Nicotiana suaveolens*, var. *angustifolia*, and *N. angustifolia*, var. *acuminata*), and that so comparatively a rare one that it may be regarded as being in the course of extinction; whilst all other varieties, some of them very numerous in individuals over extended areas, and all connected by nice gradations, diverge nevertheless in the two countries in different directions and with different combinations of characters, no two of them growing in the two countries being at all connected but through the medium of that one which is still common to both. When that shall have expired, the distinct species may be considered established. A still further advance in specific change is exemplified in *Cassia* itself, in which I have shown that no less than eight or nine different modifications of type, sectional and subsectional, are common to South America, tropical Africa, and Australia, but without any specific or, at least, subspecific identity, except perhaps in a few cases where a more modern interchange may be presumed. The original common specific types are extinct, the species have risen into sections. Common types of a still higher order have disappeared in the case of Proteaceæ, an order so perfectly natural and so clearly defined that we cannot refrain from speculating on the community of origin of the African and of the Australian races, both exceedingly numerous and reducible to definite groups—large and small well-marked genera in both countries, and yet not a single genus common to the two; not only the species, but the genera themselves have become geographical. As in the case of the varieties of *Pelargonium* and *Nicotiana*, so in that of the species of *Cassia* and of the genera of Proteaceæ, it is not to be denied that precisely similar modifications of character are observed in the two countries; but these modifications are differently combined, the changes in the organs are differently correlated. In Asiatico-African *Chamœcristæ* a tendency to a particular change in the venation of the leaflet is accompanied by a certain change in the petiolar gland; in America the same change in the gland is correlated with a different alteration in the venation. In Australian Proteaceæ the glands of the torus are constantly deficient with a certain inflorescence (cones with imbricate scales) which is always accompanied by them in Africa.

In selecting the above instance for illustration of what we may, without much strain upon the imagination, suppose to be cases of progressive change in races, it is not that they are isolated cases or exceptionally appropriate; for innumerable similar ones might be adduced. In the course of the detailed examination I have had successively to make of the floras of Europe, N. W. America, Tropical America, Tropical Africa, China, and Australia, I have everywhere observed that community of general type, in regions now dissevered, is, when once varied, accompanied by more or less of divergence in more special characters in different directions in the different countries.

With regard to the succession of races which have undergone a complete specific change through successive geological periods, we have not in plants, as far as I am aware, any such cases of "true linear types or forms which are intermediate between others because they stand in a direct genetic relation to them," as Professor Huxley appears to have made out in favor of the pedigree of the Horse in his last Anniversary Address to the Geological Society. And I may, in regard to plants, repeat with still greater emphasis his dictum, that "it is no easy matter to find clear and unmistakable evidence of filiation among fossil animals; for in order that such evidence should be quite satisfactory, it is necessary that we should be acquainted with all the most important features of the organization of the animals which are supppsed to be thus related, and not merely with the fragments upon which the genera and species of the paleontologist are so often based." The difficulty is much greater in the case of fossil plants; for instead of bones, teeth, or shells, portions of internal or external skeletons, the parts preserved to us from the tertiary period are generally those least indicative of structural organization. Mr. Carruthers has recently (Geological Magazine, April and July 1869, and Journal of the Geological Society, August, 1869) adduced satisfactory evidence of the close affinity of *Sigillaria* and the allied genera of the coal-period with the living Lycopodiaceæ, formerly suggested by Dr. Hooker; but, as he informs me, no connecting links, no specimens, indeed, of the whole Order, have as yet been found in any of the intermediate cretaceous or tertiary deposits. Among the latter, the presence of numerous vegetable types, to which we may plausibly refer as to the ancestors of living races, is established upon unimpeachable data; but I have been unable to find that a single case of authentic pedigree, as successively altered from the cretaceous through the abundant deposits of the eocene and miocene period to the living races, has been as yet as satisfactorily made out as that of the absolute identity of *Taxodium* and others above mentioned, although I feel very little doubt that such a one will yet be traced when our paleontologists shall have ceased to confound and reason alike upon the best proved facts and the wildest guesses. Our late distinguished foreign Member, Professor Unger, whose loss we have so recently to deplore, had indeed, shortly before his death, published, under the name of 'Geologie der europäischen Waldbäume, part 1, Laubhölzer,' no less than twelve tabular pedigrees of European forest races; but it seems to me that in this, as in another of the same eminent paleontologist's papers to which I shall presently have to refer, his speculations have been deduced much more freely from conjectures than from facts. There is no doubt that the presence of closely allied representatives of our Beeches, Birches, Alders, Oaks, Limes, &c., in the tertiary deposits of Central and Southern Europe is fully proved by inflorescences and fruits as well as leaves; but how can we establish the successive changes of character in a race when we have only the inflorescence of one period, the fruit of another, and the leaf of a third? I do not find a single case in which all three have been found in more than one stage; and by far the great majority of these fossil species are established on the authority of detached leaves or fragments of leaves alone.

Now let us consider for a moment what place a leaf really holds in systematic botany. Would any experienced systematic botanist, however acute, on the sole examination of an unknown leaf, presume to determine, not only its natural order and genus, but its precise characters as an unpublished species? It is true that monographists have sometimes published new species founded on specimens without flower or fruit, which from collateral circumstances of habitat, collector's notes, general resemblance, &c., they had good reason to believe really belonged to the genus they were occupied with; but then they had the advantage of ascertaining the general *facies* derived from insertion, relative position, presence or absence of stipular appendages, &c., besides the data supplied by the branch itself. And with all these aids, even the elder De Candolle, than whom no botanist was more sagacious in judging of a genus from general aspect, proved to have been in several instances far wrong in the genus, and even Order, to which he had attributed species described from leaf-specimens only. Paleontologists, on the other hand, have, in the majority of these tertiary deposits, had nothing to work upon but detached leaves or fragments of leaves, exhibiting only outward form, venation, and, to a certain degree, epidermal structure, all of which characters may be referred to that class which Professor Flower, in his introductory lecture at the Royal College of Surgeons in February last, has so aptly designated as *adaptive*, in contradistinction to essential and fundamental characters. They may, when taken in conjunction with relative individual abundance, assist in forming a general idea of the aspect of vegetation, and thus give some clue to certain physical conditions of the country; but they alone can afford no indication of genetic affinity, or consequently of origin or successive geographical distribution.

Lesquereux, in speaking of Cretaceous "species, or rather forms of leaves," observes, in a note to his paper on Fossil Plants from Nebraska (this Journal, vol. xlvi, July, 1868, p. 103), that "it is well understood that when the word *species* is used in an examination of fossil plants, it is not taken in its precise sense; for indeed no *species* can be established from leaves or mere fragments of leaves. But as paleontologists have to recognize these forms described and figured, to compare them and use them for reference, it is necessary to affix to them specific names, and therefore to consider them as species." But the investigators of the tertiary floras of Central and Southern Europe have acquired the habit, not only of neglecting this distinction and naming and treating these forms of leaves as species equivalent to those established on living plants, but of founding upon them theories which must fall to the ground if such specific determination proves inaccurate. Nothing can be more satisfactory than such determinations as that of *Podogonium*, for instance, which Professor Heer has succeeded in proving, by numerous specimens of leaves, fruits, and even flowers, some of them still attached to the branches, which I had myself the pleasure of inspecting last summer under the friendly guidance of the distinguished Professor himself. This genus of Cæsalpinieæ, from its evident affinity with *Peltogyne*, *Tamarindus*, and others now scattered over the warmer regions of America and Africa, and more sparingly in Asia, tells a tale of much significance as to the physico-geographical relations of the Swiss tertiary vegetation, confirmed as it is by some other, equally or almost equally convincing examples. But the case appears to me to be far different with the theory so vividly expounded by Professor Unger in 1861 in his Address entitled "Neu Holland in Europa." This theory, now generally admitted, seems to me to be established on some such reasoning as this:—There are in the tertiary deposits in Europe, and especially in the earlier ones, a number of leaves that look like those of Proteaceæ are a distinguished feature in Australian vegetation; *ergo*, European vegetation had in those times much of an Australian type derived from a direct land communication with that distant region.

This conviction, that Proteaceæ belonging to Australian genera were numerous in Europe in Eocene times, is indeed regarded by paleontologists as one of the best-proved of their facts. They enumerate nearly one hundred tertiary species, and most of them with such absolute confidence that it would seem the height of presumption for so inexperienced a paleontologist as myself to express any doubt on the subject. And yet, although the remains of the tertiary vegetation are far too scanty to assert that Protaeceæ did not form part of it, I have no hesitation in stating that I do not believe that a single specimen has been found that a modern systematic botanist would admit to be Proteaceous unless it had been received from a country where Proteaceæ were otherwise known to exist. And, on other grounds, I should be most unwilling to believe that any of the great Australian branches of the Order ever reached Europe. As this is a statement requiring much more than mere assertion on my part, I shall beg to enter into some detail, commencing with a short summary of my grounds of disbelief in European tertiary Proteaceæ, and then examining into the supposed evidences of their existence.

* * * * * * *

From the above considerations, I cannot resist the opinion that all presumptive evidence is against European Proteaceæ, and that all direct evidence adduced in their favor has broken down upon cross-examination. And however much these Eocene leaves may assume a general character, which may be more frequent in Australia (in Proteaceæ and other Orders) than elsewhere, all that this would prove would be, not any genetic affinity with Australian races, but some similarity of causes producing similarity of adaptive characters.

Another series of conclusions drawn by paleontologists from their recent discoveries, which appears to me to have been carried too far, relates to the region where a given species originated. The theory that every race (whether species or group of species derived from a single one) originated in a single individual, and consequently in one spot, from which it has gradually spread, is a necessary consequence of the adoption of Darwinian views; and when Mr. R. Brown ("On the Geographical Distribution of Conifers," Trans. Bot. Soc. Edin., x, p. 195) sneers at my having qualified it as a perfect delusion, he must have totally misunderstood, or rather misread the passage he refers to in my last year's Address. The expression is there specially applied to the idea of general centers of creation whence the whole flora of a region has gradually spread, in contradistinction to the presumed origin of individual races in a single spot, which is there as distinctly admitted. The determination of where that spot is for any individual race, is a far more complicated question than either geographical botanists or paleontologists seem to suppose. "Every vegetable species," as well observed by Professor Heer, "has its separate history," and requires a very careful comparison of all the conclusions deducible as well from present distribution as from ancient remains. The very important fact that *Taxodium distichum*, *Sequoiæ*, *Magnoliæ*, *Salisburia*, &c., existed in Spitsbergen in Miocene times, so satisfactorily proved by Heer, shows that the vegetation of that country then comprised species and genera now characteristic of North America; but it appears to me that the only conclusion to be drawn (independently of climate and geology) is, that the area of these species and genera had extended continuously from the one country to the other, either at some one time, or during successive periods. The proposition that "Spitsbergen appears to have been the focus of distribution of *Taxodium distichum*," because an accidental preservation of its remains shows that it existed there in the lower Miocene period, would require at least to be in some measure confirmed by a knowledge of the flora of the same and preceding periods over the remainder of its present area, the greater part of which flora, however, is totally annihilated and forever concealed from us. The fact that *Pinus Abies* existed in Spitsbergen in Miocene times, and that no trace of it has been found in the abundant Tertiary remains of Central Europe is very instructive. It might show that that tree was of more recent introduction into the latter than the former country; but it cannot prove that it was not still earlier in some other region, whence it may have spread successively into both territories, still less that its course of dissemination was directly from Spitsbergen over Northern and Central Europe. Moreover the determination of *Pinus Abies* is not so convincing as that of the *Taxodium*, resting as it does, if I correctly understand Prof. Heer's expression, on detached seeds and leaves, with a few scales of one cone, and may require further confirmation.

In the above observations it is very far from my wish to detract from the great value of Professor Heer's researches. Interested as I have been in the investigation of the history of races of plants, I have deeply felt my general ignorance of paleontology, and consequent want of means of checking any conclusions I may have drawn from present vegetation by any knowledge of that which preceded it, and the impossibility at my time of life of entering into any detailed course of study of fossils. Like many other recent botanists, I am obliged to avail myself of the general results of the labors of paleontologists; and if I have here ventured on a few criticisms, it is only as a justification of the hope that they may in some measure distinguish proved facts from vague guesses, in order that we may know how far reliance is to be placed on their conclusions. * * * * * *

ART. XXXII.—*Account of the fall of a Meteoric Stone in Stewart County, Georgia;* by Professor JOSEPH E. WILLET.

IN October, 1869, I learned that a metoric explosionh ad occurred in Stewart county, Georgia. I immediately requested Hon. John T. Clarke, a resident of the county adjoining Stewart, to enquire whether any stone or stones had fallen, and to endeavor to procure them for Mercer University. Judge Clarke, after considerable labor, was entirely successful in his search; and, through him, Mr. Barlow, in whose yard the meteorite descended, generously presented it to our Museum. To Judge Clarke and to Mr. Latimer, I am indebted for the following history of the phenomena attending the descent of the meteorite.

Mr. J. B. Latimer of Bladen's creek, Stewart county, has kindly furnished the following particulars of the *flight* of the body through the air, and of the several *explosions*, which occurred nearly vertically above him.

"The morning of the 6th October last (1869) was quite clear, scarcely any cloud being visible, quite calm; about 10 A. M. the atmosphere grew somewhat hazy, no clouds; at about 15 or 20 minutes before 12 M. a roaring, rushing sound was heard in a northwesterly direction, about 80 degrees above the horizon. In a moment or two, it was almost directly over head, at which point a loud explosion occurred, followed in rapid succession by six other reports, but less in volume than the first—making seven in all. The explosions appeared about as loud as a 12-pound cannon, at a distance of 10 or 12 miles. These explosions did not occur all at the same point in the heavens, but seemed to emanate from some body moving rapidly to the southeast. After the explosions, a peculiar whirring sound was heard, apparently produced by some large irregular body, moving very rapidly. This also went in a southeasterly direction. This sound was heard several seconds; many have compared it, and aptly too, to an imperfect steam-whistle. I have no precise idea of the time consumed in all this demonstration; some persons say several minutes—but I think 10 or 15 seconds would about cover the time.

"As the larger body was going out of our hearing, (some moments after the explosions) a smaller one passed to the southwest, with just such a noise as is always produced by a flying fragment of a shell after its explosion, or of any angular body cast violently through the air. This piece descended to the earth, distinctly traced in its passage by many persons, and struck in the yard of Capt. E. Barlow—which point of contact is, on an air-line, about 2½ miles from a perpendicular beneath where the explosions occurred. This is the only one known to have fallen in this section.

"The explosions, together with the rushing sound afterward, were heard over a region about 30 miles N.E. and S.W. and 50 or 60 miles N.W. and S.E. No shock was felt—at least no tremor of the earth.

"Two men say, that they were looking in the exact direction of the explosions at the time they occurred, and saw a quantity of vapor, much like the volume of steam escaping from the pipe of an engine, at each successive stroke; which vapor or mist was violently agitated, and increased in bulk, with each successive report, but disappeared soon after the cessation of the reports. This corroborates the testimony of some of my own laborers, who say, that immediately after the explosions something like a thin cloud cast its shadow over the field they were in."

Hon. John T. Clarke, of Cuthbert, Ga., who has interested himself in collecting the history of the meteorite, and through whose influence it has come into the possession of Mercer University, writes me the following particulars of its *fall.*

"It fell about 11½ A. M. on the 6th of October last, (1869), in Stewart county, Ga., on the premises of Elbridge Barlow, Esq., about 12 miles south of west from Lumpkin. Capt. Barlow picked it up a few moments after it fell. His account of it is this. While standing in the open yard, the sky being bright and clear, he heard first a succession of about three explosions, resembling sudden bursts of thunder, or discharges of artillery, followed by a deep roaring for several seconds, and then by a rushing or whizzing sound of something rushing with great speed through the air near by. The sound ceased suddenly. The noise, from first to last, was some half a minute. Two negroes were washing near the well, in the same yard, about sixty yards from where Barlow stood. They heard the noise, and supposed it to be the falling in of the plank well-curbing, banging from side to side in its descent, and so spoke of it to one another before it fell. While they were speaking thus, it struck the ground about twenty steps from them, in full sight, knocking up the dirt. They called Capt. B. and showed him the spot. It was upon very hard trodden ground in the clean open yard. The earth was freshly loosened up very fine in a circle of about one and a half feet in diameter; and, upon scraping the loose dirt away with the hands, the stone was found about ten inches below the surface. From the direction in which the ground was crushed in, it must have come from the northwest, and at an angle of about 30 degrees with the horizon. The stone when picked up was covered all over with the black shell which it bears now, except a triangular spot on one corner, about one inch each way, where the corner appeared freshly knocked off, and about four other spots near a quarter of an inch in diameter, where the shell was slightly knocked off. The other bruises, which you will find upon it, have been made since by persons who have handled it. To enable you to distinguish the original breaks upon it, I have marked each of them with a red cross. The stone still has a strong odor, which I will not undertake to describe. Capt. B. says it smelled stronger when he first picked it up. He does not remember that it had any noticeable heat. It was not cold, as a stone found so deep in the ground should be.

"The stone weighs now 12¼ ounces, about ½ ounce has been pecked off from it. Its color within is strikingly like very light granite; and, with the exceptions above noted, it is entirely covered with a smooth, almost black shell, a trifle thicker than common letter paper, so that externally it looks very much like a lump of iron ore. It is an irregular, seven-sided figure, its longest side being about 2¾ inches long. If put into a spherical form, it would make a ball about 1¾ inches in diameter. So far as I have been able to ascertain, no other parts have been found.

"The noise attending this phenomenon is variously described by different persons, and from different places. Two intelligent ladies residing four miles south of Lumpkin, nearly east of where the stone fell, and about ten or twelve miles off, describe it thus. While sitting in the house they heard, as it were, the sound of a great fire suddenly bursting forth from some confinement into the open air. They rushed out of doors, and heard the roaring sound continue for several seconds. They located the source of the noise in the direction of Barlow's.

"In Cuthbert, about 18 miles from Barlow's, nearly southeast, a gentleman, engaged in a workshop, heard a lumbering noise, which he took to be several heavy pieces of machinery in an adjoining room, falling down one after another. On going in, he found no one, and that he had mistaken the cause of the noise. Many persons here heard sounds like repeated thunder followed by roaring. Some say that they first heard several rapid, cracking explosions, like that of volleys of small arms, followed immediately by the louder burst of artillery. Most persons here thought the noise came from the southeast, passed over the place in a northwesterly direction, and died away in the distant northwest.

"The foregoing statements have been selected from many in circulation, showing how differently the senses were affected at different points. The facts are purposely presented in their nakedness. If you can find them available in aid of a scientific investigation of the origin of this phenomenon, I shall have accomplished more than I expect."

The above accounts agree as to the main facts. They were furnished by Mr. Latimer and Judge Clarke, without being compared by them. It is possible that a comparison of notes by them might have thrown some light on the point of greatest discrepancy, viz: the direction of flight. It is probable that the meteorite came from some point in the *north* quarter; the statement of Mr. Latimer over whom it exploded, and that of Mr. Barlow as to the direction in which the earth was penetrated, concur in this regard. Persons in Cuthbert, who represent it as coming from the south, may have been misled by an echo, mistaking this for the original sound.

Prof. J. Lawrence Smith, who is giving special attention to the subject of meteorites, has requested the privilege of analyzing the stone above described.

Mercer University, Penfield, Georgia.

ART. XXXIII.—*Description and Analysis of a Meteoric Stone that fell in Stewart county, Ga. (Stewart county Meteorite), on the 6th of October*, 1869; by J. LAWRENCE SMITH.

IN October, 1869, I learned through the public press that certain meteoric phenomena had occurred in Stewart county, Georgia, and that one or more stones had fallen. Enquiries were immediately instituted by me, and, through Prof. Willet, I obtained for examination the only stone found, one that was seen to strike the ground, and from him received an account of the phenomena observed at the time by Messrs. Latimer, Clarke and others. [See preceding Article.]

The stone, as it reached me, was nearly intact, and weighed 12¼ ounces; it must originally have weighed 12⅓ ounces. It is of an irregular conical shape, having a flattened base, and is covered with a dull heavy black coating. The specific gravity is 3·65. The fractured surface has a grayish aspect, and when examined closely, especially by the aid of a glass, exhibits numerous greenish globules with a whitish granular material between; through the mass are dark particles consisting principally of nickeliferous iron, with some pyrites, and a few specks of chrome iron. The nodules are sometimes three or more millimeters in diameter, and of an obscure fibrous crystalline structure, the crystals radiating usually from one side of the nodule; they have a dirty bottle-green color, a greasy aspect when broken, and are more or less opake.

Some of these little nodules were separated in a tolerable state of purity, amounting to 121 milligrams; on analysis they afforded:

		Oxygen.	Ratio.
Silica	48·62	25·90	2
Alumina	8·05	3·79	
Protoxyd of iron	11·21	2·51	1
Magnesia	30·18	11·80	
	98·06		

The hardness of the mineral is about 6, and it is quite tough. The formula would be $\dot{R}\ddot{S}i$, with a part of the silica replaced by alumina, a not unfrequent case in minerals such as hornblende, hypersthene, &c. As it is impossible to derive any light from its crystalline structure, the above analysis warrants me in concluding that it is either *bronzite*, or *hornblende*, but I am more inclined to the former supposition as it appears to take the place of the enstatite in many meteorites.

Nickeliferous iron constitutes about 7 per cent of the mass, and a portion separated in as pure a state as possible, afforded on analysis—

Iron	86·92
Nickel	12·01
Cobalt	0·75
	99·68

These are the proportions after allowing iron for a small amount of sulphur, present in a minute quantity in the nickeliferous iron, which could not be separated mechanically. I did not test for copper or phosphorus; the quantity of iron separated from the stone did not warrant my making special analyses for substances, the quantity of which present could only be exceedingly minute.

The stony matter freed from the iron was treated with nitro-muriatic acid and water, and heated for some time over a water bath, renewing the water and acid once or twice; the solution was filtered, and the residue washed; the residue was then treated with a warm solution of caustic potash, filtered and again washed. The filtrate was neutralized by hydrochloric acid, and added to the first filtrate, and the whole evaporated to dryness over a water bath, warmed gently over the lamp, and treated with water and a little hydrochloric acid, thrown on a filter, the silica collected and estimated; the last filtrate was treated with a solution of hydrochlorate of baryta to ascertain the quantity of sulphuric acid present, (due to the pyrites in the original mass); it was found to indicate 6·10 per cent of magnetic iron pyrites. The solution freed from the excess of baryta was now analyzed in the ordinary way.

The insoluble portion of the meteorite was fused with carbonate of soda and a small fragment of caustic potash, and its ingredients ascertained.

A separate portion of the stony part of the meteorite was examined for alkalies.

The various analyses referred to above gave—omitting the nickeliferous iron:

The part soluble in acid	58·05
" " insoluble "	41·95

	Soluble part.	Insoluble part.
Silica	41·08	56·03
Alumina	0·32	5·89
Protoxyd of iron	18·45	15·21
Magnesia	41·06	21·00
Lime	----	0·10
Soda, with a little $\dot{K}$ and $\dot{L}i$	----	2·97
	100·83	101·20

The soluble part consists principally of olivine. The insoluble is doubtless the bronzite already refer to, with a little albite or oligoclase.

Chrome iron was detected by fusing some of the stony part of the meteorite with carbonate of soda and a little niter, and separating in the usual way. The quantity was quite minute.

The composition of the stone as made out would be

Nickeliferous iron	7·00
Magnetic pyrites	6·10
Bronzite, or hornblende	86·90
Olivine	
Albite, or oligoclase	
Chrome iron	
	100·00

ART. XXXIV.—*Some practical remarks on the use of Flame Heat in the Chemical Laboratory, especially that from burning gas without the aid of a blast;* by J. LAWRENCE SMITH, Louisville, Ky.

THERE is probably no more important era in the operations of the chemical laboratory than that of the introduction of the lamp as a source of heat for a large number of chemical operations, and that without the aid of a blast. Berzelius was doubtless the first to accomplish much in this direction, which he did by the agency of the lamp that so commonly bears his name, and which, more or less modified, is still in use where the ordinary illuminating gas is not to be had.

Although illuminating gas has been in use for about seventy years, it is only within a comparatively recent date that it has been pressed into service, and used as a heating agent in the laboratory. The reason of this arose from the fact that when burnt in the ordinary manner it deposited soot on the vessels heated by it. This difficulty has been overcome by burning the gas from small orifices made in a tube bent in the form of a circle, the holes being from 1 to 2 centimeters apart, and, sometimes, combining two or more rings in concentric circles. This method, however, has not been generally adopted.

We must date the successful introduction of gas for heating purposes to the use of a mixture of gas and air passed through wire gauze and ignited above the gauze, giving a flame without light and with great heat; the invention of this method is claimed by several, and doubtless was discovered by different individuals at about the same time, without a previous knowledge of each other's results; this method is still more or less employed for certain purposes.

The next step in this direction, and doubtless the most important up to the present time, is to burn the mixture of gas and air without the agency of wire gauze; it was first made known to the public in the burner commonly called the Bunsen burner, doubtless from its being either invented or brought into extensive practical use by the distinguished chemist of Heidelberg. Its form is too well known to require more than a mere mention here, and it is now made of all sizes from those capable of burning 4 cubic feet of gas and under, to those which can burn 15 or 20 cubic feet from a single burner, or from a combination of several smaller ones. To this burner, some material additions have been made by different individuals. J. J. Griffin, (the chemical instrument dealer in London), was, I believe, the first to introduce the use of the rosette and the register for the supply of air. The most remarkable results accomplished by this method of burning gas and air are those obtained by G. Gore of Birmingham, (all of whose results I have verified), where gold, copper, cast iron, &c., were fused in crucibles without the agency of any artificial blast. Mr. Gore evidently realized fully the true principle of burning this mixture, so as to obtain a maximum effect; the burner, however, with its furnace arrangements, is unavoidably of a form and on a scale limiting its application.

The usual form of the Bunsen burner, with the rosette and register (when required), bids fair to hold its own against any other form for general purposes, and whatever modifications may be made on it should be of such a character as not to entrench on its simplicity. One or two of these modifications are now in daily use in my laboratory, for which there is no claim to any special originality, nor are they intended to supplant the ordinary form.

As simple an instrument as the Bunsen burner appears to be, its principles and effects are well worthy of being carefully studied.

As the gas passes from the small orifices* in the lower part of the burner, and mixes with the air drawn in at the lower opening, and passes out at the open end of the tube, it usually contains not quite enough oxygen for its complete combustion, and requires free access of air to the outer portion of the flame to complete the combustion; yet even with this, the flame is hollow in its lower portion, having a cool center, its most intense heat being at about three or four inches above the end of the tube in the smaller Bunsen-burners, and eight or ten inches in the largest size. If a proper access of air is not allowed to the flame, as sometimes happens in some of the furnace connections occasionally used with Bunsen's burner, acetylene is formed from the imperfect combustion, which is recognized by its disagreeable odor, or by collecting some of the gas formed during the combustion; the presence of acetylene may be rendered evident by a small amount of a solution of ammoniacal cuprous chlorid.

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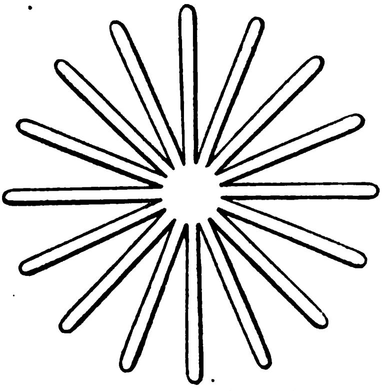
* The outlet for gas may be in the form of crossed slits or two small holes of (1-32 inch diameter each) for the small size burner, the length of tube being about 4 to 4½ inches long; the next larger has four openings (about 1-25 inch diameter each) and the tube about 5 inches long.

The best heating effects of the gas used in the ordinary round Bunsen burner, when employed in the heating of crucibles and other vessels, are not obtained; yet in the great majority of cases the small loss of gas is not worth considering, especially as to obtain better results in most cases, would only complicate this beautifully simple instrument.

To get the best effects of heat, we must imitate the principle applied in the Argand burner, namely to flatten down the exit of the mixed gases. It was by following out this principle that Mr. Gore was enabled to make a burner having a number of radial flat orifices as represented in the figure (1), the air from without having free access to the flame along the entire length of the slit openings, the number of slits used are more numerous than those represented in the figure. With the flame from this burner introduced into a certain form of refractory cylinders, cast iron can be melted in a crucible, without the aid of a blast, as has already been stated; the little chimney to the furnace being two inches in diameter, and four feet long. This burner and its furnace is of but limited application, and the amount of gas consumed considerable.

1.



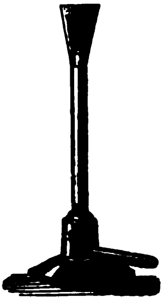
The openings at the exit of Gore's burner.

The principle, however, of the above burner is introduced in constructing a more simple form, and the flattened orifice is now used in the construction of what I conceive to be the best form of furnace for heating glass tubes for organic analyses and other purposes; such furnaces are made by Weisnig of Paris, and Desaga of Heidelberg.

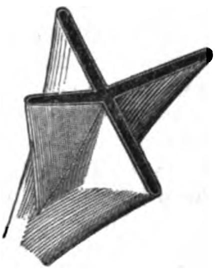
The use of the flattened burner is not fully appreciated; its advantages are, that there is no cold point in the flame, and the burner can be brought much nearer to the object to be heated, within 20 to 25 millimeters for the small sized burners. In this burner as usually made, the opening is too broad, experience having convinced me that a slit 2 millimeters across and about 40 millimeters in length is the most effective one for a small size burner, consuming about 5½ cubic feet per hour; this burner is represented in fig. 1, which can be used with the ordinary tube, by detaching the tube with the flattened orifice.

By taking a burner of this description and putting two pieces on each side of the center, as represented in fig. 2, a very efficient burner is made for heating platinum crucibles in silica fusions, &c., and with such a burner, consuming 5½ to 6 cubic feet of gas per hour, I conduct most effectually all silica fusions in one hour or less, taking care to protect the crucible from the current of the air by a properly constructed short conical chimney, which chimney can be made of soap stone, sheet iron, or any other convenient material.

2.



3.



As was stated in the commencement of this article, it was not intended to describe the more complicated methods of burning gas in furnaces and by means of a blast, but to confine the remarks to the simpler forms in every day use, which can be made to accomplish all the requirements of the usual laboratory operations, and when a higher heat is required, the furnace must be our recourse, whether burning gas, charcoal or coke. The burner represented in figure 2 is the one I now employ in heating the crucible in my method of alkali determination with carbonate of lime and sal ammoniac, which method, with its more recent modifications, will be published in a very short time. The description of it, with all the minute details of manipulation, being ready for the press.

ART. XXXV.—*On the connection between Terrestrial Temperature and Solar Spots;* by CLEVELAND ABBE, Director of the Cincinnati Observatory.

WE are indebted to Sir Wm. Herschel for the suggestion that probably the presence of numerous spots on the surface of the sun is indicative of increased chemical activity, and is accompanied by increased radiation of heat. The investigations and theories of the past ten years however would lead us to an opposite conclusion from that of Herschel.

Immediately on the receipt of the Astronomische Nachrichten containing Wolf's tabular view of the relative frequency of the solar spots for the past three centuries, I made an extended comparison of the numbers therein given with such meteorological tables as were then accessible to me. After much labor I was forced to conclude that the variations of solar heat are so slight that they are masked in the local climatic peculiarites.

On further reflection, however, it seemed certain that the heat radiated from a dark spot should be of low intensity, and would therefore be largely absorbed by the aqueous vapor of our own atmosphere as well as by that of the sun. I have therefore been lately led to make a special study of the series of observations made on the Hohenpeissenberg, and published in the supplementary volume I. of the Annals of the Munich Observatory.* This series specially deserves attention because of the remarkable uniformity of the circumstances under which the observations were made; it extends from 1792 to 1850, omitting the years 1793, 1799, 1811, 1812 and 1817.

Assuming that the number of visible solar spots or groups are an index of the existing solar radation of heat, we have but to compare the number (s) expressing the relative spot frequency as given by Wolf with the mean annual temperature, (t_1) as given by Lamont. The solution of the equation $t_1=t_0+s\tau$ gives us t_0 and τ, which latter is the coefficient of solar spot influence on the radiation of heat.

The accompanying table exhibits for each year the value of s and t_1,—the latter expressed in degrees of Reaumur.

The arithmetical mean of the annual temperatures gives

$$M_1=+5^\circ{\cdot}178\pm0^\circ{\cdot}061$$

$$\text{prob. error of one annual mean}=\pm0{\cdot}449$$

The residuals are given in the column t_1-m_1.

Introducing the term $s\tau$ we find by the method of least squares

$$(t_1)=+5^\circ{\cdot}450(\pm0^\circ{\cdot}086)-s\times0^\circ{\cdot}00789(\pm0^\circ{\cdot}00204)$$

$$\text{p. e. of one annual mean}=\pm0^\circ{\cdot}430.$$

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* Volume VII, containing the continuation of this series has not yet been received.

The residuals are given in the column $t_1-(t_1)$.

Year.	s.	t_1	t_1-m_1	$t_1-(t_1)$	t_2	t_2-m_2	$t_2-(t_2)$
		°	°	°	°	°	°
1792*	53	5·38	+0·20	+0·38	+7·11	+0·28	+0·43
94	24	6·16	+0·98	+0·90	7·77	+0·94	+0·85
95	16	5·66	+0·48	+0·34	7·32	+0·49	+0·34
96	9	5·36	+0·18	−0·02	6·90	+0·07	−0·14
97	6	6·06	+0·88	+0·66	7·77	+0·94	+0·71
98*	3	5·30	+0·12	−0·13	7·61	+0·78	+0·53
1800	10	6·24	+1·06	+0·87	8·68	+1·85	+1·65
01	31	5·56	+0·38	+0·35	7·23	+0·40	+0·37
02	38	4·64	−0·54	−0·51	7·50	+0·67	+0·70
03	50	4·84	−0·34	−0·22	6·56	−0·27	−0·15
04	70	5·31	+0·13	+0·41	6·87	+0·04	+0·32
05	50	4·09	−1·09	−0·97	5·67	−1·16	−1·04
06	30	6·24	+1·06	+1·03	7·54	+0·71	+0·67
07	10	5 87	+0·69	+0·50	7·44	+0·61	+0·41
08	2	4·90	−0·28	−0·53	6·47	−0·36	−0·62
09	1	6·09	+0·91	+0·65	6·80	−0·03	−0·30
10*	0	6·56	+1·38	+1·11	8·20	+1·37	+1·09
13	14	4 39	−0·79	−0·95	6·07	−0·76	−0·93
14	20	4·34	−0·84	−0·95	6·16	−0·67	−0·79
15	35	4 05	−1·13	−1·12	6·15	−0·68	−0·63
16*	46	3·66	−1·52	−1·43	5·31	−1·52	−1·43
18	34	5·57	+0·39	+0·39	7·30	+0·47	+0·46
19	22	5·58	+0·40	+0·30	7·30	+0·47	+0·27
20	9	4·38	−0·80	−1·00	6·21	−0·62	−0·83
21	4	5·42	+0·24	0·00	7·07	+0·24	−0·01
22	3	6·37	−1·19	+0·94	8·09	+1·26	+1·01
23	1	4·82	−0·36	−0·62	6·67	−0·16	−0·43
24	7	5·83	+0·15	−0·06	6·85	+0·02	−0·20
25	17	5·27	+0·09	−0·05	7·00	+0·17	+0·03
26	29	5·15	−0·03	−0·07	6·80	−0·03	−0·08
27	40	5·06	−0·12	−0·07	6·15	−0·68	−0·63
28	52	5·46	+0·28	+0·42	7·04	+0·21	+0·35
29	54	3·99	−1·19	−1·03	5·06	−1·77	−1·62
30	59	5·00	−0·18	+0·02	6·30	−0·53	−0·34
31	39	5·39	+0·21	+0·25	6·87	+0·04	+0·07
32	22	4·96	−0·22	−0·32	6·70	−0·13	−0·23
33	8	5·17	−0·01	−0·22	6·07	−0·66	−0·97
34	11	5·99	+0·81	+0·63	7·88	+1·05	+0·86
35	46	4·69	−0·49	−0·40	6·26	−0·57	−0·48
36	97	4·98	−0·20	+0·30	6·51	−0·32	−0·18
37	111	4·23	−0·95	−0·34	5·74	−1·09	−0·48
38	83	4·20	−0·98	−0·60	5·76	−1·07	−0·68
39	68	5·10	−0·08	+0·20	6·82	− ·01	+0·26
40	52	4·38	−0·80	−0·66	5·96	− ·87	−0·73
41	30	5·60	+0·42	+0 39	7·37	+ ·54	+0·50
42	20	5·09	−0·09	−0·20	6·80	− ·3	−0·15
43	9	5 24	+0·06	−0·14	6·79	− ·4	−0·25
44	13	4·67	−0·51	−0·68	6·32	− ·51	−0·68
45	33	4·87	−0·31	−0 32	6·58	− ·25	−0·26
46	47	6·24	+1·06	+1·16	7·87	+1·04	+1·14
47	79	5·05	−0·13	+0·22	6·76	− ·7	+0·28
48	100	5·62	+0·44	+0·96	7·36	+ ·53	+1·05
49	96	5·27	+0·09	+0·58	7·03	+ ·20	+0·69
1850	64	4·76	−0·42	−0·19	6·45	− ·38	−0·15

That the probable errors are on the whole very little diminished, is owing to the presence of a few large discordances; on the other hand, the small probable error of the coefficient of s would indicate that it has a real existence.

As the daily 2 P. M. observation may be supposed to show with special clearness the direct heating power of the sun, I have sought for a confirmation of the preceding results by applying the same formula to the annual means of the temperatures observed at this hour.

The annual mean temperature at 2 P. M. is given for each year in the column t_2 of the accompanying table.

The arithmetical mean gives

$$M_2 = +6^{\circ}{\cdot}830 \pm 0^{\circ}{\cdot}067$$

$$\text{p. e. of one annual mean} = \pm 0^{\circ}{\cdot}489$$

The solution for the co-efficient of s gives

$$(t_2) = +7^{\circ}{\cdot}108(\pm 0^{\circ}{\cdot}100) - s \times 0^{\circ}{\cdot}00801(\pm 0^{\circ}{\cdot}00221)$$

$$\text{p. e. of one annual mean} = \pm 0{\cdot}465.$$

This result therefore corroborates the former in indicating a decrease in the amount of heat received from the sun during the prevalence of spots—a result clearly in harmony with the recent investigations into the nature of the solar photosphere.

The reality of the existence of the above coefficient of s will be rendered more striking to the eye if the mean of several years' observations is taken at the period of maximum and minimum spot frequency.

It would be interesting to seek in the above residuals for evidence of other temperature periods than that dependent on the eleven year spot period. There are indeed plain indications of such a period of about fifty or fifty-five years duration—probably identical with Wolf's fifty-six year period—but our series of observations is not extended enough to justify any exact conclusion.

If we acknowledge the probability of a connection between planetary configurations and solar spots, then we are at once led to make a direct connection between the former and the temperature variations. Such an investigation I have begun and the indications are that positive results will be attained, and such as will demonstrate that the solar spots are but an imperfect index to the periodic changes in the solar radiation; these periodic changes being apparently more intimately and directly connected with the tides in the cool atmosphere surrounding the solar photosphere. The results of this investigation will be made known so soon as the recent observations on the Hohenpeissenberg can be incorporated into the work.

Cincinnati, July 20, 1870.

ART. XXXVI.—*On a new method of determining the Level-error of the axis of a meridian instrument;* by C. A. YOUNG, Ph.D., Professor of Natural Philosophy and Astronomy in Dartmouth College.

[Read at the Troy meeting of the Am. Association for the Advancement of Science]

THE inclination of the axis of a meridian instrument to a horizontal plane has hitherto been measured by three different methods: by the use of the spirit level; by examining with a collimating eye-piece the image of the wires as in nadir-point observations, the collimation having been previously determined either by reversal of the instrument or by collimators; and lastly by observing the transits of stars by reflection from an artificial horizon.

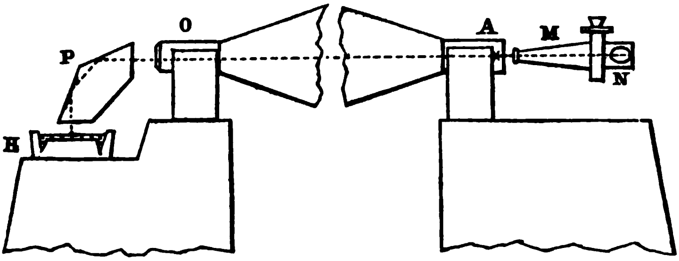
The first of these methods is by far the most used, and with portable instruments is sufficiently convenient. Still it requires a good deal of time, and, in the case of a large instrument, of hard work; and if there are sensible irregularities upon the pivots of the instrument it is a very troublesome operation to ascertain and apply the necessary corrections.

The second and third methods are still more laborious: the second gives the level error corresponding to but one single position of the telescope, i. e., with the telescope pointing downward, and is therefore liable to a constant error depending upon any malformation of the pivots which affects the instrument in this particular position: the third method can be used only when the air is perfectly still.

The method I have to propose, allows the determination of this error without any further labor than two readings of a microscope, in any position of the telescope, and without that uncomfortable climbing which is involved in the use of the striding level or nadir observations.

The annexed diagram illustrates the arrangement of the apparatus, in which, however, no regard is paid to the relative proportion of parts; the prism and mercurial horizon being grossly exaggerated in size for the sake of distinctness.

The axis of the instrument is to be fitted up as a collimator in the same manner already practised by Challis, Airy and others. In place of cross-wires, however, the extremity A should be provided with a plate of thin glass having a minute dot or circle engraved upon it, the plate being adjustable so that this dot can be brought into the geometrical axis of revolution and into the focus of the small object-glass which is situated in the other pivot, O. A reading microscope, M, is attached to the pier and provided with an ordinary collimating eye-piece N, by which light can be thrown upon the dot through the tube of the microscope. This enables us to measure the vertical distance between the dot and its image formed by reflection in the manner to be described.



Opposite O is fixed a prism shaped like that of a camera lucida, which by *two* total reflections bends the light through a right angle. Immediately below it is placed a mercurial horizon. If an ordinary right angled prism were employed, producing the bend by a *single* reflection, then any disturbance of the prism would disturb twice as much the relation between the ray passing from A to O and that returning; but with a prism of the form proposed this relation is independent of any small changes in the position of the prism. Distortion of the prism, which is hardly to be feared, could alone do any harm.

A prism of this form and mercurial horizon, thus combined, form in effect a vertical plane mirror, whose verticality is independent of any small instability of the pier upon which it is mounted.

It is then easy to see that if the dot be accurately centered and if the axis is level the image of the dot will exactly coincide with the dot itself, provided that the reflecting angle of the prism be exactly 135°. If, however, the angle vary slightly from this the image of the dot will fall above or below the dot itself by a small amount, which will be constant, and can be determined once for all by any one of several different methods. Any deviation of the axis from horizontality will immediately be indicated by a change in this distance twice as large as the deviation itself, and may be accurately measured by the microscope. Any inaccuracy in the centering of the dot is immediately eliminated by taking two measurements in opposite positions of the telescope.

The mercurial horizon employed should of course be so devised as to be free from tremors as far as possible. The form recently described by J. H. Lane, of the U. S. Coast Survey, appears to leave little to be desired in this respect.

There is no difficulty in arranging the apparatus so that the ordinary illumination of the wires at the eye-piece of the telescope shall be effected by the light transmitted through the body of the microscope. So arranged, the apparatus remains in constant readiness for use, and, as before remarked, requires only the labor of taking two microscope readings for each determination of level error, without involving any disturbance of the setting of the instrument.

I may add in closing, that this virtual mirror of constant inclination to the horizon may easily find other applications,—as for instance in determining the horizontal points of a vertical circle where the object glass of the instrument is not so large as to require a too unwieldy and expensive prism.

Hanover, N. H. Aug. 1, 1870.

ART. XXXVII.—*Influence of Temperature on the modulus of Elasticity of certain Metals;* by F. KOHLRAUSCH, Ph.D., and FRANCIS E. LOOMIS, Ph.D.

[Communicated in extracts to the Academy of Sciences, Göttingen, May 7, 1870.]

THE results obtained by Wertheim,* in his investigations respecting the influence of temperature on elasticity, have thus far generally served as a basis for calculation. It will be observed in comparing these data, that for certain substances, especially for iron, the singular phenomenon manifests itself that for different temperatures the variations of the modulus undergo a change of sign. From 0° C. to 100° the modulus increases, and then from 100° to 200° decreases. This increase is certainly altogether contrary to what would naturally have been expected. Such a maximum, however, is still more remarkable. In view, therefore, of the uncertainty of the results of investigations thus far made known, and the importance of an accurate knowledge of the variations of the modulus of elasticity for different temperatures in many of the finer measurements, the present investigations were undertaken with a view if possible of obtaining more reliable results for some of the metals of greatest practical utility. For practical purposes it would suffice to determine the variations of elasticity within the limits of the more ordinarily occurring temperatures. This was partially accomplished by Kupffer, whose investigations we shall refer to hereafter. Nevertheless in consequence of the results obtained by Wertheim, it appeared of especial interest to determine again the variations in regard to their uniformity, as well as in regard to a change of sign. The observations were extended therefore to high temperatures, in general from 90° to 20° C., and in one case very nearly to 0°. Within these limits the variations in the elasticity can be determined from the observations with great accuracy. Since they show a remarkable uniformity, there can be no hesitation in assuming the resulting empirical formula as very nearly accurate, considerably beyond these limits.

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* Pogg. Ann. Erg.. Band 2, S. 61; Ann. de Chimie, 3me S., T. 12, p. 443.

It is scarcely necessary to observe that the investigations of Wertheim have afforded the most valuable material for a knowledge of the coefficient of elasticity. His method of observation, however, is little adapted to afford a solution of the question here under consideration. For Wertheim determined the absolute moduli of elasticity by means of the dilatation of bars and wires at different temperatures. The difficulties besetting an absolute determination of the variations of quantities amounting in all to only about $1\frac{1}{2}^{\text{mm}}$ are obvious. They were especially serious for the higher temperatures, since the imperfect heating apparatus of Wertheim could not be kept at a constant temperature for any considerable period of time, and, in consequence, the variations in length due to the fluctuations of temperature, inevitably exercised a serious influence on those arising from a change in elasticity. Wertheim himself designated these investigations as not rigorously accurate.* It would unquestionably have been more rational to confine the absolute determinations to ordinary temperatures, and to determine the influence of the temperature by means of other methods which here as in other cases, where it is a question merely of variations, are capable of a much greater sensibility. The influence of the fluctuations of the temperature on the length of the object will always occasion serious inconvenience in investigations on dilatation.

All these difficulties disappear, however, and at the same time the most accurate method of observation is obtained, by employing for investigation the torsion elasticity, whose choice is further to be recommended from the fact that torsion is so generally employed in measurements. If a wire is loaded with a weight, and set in vibration about its vertical axis, the reciprocal value of the square of the time of vibration affords a direct measure for the coefficient of the torsion of the wire. Since observations of the period of vibration are among the most accurate known in physics, the variations of elasticity may be thus determined with all the rigor desirable.

1. *Apparatus and mode of observation.*—The following dispositions were adopted for the practical application of this method. (See fig. in about $\frac{1}{12}$ natural size). The vibrating body consisted of a cylindrical leaden weight with vertical axis. Above it, attached to a brass rod, is a mirror for the purpose of observation with telescope and scale. The wire is clamped at each end

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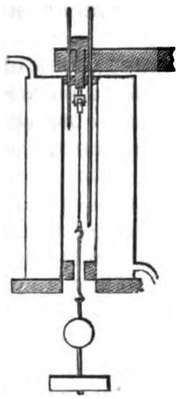
* Ann. de Chimie, 3$^{\text{me}}$ S., T. 12, p. 400 and 401.

between brass plates, of which the lower pair could be united to the weight by means of a upsilon rest, and the upper pair in like manner to a brass rod screwed into a wooden pivot. The latter was so secured in a solid support fixed to the wall as to be capable of rotation. The torsion vibrations were communicated to the weight by revolving the pivot by means of a lever. (Omitted in the figure).

2.



The space within which are enclosed the wire and a very considerable length of the connecting pieces is the hollow core of a double cylinder of tin, so constructed that the space inclosed between the two cylinders could be heated. For the determination of the temperature, three thermometers were prepared, such that the quicksilver reservoirs are at different distances from the zero point. When the graduated tubes project from the apparatus from the division 0°, the quicksilver reservoir of one of the thermometers is at a height corresponding to the middle of the wire and close beside it. The other two quicksilver reservoirs are at each end of the wire, and arranged symmetrically around it. The two latter thermometers alone are represented in the figure. The readings were made by means of a telescope.

The tin cylinder was tightly inclosed on all sides with a covering of felt of about a centimeter in thickness. It stood upon a support attached to the wall. The weight and mirror were contained in a box closed in front with a glass slide. These details are omitted in the figure.

It was originally designed to fill the vessel with water at different temperatures, but a simpler method, at first only employed to test what results might be expected, proved better adapted to the end in view. The felt envelop renders the loss of heat so gradual, that the empty apparatus, previously heated with steam, cools with sufficient slowness to permit the period of vibration at different temperatures to be observed with accuracy during the process. Accordingly the apparatus was heated for a series of observations by the admission of steam, until the temperature became tolerably stationary. The steam connection was then interrupted, and the temperature and vibrations alternately observed. Thus the variations of the elasticity of a wire for different temperatures are obtained in the course of a few hours. This simple mode of procedure affords at the same time the advantage of leaving the apparatus untouched from the beginning to the end of a series of observations, excepting only a slight turning of the wooden pivot, when the amplitude of the vibrations becomes too small. Several days were required for hot water to assume the temperature of the room, and it is not advisable to compare together observations made at long intervals, since the longer the interval the greater also becomes the danger that the conditions of the observation change in a manner to exercise on the period of vibration an influence feeble indeed, yet perhaps sensible, in view of the slight extent of the variations under consideration. For the same reason there are objections to employing water at different temperatures, since its renewal is scarcely possible without jarring the apparatus.

2. *Reduction of the observations.*—In the calculation of the data obtained from observations made with gradually diminishing temperatures, certain corrections are necessary, which render the reductions somewhat complicated. The manner in which the calculation was conducted, can be best explained by means of an example in detail. It is relative to a copper wire.

Each period of vibration was obtained according to the well known method of Gauss from two times of elongation, of which each depended in this case on six observations of the passage of the spider line through the position of rest. These times of elongation are contained in the first column of Table I. (See below). In the second column is contained the number of vibrations transpired between these times; in the third the period of vibration obtained by the division of the intervals in column one, by the numbers in column two.

TABLE 1.

Times of elongation.			Number of vibrations.	Period of vibration.	Temperature		
					Observed.	Corrections.	Corrected.
h	m	s		sec.	°	°	°
10	54	29·93	10	20·9970	79·4	—3·06	76·34
	57	59·90	9	20·9667	75·2	—2·95	72·25
11	1	8·60	9	20·9422	71·8	—2·83	68·97
	4	17·08	24	20·9000	66·4	—2·61	63·79
	12	38·68	29	20·8558	58·8	—2·28	56·52
	22	43·50	25	20·8248	52·7	—2·04	50·66
	31	24·12	39	20·7849	46·7	—1·78	44·92
	41	54·73	36	20·7614	41·3	—1·56	89·74
	57	22·14	58	20·7352	36·4	—1·36	35·04
12	17	24·78	92	20·7065	31·2	—1·15	30·05
	49	9·78	96	20·6861	27·0	—1·00	26·00
1	22	15·65	15	20·6767	25·2	—0·92	24·28
	27	25·80					

The temperatures, column 4, corresponding to the several periods of vibration, were obtained by a graphical representation of all the observed temperatures. The arithmetical mean was first taken of each three simultaneous readings of the thermometers, and then traced on coördinate paper, with the time of its observation as abscissa. The temperature for any moment can then be obtained from the curve, with an accuracy of about 0°·2 for the higher and about 0°·1 for the lower temperatures. The numbers in the 4th column are these mean temperatures for the several periods of vibration, i. e. they give the reading of the thermometers for the mean moment between the beginning and end elongation. When the interval of time is so considerable that the corresponding curve cannot be regarded as a straight line, the temperature τ would be thus obtained somewhat too low. To determine the true value, the arithmetical mean τ_0 was taken from the temperatures which correspond to the beginning and end time of elongation. The correct temperature is then $\tau + \frac{\tau_0 - \tau}{3}$.

Correction of the readings of the thermometers.—The numbers in the 4th column give the true temperature of the wire on condition, 1st, that the thermometers are accurate; 2d, that their temperature for each instant is the same as that of the space in which they hang. The heavier connecting pieces attached to each extremity of the wire extend so far within the heated space that the difference of temperature which might result in the wire from conduction may be neglected. It may be assumed, therefore, that the very fine wire follows sensibly the temperature of the space within which it is suspended. Since finally the readings of the upper and lower thermometers differed from the mean of all three in the highest temperatures only some 2°·5, the mean can be regarded as the temperature of the wire, without sensible error.

The above two conditions, however, must be fulfilled, or since they are not fulfilled, the readings must be corrected.

1. The three thermometers were therefore first calibred, and their fixed points determined. Thence tables were constructed containing the corrections with regard to a quicksilver thermometer theoretically accurate. The three tables united in one, permit then to apply the correction directly to the mean value of the three readings.

2. Secondly, in consequence of the considerable mass of quicksilver, the thermometers do not follow the temperature of the space so completely as can be assumed of the fine wire. Now according to well known laws, the amount of heat lost by a body, and consequently therefore also the variation of its temperature, is proportional to the difference of its temperature and that of the surrounding space. Since in the present case, the velocity of the variation of temperature of the thermometers is known for each moment, it is only necessary to determine once for all the relation of the true temperature to that indicated by the thermometers, to be able to calculate the difference, i. e. the correction of the readings. For this determination a very delicate thermometer was employed, whose cylindrical quicksilver reservoir possessed a diameter of only some 3^{mm}, and contained in all less than 1·5 gr. of quicksilver. It

was assumed that this thermometer follows the gradually diminishing temperature of the air to within a negligable fraction, and the three thermometers were compared with it, under conditions similar to those existing in the observations on elasticity. All three thermometers were disposed at an equal height with the thermometer of comparison, in the gradually diminishing temperature of the hollow space of the tin cylinder, and simultaneous readings were made. They were also compared together in the ordinary manner in water of different temperatures. From these observations, which were conducted by Mr. Grotrian, it resulted that the mean of the readings of the three thermometers remained 2°·02 behind the temperature of the thermometer of comparison, when the velocity of the diminishing temperature amounted to 1° in 1 min. The correction of the readings due to the mass of quicksilver is therefore

$$2{\cdot}02\,\frac{d\tau}{dt},$$

where τ denotes the temperature of the thermometers, and t the time expressed in minutes. We obtain thus the temperatures which would have been observed, if thermometers had been employed containing the same amount of quicksilver as the thermometer here employed for comparison.

3. Finally a *third correction* is necessitated by the fact that the column of quicksilver of the thermometers, beginning with the zero point of graduation, possessed a lower temperature in consequence of projecting outside of the apparatus. There exists as yet no simple method of determining the mean temperature of the projecting column. It was assumed to have possessed the temperature of the air to the extent of three parts, and that of the interior of the apparatus one part. If τ_0 denotes the temperature of the room, the correction of the reading τ is therefore $-\frac{3}{4}\tau(\tau-\tau_0)\ 0{,}00016.$

This value cannot be far removed from the truth. It coincides with the difference which the determination of the boiling point afforded, according as the thermometers were placed in the steam only to the zero point of graduation, or to the boiling point. The uncertainty of this assumption is at all events of small importance, since the whole correction for the highest temperature (90°) amounts only to 0°·8.

Thus, three corrections are to be applied to the readings of the thermometers. The first, which relates to the ordinary errors, is a function of the temperature; the second, in consequence of the mass of the quicksilver, is a function of the velocity of the diminution of the temperature; the third, finally, in consequence of the column of quicksilver projecting from the apparatus, depends on the difference of the outer and inner temperatures. Nearly the same conditions prevailed, however, during all the series of observations. The temperature of the room differed from the mean at most 5°, and consequently the velocity of the diminution of the temperature of the apparatus depends, within negligable differences, on the inner temperature alone, as is confirmed indeed by the observations themselves. Thus the true velocity of the diminution for different temperatures could be determined from a few series of observations, whence by multiplication with 2·02, (see p. 355) the correction itself is obtained as function of the temperature.

In like manner, for the calculation of the third correction in the formula (p. 355), it was permissible to take $\tau_0 = 20°$ as a mean for all the observations.

The corrections may now be very much simplified by uniting all three in tabular form, viz: for reading,

0° 10° 20° 30° 40° 50° 60° 70° 80° 90°

the correction is

0°·0[*] −0°·4 −0°·7 −1°·1 −1°·5 −1°·9 −2°·3 −2°·8 −3°·1 −3°·2

The interpolation for intermediate temperatures was accomplished by means of a graphical representation of this table. In one case, however, when the apparatus was filled with water, and the velocity of refrigeration could be neglected, the corrections 1 and 3 were applied separately.

Correction of the observed periods of vibration.—1. In the present investigations we are seeking the variations of the modulus of elasticity caused by a change of temperature, whereas we observe the directive force of the whole wire, which, in consequence of the heating, experiences a dilatation,as well as a change in its elasticity. Since this directive force of the wire depends on the length and section as well as on the modulus of elasticity, it is necessary to investigate what corrections are thereby necessitated.

It is an interesting fact that no correction at all is necessary. The dilatation in length and breadth completely neutralize each other.

If we denote by

l the length of the wire,
r the radius,
m the mass of the unity of length,
K the moment of inertia of the vibrating weight,
t the time of vibration,

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[*] It is merely an accidental coincidence, that there is no correction to be applied to the reading 0°. The mean of the zero points of the three thermometers happens to be perfectly accurate.

D the directive force of the wire, i. e. the moment of revolution which it exercises for a torsion angle of $\frac{180^\circ}{\pi} = 57^\circ \cdot 296$.

s the modulus of torsion of the substance constituting the wire, we have*

$$D = \frac{\pi^2}{t^2} K$$

and
$$s = \frac{Dl}{gmr^2} = \frac{\pi^2 K}{g} \frac{l}{mr^2 t^2}, \tag{1}$$

where g denotes the acceleration due to gravity, and s is the number, the quintuple of which (according to the theory of Poisson, i. e. when the ratio of the transversal contraction to the longitudinal dilatation is $=\frac{1}{4}$) multiplied by g gives the square of the velocity of sound in the substance.

When now in consequence of a change of temperature, the elasticity and dimensions of the wire vary, the time of vibration t changes also. If we denote the variations of s, l, m, r and t, occurring for a small change of temperature, by $d\,s$, $d\,l$, $d\,m$, $d\,r$ and $d\,t$, we obtain by differentiating (1) logarithmically,

$$\frac{ds}{s} = \frac{dl}{l} - \frac{dm}{m} - \frac{2dr}{r} - \frac{2dt}{t}.$$

But on the supposition that the wire experiences an equal dilatation in all directions on account of the temperature $\frac{dl}{l} = \frac{dr}{r}$. Also since m denotes the mass of the unity of length, obviously

$$-\frac{dm}{m} = \frac{dl}{l} = \frac{dr}{r}.^*$$

Therefore
$$\frac{ds}{s} = -2\frac{dt}{t}. \tag{2}$$

The modulus of torsion s is therefore without correction reciprocally proportional to the square of the time of vibration.

The definition of the modulus of elasticity assumed here as a basis, denotes, with regard to the dilatation, the weight which must be suspended to a wire, whose unity of length is the same as the unity of its mass, to double the length. In practice it is customary, though frequently less convenient, to employ the section instead of the weight of the unity of length. In such case, the above expression (1) must be multiplied further by the density of the substance, and the coefficient of the cubical dilatation 3α must be subtracted from formula (2), so that

$$\frac{ds}{s} = -\frac{2dt}{t} - 3\alpha.$$

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* $m \cdot l = (m + dm)(l + dl)$

$$0 = l\,dm + m\,dl$$

$$\frac{dm}{m} = -\frac{dl}{l}.$$

2. A small correction is necessary when the moment of inertia of the vibrating weight changes in consequence of fluctuations in the temperature. In general it is inconsiderable in the same series of observations. The temperature in the box containing the vibrating weight was observed from time to time, and the period of vibration corrected when changes of temperature occurred. This was accomplished by multiplying the observed time of vibration by $1-0{,}00003.\Delta\tau$, where 0,00003 denotes the coefficient of the linear dilatation of lead, and $\Delta\tau$ the variation of temperature.

3. Theoretically the time of torsional vibrations should be independent of the amplitude. In fact in the present observations on the brass wire, no influence was perceptible. The iron wire, hereafter to be discussed, was especially investigated with respect to such an influence, however, and showed a manifest, though very small, increase of the time of vibration for an increment of amplitude. The increase appeared to be proportional to the latter, and on this supposition amounted to $0{\cdot}000037^{\text{sec.}}$ or $\frac{1}{300000}$ of the whole time for one division of the scale ($\frac{1}{7000}$ to one degree of arc). A series of observations on the copper wire were incidentally of a nature to afford the means of calculating the increase approximately. It amounted to $0{\cdot}000015^{\text{sec.}}$ for one division of the scale. The times of vibration were reduced by means of these numbers to an infinitely-small amplitude.

Although it would be of interest to examine this phenomenon more minutely, it hardly enters within the scope of the object in view in these observations. For in consequence of the inconsiderable amplitudes which we employed (at most 200 divisions of the scale, or some 3^{c}) the correction in extreme cases amounted only to $0{\cdot}005^{\text{sec.}}$, and it is almost completely compensated in its influence on the final result.

3. *Calculation of the coefficient of temperature.*—We have thus far obtained a series of temperatures with the corresponding times of vibration. From each two pairs of corresponding values may be readily derived the variation of the elasticity for 1°. We express it as a function of the total elasticity at 0°. If ε_0 designates the modulus of elasticity at 0°, and ε_1, ε_2, the moduli corresponding to the temperatures τ_1, τ_2, the desired decrease for 1° is found from formula (1) to be

$$\frac{1}{\varepsilon_0}\,\frac{\varepsilon_1-\varepsilon_2}{\tau_2-\tau_1}=t_0^2\,\frac{\dfrac{1}{t_1^2}-\dfrac{1}{t_2^2}}{\tau_2-\tau_1},$$

where t_1 and t_2 designate the times of vibration for the temperatures τ_1 and τ_2, and their squares are inversely proportional to the modulus of elasticity.

The following Table 2 contains in the second column the preceding diminutions of the modulus of elasticity for 1° taken from Table 1, by combining in it each two adjoining values. The time of vibration t_0 for 0° was obtained from the graphical representation of the observations by prolongation of the curve, a method which affords sufficient accuracy. Thus it was found that $t_0=20{\cdot}58$ sec. In the first column is contained the mean of the two temperatures at which the times of vibration were observed.

TABLE 2.

Temperature.	Decrease of the coefficient of elasticity for 1° as func. of the coeff. of elast. at 0°. Observed.	Calculated.	Observ.—Calc.
74°	0,00068	0,00068	±0,00000
71	69	67	+ 2
66	75	65	+ 10
60	56	62	− 6
54	50	60	− 10
48	65	57	+ 8
42	43	55	− 12
37	53	53	± 0
33	55	51	+ 4
28	48	49	— 1
25	0,00052	0,00047	+0,00005

The numbers of the second column can be regarded as the values of $-\frac{1}{s_0}\frac{ds}{d\tau}$ for the temperature τ of the first column. It will be observed that this mode of observation affords the decrease of the modulus of elasticity for 1° with tolerable accuracy, even for the times of vibration for two temperatures differing only by a few degrees. (The greatest difference is 7°, the smallest 2°). This decrease for the brass wire is noticeably greater for the higher temperatures than for the lower.

The numerical law of the variation is to be derived from all the observations. We seek to represent it by the formula

$$s=s_0\,(1-a\,\tau-b\,\tau^2).$$

It would obviously be more in accordance with the laws of the calculus of errors to determine the values of the coefficients s_0, a and b by means of least squares from all the observed values of s or $\frac{1}{t^2}$ for all temperatures. This would prove, however, when applied to all the series of observations, excessively laborious. It would be hazardous to unite previously together all the observations made on the same wire, since the time of vibration for the same temperature appears to be subject to slight variations from one day to another, probably in consequence of the considerable heating to which the wire is subjected.

By differentiating the above equation we obtain

$$-\frac{1}{\varepsilon_0}\,\frac{d\varepsilon}{d\tau}=a+2b\tau.$$

The left hand expression denotes the values contained in the second column of Table 2. If therefore we substitute these values in the above equation, the coefficients a and b can be readily calculated.

By means of least squares there results for the brass wire

$$a=0{,}000368 \qquad 2b=0{,}00000425$$

by means of which the figures in the third column were calculated. In view of the small differences of temperature the accordance must be regarded as satisfactory. It appears still more manifest when the time of vibration itself is calculated. Since a and b are known, the most probable value of the time of vibration for 0° may also be readily determined by means of least squares. The calculation gives $t_0 = 20{\cdot}5696^{\mathrm{sec.}}$

The times of vibration calculated from this value of t_0, together with the times observed, are given in the following Table 3. The difference between the observed and calculated values of the times of vibration amounts at most to $\frac{1}{130}$ of a second, and indicates sufficiently the admissibility of the simplified method of reduction, although the regularity in the sign of the differences would seem to indicate that the rigorous method would afford a still greater accordance.

TABLE 3.

Times of vibration.

Temperature.	Observed.	Calculated.	Observ.—Calcul.
76°·34	20^{s}·9970	20^{s}·9986	—0^{s}·0016
72·25	20·9667	20·9680	—0·0013
68·97	20·9422	20·9443	—0·0021
63·79	20·9000	20·9078	—0·0078
56·52	20·8559	20·8592	—0·0033
50·66	20·8248	20·8218	+0·0030
44·92	20·7849	20·7869	—0·0020
39·74	20·7614	20·7569	+0·0045
35·04	20·7352	20·7309	+0·0043
30·05	20·7065	20·7041	+0·0024
26·00	20·6861	20·6837	+0·0024
24·28	20·6767	20·6751	+0·0016

4. *Temperature and modulus of elasticity of iron, copper and brass.*—The method of observation and reduction, already described and applied to an example, was employed for wires of iron, of copper and of brass. All three wires were hard drawn and of a diameter between 0·2 and $0{\cdot}3^{\mathrm{mm}}$. The copper wire was composed of copper precipitated electrolytically. The iron

and brass wires are the ordinary wires of commerce wound on spools.

To abbreviate the calculation, the times of vibration and the temperatures of each series of observations are united first in groups by taking the mean, and especial care was taken to allow about the same weight to all the several coefficients to be derived. The observed data corrected according to the rules of chapter 2, are contained in the following Table 4, in which the Roman characters denote the chronological succession of the several series of observations.

TABLE 4.

Iron.			Brass.			Copper.		
	Temp.	Time of Vibration.		Temp.	Time of Vibration.		Temp.	Time of Vibration.
	°	s.		°	s.		°	s.
I	76·34	17·6154	I	69·01	20.9412	I	74·06	12·4444
	56·87	17·5282		47·95	20.8067		47·96	12·3476
				28·85	20.7012		55·20	12·3696
II	76·55	17·5950					40 80	12·3221
	55·75	17·5087	II	72·14	20.9105		24·04	12·2667
	42·42	17·4553		48·78	20.7600			
	32·11	17·4150				II	62·60	12·3852
	21·91	17·3679	III	70·28	20·8694		22·05	12·2465
				49·11	20·7368			
III	78·73	17·5920		36·24	20·6669	III	77·27	12.4303
	59·72	17·5130		27·23	20·6193		56·16	12·3546
	20·97	17·3583					40·79	12·3028
			IV	50·05	20·7333		23·37	12·2413
IV	49·57	17·4715		42·81	20·6915			
	37·80	17·4279		34·59	20·6425			
	21·95	17·3627						
			V	19·43	20·5428			
				5·05	20·4737			

The following Table 5, in which the numbers are arranged according to the temperatures, is obtained by reducing the values contained in Table 4 in the same manner as those of Table 2 of the example.

The coefficients *a* and *b* are derived by means of least squares from the values contained in the third and fourth columns of Table 5, as in the example. Designating by s_0 the modulus of elasticity at 0°, the modulus for the temperature τ is found to be,

for iron, $s = s_0(1 - 0{\cdot}000447\tau - 0{\cdot}00000012\tau^2)$
for copper, $s = s_0(1 - 0{\cdot}000520\tau - 0{\cdot}00000028\tau^2)$
for brass, $s = s_0(1 - 0{\cdot}000428\tau - 0{\cdot}00000136\tau^2)$

The numbers of the next to the last column of Table 5 were calculated with these values. The probable error of each value of the decrease of the coefficient of elasticity for 1° for a given temperature calculated from two groups of observations amounts accordingly to ± 0·000014.

TABLE 5.

			Decrease of the coeff. of elastic. for an incr. of temp. of 1° as func. of the coeff of elast. at 0°.		
		Temp.	Observed.	Calcul.	Observ. — Cal.
Iron.	IV	29°·9	0·000467	0·000454	+ 0·000018
	II	32·2	482	455	+ 27
	III	40·3	450	457	— 07
	IV	43·7	416	458	— 42
	II	48·9	445	458	— 13
	II	66·1	459	463	— 04
	I	66·6	494	463	+ 31
	III	66·9	459	464	— 05
Copper.	III	32·1	0·000566	0·000538	+ 0·000028
	I	32·4	528	538	— 10
	II	42·3	542	544	— 02
	I	48·0	521	547	— 26
	III	48·5	531	547	— 16
	I	61·0	578	554	+ 24
	III	66·7	556	557	— 01
Brass.	V	12·2	0·000466	0·000461	+ 0·000005
	III	31·7	504	514	— 10
	I	38·4	523	532	— 09
	IV	38·7	570	533	+ 37
	III	42·7	514	544	— 30
	IV	46·4	556	554	+ 02
	I	58·5	593	587	+ 06
	III	59·7	584	590	— 06
	II	60·5	599	592	+ 07

If the other definition of the modulus of elasticity is employed (as was done by Wertheim) referring to the section, and not, as above, to the mass of the unity of the length of the wire, the coefficients of the temperature will be slightly altered in accordance with the observation on page 357. If the coefficients of dilatation for 1° are assumed to be for iron = 0·000012; for copper = 0·0000175; and for brass = 0·000019, the coefficients of τ become,

for iron,	0·000483,
for copper,	0·000572,
for brass,	0·000485.

The coefficients of the quadratic terms remain sensibly unchanged.

It is evident from the results of these observations that the mean variation of the elasticity for the three metals investigated differs but little from that of the temperature. For a change of temperature from 0° to 100° the diminution is for

Iron,	4·6	and	5·0	per cent.
Copper,	5·5	"	6·0	"
Brass,	5·6	"	6·2	"

where the numbers of the second column relate to the second definition of the modulus of elasticity.

If the variation in elasticity is compared with the influence exercised by the temperature on other properties of bodies it will be remarked that it is much greater than the cubical dilatation as well as the variations of refraction. It is of about the same order as the variation of the permanent magnetism caused by the temperature, as well as that of specific heat. The increase of the galvanic resistance on the other hand is much greater.

It results further from the sign of the quadratic term that the variation of elasticity for all three metals is the most rapid in the higher temperatures. While, however, the increase for iron is almost imperceptible, and is also very small for copper, it is quite considerable for brass. The decrease for 1° is for

	At 0°.	At 50°.	At 100°.	
Iron,	0·0447	0·0459	0·0472	per cent.
Copper,	0·0520	0·0548	0·0576	"
Brass,	0·0428	0·0564	0·0699	"

It will be observed that the differences in the variation of the coefficient of elasticity for the three metals investigated, are in the order of the height of their melting points.

The results of these investigations show no trace at all of the remarkable phenomenon of a maximum, alluded to at the beginning of this article, which would seem to be indicated for iron by the investigations of Wertheim. If, therefore, different varieties of iron do not manifest a totally different behavior, or if the modulus of the longitudinal elasticity does not undergo a very different change in consequence of temperature from that of the torsional elasticity, this anomaly must be accounted for by the imperfect method of observation employed by Wertheim. This supposition is confirmed further by the observations of Kupffer (see below), as well as by a very simple experiment. If, namely, two tuning forks are in vibration, and one of them is heated, the number of vibrations changes in the manner demanded by the assumption of a decrease of elasticity for increasing temperatures.

It is to be remarked here that Wertheim's calculation of the heat generated by condensation during vibration, loses thus all its value.*

Observations of Kupffer.†—These investigations appear to be much less generally known than they deserve, for they contain much varied and valuable material for practical use. Kupffer's observations are in general on bars vibrating transversally at the temperatures of about −10°, +15° and +80°. Unfortunately, however, very long intervals (at least a year) occur between the observations, and it would seem that he did not always employ the same bars, so that his investigations permit but a limited conclusion in regard to the law of the variations of elasticity as modified by temperature. Finally, the dilatation caused by temperature was disregarded in the numerical values given by Kupffer, a source of serious errors in view of the method of observation which was employed.

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* Pogg. Ann., Bd. 11, S. 32.

† Mem. de l'Acad. de St. Petersb. 1856, 6 ser. T. vi, p. 400.

A few of the results derived by Kupffer were obtained by means of torsional vibrations. Among these are the coefficients of temperature for the elasticity of an iron wire (piano cord), of a copper wire and of a brass wire. After having applied to these values the correction required for the dilatation of the vibrating disc, the decrease of the elasticity for 1° expressed in parts of the total elasticity is found to be

for	iron,	0·00055	(" mem." S. 446)
"	copper,	0·00082	S. 464
"	brass,	0·00039	(S. 467)

These values agree tolerably well with those found by us, with the exception of the copper wire, in regard to which it is possible that the divergence may be caused by the circumstance that we employed a chemically pure metal.

5. *The absolute moduli of elasticity of the iron, copper and brass wires calculated from torsional vibrations and from the velocity of sound.*

To determine from our investigations the absolute moduli of torsional elasticity, it is necessary to know the dimensions and mass of the wires, as well as the moment of inertia of the vibrating weight.

The latter is to be calculated from the mass of the perforated lead cylinder =1818 grs. and the interior radius =0·34 c. m. and the exterior radius =5·07 c. m. It is found to be

$$\frac{1818}{2}\left(\overline{5{\cdot}07}^2+\overline{0{\cdot}34}^2\right)=23470 \text{ grs. } \square \text{ c. m.}$$

To this must be added the moment of inertia = 40 grs. □ c. m. of the connecting pieces and the mirror, which was calculated from their size and form. The total moment of inertia therefore is $K=23510$ grs. □ c. m.

Further was determined

		Iron.	Copper.	Brass.
The length of the wire,	$l=$	20·80	20·75	20·80 c. m.
The mass of one c. m. length,	$m=$	0·00301	0·00655	0·00403 grs.
The density,	$\Delta=$	7·82	9·00	8·41
The time of vibration at 20°,	$t=$	17·35	12·23	20·55 sec.
The radius of the wire,	$r=$	0·0111	0·0152	0·0123 c. m.

The modulus of torsion T is derived from these data by means of the formula, p. 357,

$$T=\frac{\pi^2 K}{g}\,\frac{l}{mr^2t^2}.$$

It is found to be

for iron =444 kilometers.
" copper=217 "
" brass =190 "

If it is desired to follow the definition customary in practice, and to refer the modulus to the section instead of to the mass of the unity of length, these numbers must be multiplied by the corresponding densities; whereby they become

for iron,	3470	$\frac{\text{kilogrammes}}{\text{quadratmillimeter.}}$
" copper,	1950	"
" brass,	1600	"

The velocity of sound was measured in the same species of wires by stretching them in a Weber's monochord, and by varying the length, comparing the longitudinal tone with tuning forks. The normal tone of the set of tuning forks was determined by comparison with two normal tuning forks of Appunn.

The modulus of dilatation of the wire (i. e. the length of a wire of the same species possessing the weight necessary to double the original length), is obtained from the velocity of sound c by means of the formula $A=\frac{c^2}{g}$, where g denotes the acceleration due to gravity. The value A′ ordinarily employed in practice, and made use of by Wertheim, (i. e., the weight which hung on a wire whose section is unity would double the length), is obtained as above by multiplication with the density. Thus it was found

	Velocity of sound.	Modulus of elasticity. A.	A′.
Iron,	5050 meters.	2580 kilometers.	20310 $\frac{\text{kgr.}}{\text{quad. mill.}}$
Copper,	3640 "	1350 "	12140 "
Brass,	3380 "	1170 "	9810 "

Wertheim gives A′ for iron wire =19445, copper =12536, brass =9000 $\frac{\text{kgrs.}}{\square\ \text{m.m.}}$

According to these observations, the moduli of torsion bear to the moduli of dilatation, for all three wires very nearly the same ratio. The ratio $\frac{A}{T}$ is

for iron,	5·85
" copper,	6·23
" brass,	6·15
Mean,	6·07

The deviations of these values from the mean are perhaps due to errors of observation, especially in the determination of the densities, since in order to be sure of investigating the same material as in the original experiments, but small masses could be employed. It is possible too that the deviation of the mean from the whole number 6, is to be sought in these errors.

Without entering on a more extended investigation of this point, and without discussion as to whether the customary theory of elasticity is applicable to such thin wires, or whether the lack of isotropy, or the relations of superficies, forbid this application, it may be remarked that according to the theory

$$\frac{A}{T} = 4(1+\mu),$$

where μ denotes the ratio of the transversal contraction to the longitudinal dilatation. By a comparison of the torsional and longitudinal vibrations, there would result accordingly for our three wires very nearly the same value for μ. If it is assumed that $4(1+\mu)=6$ there results $\mu=\frac{1}{2}$. This is the extreme value permitted by the theory, which would correspond to a volume unchanged by dilatation.

It may suffice to have indicated the fact. It would lead us too far from the chief object of our investigation to develope the subject more in detail.

ART. XXXVIII.—*On the Oxy-Calcium Light as applied to Photo-Micrography;* by Lieut. Col. J. J. WOODWARD, Assistant Surgeon, U. S. Army. Report to the Surgeon General of the U. S. Army, dated June 4, 1870.

SINCE the preparation of my report of January 4, 1870, on the use of the Magnesium and Electric lights in Photo-micrography, I have made some experiments with the Oxy-calcium, or Hare's light, as a source of illumination for the same purpose, and have succeeded in obtaining excellent pictures with powers as high as a thousand diameters. This result appears to me of considerable importance, both because of the comparative cheapness of this light, and because the apparatus for its production is so common as to be practically within the reach of every microscopist. In addition to these advantages the oxy-calcium light possesses the quality of steadiness to a greater degree than either the electric or the magesium lamp and requires much less trouble and skill to manage.

For the purposes of my experiments, I made the hydrogen as I consumed it, in a Hare's self-regulating generator, by the action of dilute sulphuric acid on scraps of ordinary sheet-zinc.

The oxygen was sometimes made in the usual way from chlorate of potassa, sometimes purchased compressed in iron cylinders; in either case it was transferred to a large sheet-iron gasometer for use. The gases were burned under a pressure equal to a column of water fourteen inches high. I used for lamp one of the first-class magic-lanterns, manufactured by J. W. Queen & Co. (No. 924, Chestnut street, Philadelphia, Pennsylvania,) in which the disc of lime is revolved by clock-work before the burning jet of gas and a fresh surface constantly presented to the flame. I simply removed from the lantern the lens intended to magnify the image on the slides, when the apparatus is in ordinary use, and allowed the cone of light proceeding from the large condenser of the instrument to fall upon the achromatic condenser of the microscope, in the same manner as described and figured for the magnesium lamp in my report of January 4th, a reference to which will render any description of the arrangement of the microscope and of the sensitive plate unnecessary in this place.

I employed the ammonio-sulphate cell, as I do in taking Photo-micrographs with other sources of light, but found I could dispense with the ground glass which is necessary in photographing so many objects, if sunlight or the electric lamp is employed; a large portion of the lime disc being luminous, the resulting mixed divergent pencil, like that obtained from the magnesium lamp, does not produce the interference phenomena which result when tissues and many other objects are illuminated by powerful parallel rays. This circumstance, however, renders the calcium light inferior to the sun and the electric lamp, in the resolution of the Nobert's plate and certain lined test objects.

I did not find the time of exposure differed materially from what I had given in making photographs of the same objects with the magnesium lamp, and the pictures produced were not inferior to these in quality. This arose from the fact that the greater steadiness of the calcium light permitted the use of condensers which concentrated the light to a greater degree than I had found advantageous with the magnesium lamp, and not from equality in the actinic power of the two sources of illumination. I have recently made some experiments with the view of obtaining positive information with regard to the comparative actinic energy of the electric, magnesium and calcium lamps which I employ. For this purpose all condensers being removed the divergent pencil proceeding from each lamp in turn was permitted to fall, for the space of five seconds, on an exposed circular portion of a sensitive plate thirty feet distant.

The whole operation was completed in less than a minute, when the plate being developed in the ordinary way three circular spots appeared as the results of the exposures. The spot produced by the electric light was intensely black, that by the magnesium of a rich middle-tint, while the circle impressed by the calcium light was extremely pale. Want of time prevented me from continuing these experiments and obtaining as I desired numerical values for the relative actinic powers of these sources of illumination under definite conditions; this I have however regretted the less, as the actual energy of the naked flames is not really the measure of their availability in photo-micrography; here the question of steadiness, involving, as it does, the possibility of great concentration, plays a most important part and materially modifies the result.

So far as I know, the Calcium light has never before been successfully employed as the source of illumination for making Photo-micrographs in this country. My friend Dr. R. L. Maddox, however, writes me that it has been experimented with in England by Drs. Abercrombie and Wilson. He thinks they used powers as high as an eighth with pleasing results. This information has directed my attention to the essay of Dr. Wilson in the Popular Science Review for 1867, volume vi, page 54, in which that gentleman gives in detail the process employed by himself and Dr. Abercrombie. He experimented with an oil lamp and with the Oxy-calcium and magnesium lights: "I can scarcely think it would be used now that the more active light of magnesium is within the reach of every one." And of the magnesium: "The light fails only in steadiness, and if some means could be devised for burning the metal uniformly and at a fixed point nothing would be left to desire." Dr. Beale (How to work with the Microscope, 4th edition, p. 248) tells us that some of the pictures of these gentlemen were remarkably good, "they possess a peculiar delicacy in the half tones and the shadows, with much roundness of the objects, but the definition, as might be expected, does not quite equal in some of the finest markings, prints obtained from sun negatives." A perusal of Dr. Wilson's paper will show that my process differs from his in the use of the following precautions: the interpolation of an ammonio-sulphate cell to exclude the non-actinic rays, the use of lenses specially made for photography for all powers from the ⅛th down, the use of much larger condensers to concentrate the light and so to shorten the exposure, and in the case of the magnesium light, in the use of a clock-work lamp to increase the steadiness of the illumination. Each of these points are in my judgment essential to obtain the best results.

I learn from the same letter of Dr. Maddox that he had himself made experiments with the magnesium lamp some time before those of Abercrombie and Wilson. He used powers as high as a fifth, and appears to have obtained better results than I supposed any one had done prior to the publication of my report. He gives me the following account of his experience: "The first picture I took with the magnesium light was done in a very rude way. An inch and a quarter of wire was held in a small spirit flame and advanced by hand as burnt. The objective was Beck's $\frac{2}{3}$ds, the object the sycamore-leaf insect, and about $\frac{3}{8}$ths of an inch of wire remained after use. I sent a print, with a sun print of the same to the British Journal of Photography, and in the number for July 1, 1864, you will find some remarks by myself and the editors. Now to try and meet any error that might arise from what we may term want of correction, I used the $\frac{2}{3}$ds with the correcting lens, which is excellent for sunlight; the picture was soft, full of half tone, but wanted, as in other pictures I had seen by artificial light, the decision of definition in the outlines." "After this I used the $\frac{1}{5}$th with the little apparatus sketched in Beale's book (page 275) and which I venture to think, embraces all that is required for its use, provided the condenser has its focus at the burning point, and that the reflector has the same." With the $\frac{1}{5}$th, fibers of cotton and muscular fibrillæ of boiled shrimp, with several other objects were taken, but I did not use any higher power, nor indeed pay much attention to the subject as I gave the preference to the sunlighted prints and negatives." I give these extracts with great pleasure as showing the experience in this direction of one of the most distinguished laborers in the field of Photo-micrography, and regret that I was not acquainted with them at the time of publishing my first report. The method of Dr. Maddox, however, differed from mine in the same essential points as that of Abercrombie and Wilson, and the peculiar fitness of the magnesium light for photographing the animal tissues and those objects generally, which require the use of ground glass when sunlight is employed, would appear to have escaped the observation of these accomplished gentlemen, and to have remained unnoted until the publication of my report.

In conclusion I append to this paper two illustrative photographs. The first, which represents the 6th square of the Möllers type-plate of the diatomaceæ, taken with Wales's $1\frac{1}{2}$ inch objective, arranged to give thirty-five diameters, will serve for comparison with the photographs of the same object with the same lens taken by sunlight and by the electric and magnesium lamps, which were published with my former report. The second represents the Navicular Lyra, taken with the Powell and Lealand's immersion $\frac{1}{16}$ arranged to magnify 1000 diameters.

ART. XXXIX.—*On the Secular Perturbations of the Planets;* by ASAPH HALL.

IN view of the recent consideration in geological speculations of the secular inequalities of the excentricity of the earth's orbit, it may be worth while to state briefly the method of treating secular perturbations, and our present knowledge of the subject.

Denoting by e, π, φ, and θ the excentricity, the longitude of the perihelion, the inclination of the orbit and the longitude of its ascending node, it has been found easier in the discussion of the problem to substitute for these quantities four others which are simple functions of them; and thus are assumed

$$h = e \sin \pi. \qquad p = \text{tang}\, \varphi \sin \theta.$$
$$l = e \cos \pi. \qquad q = \text{tang}\, \varphi \cos \theta.$$

Neglecting terms of the third and higher orders of the excentricities and inclinations in that part of the development of the perturbative function from which the secular perturbations arise, linear differential equations of the first order are established containing the first differential co-efficients of h, l, p and q with respect to the time; and it is by the integration of these equations that the secular perturbations are obtained. By this process also the equations are separated into two distinct classes admitting of separate treatment; the one class containing the differential co-efficients of h and l and the solution furnishing the values of the excentricities and the longitudes of the perihelia, and the other containing those of p and q and the solution giving the values of the inclinations and the longitudes of the ascending nodes. In this way the discussion of the problem is much simplified. The numerical co-efficients in the differential equations depend on the semi-major axes of the orbits, and on the masses of the planets. Considering the eight principal planets of our solar system, there will be of course eight values of h, l, p, and q, and these are usually distinguished by the addition of accents to the symbols.

In order to effect the integration, Lagrange, whose method is still followed, assumed

$$h = \text{N} \sin (g\,t + \beta), \qquad l = \text{N} \cos (g\,t + \beta),$$
$$h' = \text{N}' \sin (g\,t + \beta) \qquad l' = \text{N}' \cos (g\,t + \beta), \qquad \&\text{c.}$$

Differentiating these equations and substituting the value of $\frac{dh}{dt}$, $\frac{dh'}{dt}$, $\frac{dl}{dt}$, &c., in the differential equations and then eliminating the ratios of the coefficients N, N′, N″, &c., a numerical equation is obtained for the determination of g. This equation will rise to the degree denoted by the number of planets considered, and in our solar system will be of the eighth degree. If we denote by $g_1, g_2, g_3 \cdots\cdots g_8$ the roots of this equation the general integrals will be

$$\left.\begin{aligned} h &= \mathrm{N}_1 \sin(g_1t+\beta_1)+\mathrm{N}_2 \sin(g_2t+\beta_2)+\ldots\ldots+\mathrm{N}_8 \sin(g_8t+\beta_8), \\ l &= \mathrm{N}_1 \cos(g_1t+\beta_1)+\mathrm{N}_2 \cos(g_2t+\beta_2)+\ldots\ldots+\mathrm{N}_8 \cos(g_8t+\beta_8), \\ h' &= \mathrm{N}'_1 \sin(g_1t+\beta_1)+\mathrm{N}'_2 \sin(g_2t+\beta_2)+\ldots\ldots+\mathrm{N}'_8 \sin(g_8t+\beta_8), \\ l' &= \mathrm{N}'_1 \cos(g_1t+\beta_1)+\mathrm{N}'_2 \cos(g_2t+\beta_2)+\ldots\ldots+\mathrm{N}'_8 \cos(g_8t+\beta_8), \\ &\qquad\qquad \&c., \qquad\qquad\qquad\qquad \&c. \end{aligned}\right\} \quad (1)$$

The arbitrary quantities $\mathrm{N}_1, \mathrm{N}'_1 \cdots\cdots \beta_1, \beta_2$, &c., are determined by the initial values of h and l. The solution for p and q is quite similar to that for h and l.

The conditions necessary for the stability of the system are, first, that the eight roots of the equation for g shall be real and unequal, in order that outside the circular functions there may be no terms containing the time as a factor or exponent and which would therefore increase indefinitely; and secondly, it is necessary that the coefficients N may not be great in order that the excentricity may not increase so as to render divergent the series which have been assumed in the solution to be rapidly convergent.

The actual numerical solution by several eminent astronomers, Lagrange, Pontecoulant and Leverrier, their results being essentially accordant in this respect, shows with a great degree of probability that our solar system is a stable one, the law of gravitation alone being considered; although to speak with certainty on this point an analytical solution is to be desired. But when it is required to compute for very remote epochs the values of the elements of the orbits the co-efficients g which in equations (1) are multiplied by the time must be carefully considered. These coefficients depend on the assumed masses of the planets, and are generally determined by neglecting terms of the third order. The most complete investigation of this subject is that given by Leverrier in the Connaissance des Tems for 1843 and 1844, and reproduced with some additions in the memoirs of the Paris Observatory for 1856. In this work, which is a masterpiece of astronomical calculation, Leverrier shows that terms of the third order may produce corrections of the values of g amounting to three or four tenths of a second. Probable uncertainties in the assumed values of the masses of the planets may give rise to errors of nearly two tenths of a second. Hence, if we consider the forms of the general integrals (1), we shall readily see that for very remote epochs, distant by millions of years, our calculations must be very untrustworthy; since when the time is great the errors in the values of g may completely change the character of the circular functions.

For a satisfactory solution of this problem, certainly one of the most interesting in astronomy, our knowledge of the masses of the planets must be greatly advanced. In the case of the interior planets it appears that we must wait patiently until the theory of their own motions, or the motion of some one of the periodical comets, shall furnish the data for an exact determination of their masses. The masses of Mars and Jupiter will in time be very accurately known from the theories of some of the minor planets. But in the case of Saturn, Uranus and Neptune it appears to me that the instrumental means are already at hand for making an accurate determination of their masses, and a more complete investigation of the theories of their satellites. When the novel and entertaining observations with the spectroscope have received their natural abatement and been assigned their proper place, it is to be hoped that some of the powerful telescopes recently constructed may be devoted to this class of observations, where a rich and an ample field awaits the skillful observer. One could not wish a better example than the beautiful work of Bessel on the satellites of Jupiter.

August 2, 1870.

ART. XL.—*Farmer's Theorem discussed;* by FRED. E. STIMPSON.

TOWARD the close of Prof. Silliman's paper, "On the relation between the intensity of light produced from the combustion of illuminating gas and the volume of gas consumed" (this Jour., xlix, 17), is the following:—"A comparison of the foregoing results will show that the coincidences, with the requirements of the theorem of Farmer are, within the limits assigned, too numerous, and too closely accordant, to be considered as otherwise than pointing clearly to its general truth."

What I propose to examine now is, whether the data given in the paper referred to, do warrant the conclusion reached.

According to the data given for the first experiment, two lights were made equal, so that the disk of a Bunsen Photometer stood midway between the flames, and the consumptions were found to be 3·66 feet per hour.

"The screen was then moved upon the bar to a point just four times as far from one flame as it was from the other, i. e., the bar being 100 inches, the screen stood at 80, i. e., as 1:4. The light from the distant burner was then increased, until the disk again showed as an equality of illumination. On reading the rate of the gas consumed by the two burners respectively. one gave 3·66 cubic feet and the other 7·32 cubic feet."

Now from this it will be seen that the ratio of consumption was as 1 to 2, but the ratio of the lights being as the squares of the distances from the disk, becomes $200^2 : 80^2$ or 1 : 16, and the result on the paper should stand,

$$3{\cdot}66^4 : 7{\cdot}32^4 = 1 : 16 \text{ and not}$$
$$3{\cdot}66^2 : 7{\cdot}32^2 = 1 : 4.$$

In other words, the lights were proportional to the fourth powers of the consumptions and not to the squares.*

In experiments 2, 3 and 4, the ratio of the squares of consumption is nearer to the ratio of the lights than the simple ratio of the consumptions, but in experiment 2, the ratio of the 2·67 powers of the consumption almost exactly expresses the ratio of the lights. In experiment 3 the ratio of the squares is too small, and in experiment 4 it is too great; while in experiment 5, the simple ratio is certainly much nearer than the ratio of the squares.

The following table will, I think, suffice to show these various relations:

No. Exp't.	Rate of consumption per hour.	Intensity found by experiment.	Intensity by old law.	Intensity by law of the squares.	Intensity by the 2·67 power of consumption.	Difference between calculated and observed results.		
						Old law.	Law of squares.	2·67 power.
2	3·30	1·	1·	1·	1·	0	0	0
	4·35	2·1	1·32	1·73	2·098	−0·78	−0·37	−0·002
	5·136	3·2	1·55	2·42	3·222	−1·65	−0.78	+0·022
	5·556	4·0	1·68	2·83	3·995	−2·32	−1·17	−0·005
3	3·72	1·	1·	1·	....	0	0	....
	4·884	2·	1·312	1·72	....	−0·628	−0·28	....
	6·000	3·	1·61	2·60	....	−1·39	−0·40	....
	7·218	4·	1·94	3·76	....	−2·06	−0·24	....
4	4·5	1·	1·	1·	....	0	0	....
	9·519	4·	2·114	4·46	....	− 1·886	+0·46	....
5	5·16	1·85	1·85	1·85	....	0	0	....
	10·06	4·00	3·70	5·72	....	− 0·30	+1·72	....

In experiment 6, the reductions have been made by applying the old rule to the correction for the candle in both cases; the correction by Farmer's Theorem being applied only to the consumption of the gas, and so applied gives the most concordant results, the difference shown by the old rule however, (1·34 candles) is no greater than might have occurred between two observations, even if the consumption in both cases had been

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[* Since the above was written, I have been informed that the error is not in the proportion of ("1 to 4") but in the statement of the position of the disk upon the bar. It should read *twice* as far from one flame as the other, i. e., the screen stood at 66⅔, and thus the experiment supports the theorem.]

equal, i. e., five feet per hour. But this experiment alone certainly does not warrant the conclusion reached, viz:—"That this theorem applies with equal force to the weight of sperm consumed by the standard candle as to the volume of the gas burned in equal times," because the correction has *not* been applied to the candle either here or in any other observation given in Prof. Silliman's paper.

The confirmation obtained by experimenting upon Peytona, Albert and Wollongong gas depends upon the assumption that the true candle power of a rich gas can be obtained by mixing it in definite proportions, with another gas whose illuminating power is known, and deducing from the observed candle power of the mixture, the candle power of the rich gas; until this assumption has been proved to be correct, it is of course useless in establishing Farmer's Theorem.

The next proof offered is drawn from a tabular statement in Sugg's Gas Manipulation. Prof. Silliman says, "By this statement the burner in question produced from five cubic feet of gas exactly 15 (14?) candle power, but when reduced to 4·5 cubic feet consumption the candle when 'corrected to the standard quality of gas by proportion' was only 11·93 candles. The values of the 'correction' referred to can only be conjectured, but assuming that the observation made the uncorrected rendering 11·32 candles (a very probable quantity), we find that the law of the squares of consumption then makes the ratio as follows:—$4{\cdot}5^2 : 11{\cdot}32 = 5^2 \cdot 14$." The assumption here made is not at all necessary because we can find the exact value of the 'correction' for the gas by reversing the proportion used, thus:—$5 : 11{\cdot}93 = 4{\cdot}5 : 10{\cdot}73$, and now applying Farmer's Theorem the ratio becomes $4{\cdot}5^2 : 10{\cdot}73 = 5^2 : 13{\cdot}24$ only. The relation can be exactly expressed by the ratio $4{\cdot}5^{\frac{5}{2}} : 10{\cdot}73 = 5^{\frac{5}{2}} : 13{\cdot}997 = 14$.

The last proof offered is drawn from Audoin and Berard's experiments. But after several fruitless efforts to obtain the results, as given under the head of "Intensities by law of the squares of consumption," I am forced to conclude that these figures are incorrect through some inaccuracy in applying that law. The two tables referred to may be found in the author's original memoir on pages 439 and 441 of Annales de Physique et de Chimie, vol. lxv, 1862. In these experiments they compared the burning of two batwing burners at different rates of consumption, with a "Bengel Argand" whose rate of consumption was nearly constant. The first three columns of the table below give their experimental data.

TABLE I. *Burner of the fifth series—slit $\frac{1}{32}$ inch wide.*

Consumption of the Batwing under trial.	Consumption of the Bengel Argand.	Comparative intensities. The Bengel burner being 100	Intensties by old formula or direct ratio of consumption.	Difference between calculated and observed results.	Intensities for law of the squares of consumption.	Difference between calculated and observed results.
Liters per hour.	Liters per hour.					
			l'		l''	
211	100	120	99·38	−0·62	82·31	− 17·69
189	96	110	97·64	−2·36	86·38	− 13·62
180	103	100	100·00	0·00	100·00	0·0
156	104	90	104·85	+4·85	122·17	+ 22·17
142	104	80	102·39	+2·39	131·06	+ 31·06
124	101	70	99·65	−0·35	141·84	+ 41·84
102	101	60	103·82	+3·82	179·68	+ 79·68
88	102	50	99·30	−0·70	197·19	+ 97·19
68	100	40	102·79	+2·79	264·20	+164·20
57	102	30	90·73	−9·27	293·40	+193·40

TABLE II. *Burner of the same series—slit $\frac{1}{63}$ inch wide.*

Consumption of the Batwing under trial.	Consumption of the Bengel Argand.	Comparative intensities. The Bengel burner being 100.	Intensities by old formula or direct ratio of consumption.	Difference between calculated and observed results.	Intensities for law of the squares of consumption.	Difference between calculated and observed results.
Liters per hour.	Liters per hour.					
264	106	200	100·4	+ 0·4	50·3	− 49·7
236	105	180	100·1	+ 0·1	55·6	− 44·4
208	105	160	100·9	+ 0·9	63·5	− 36·5
182	105	140	100·9	+ 0·9	72·8	− 27·2
152	104	120	102·6	+ 2·6	87·7	− 12·3
130	104	100	100·0	0·0	100·0	0 0
112	104	80	92·8	− 7·2	107·8	+ 7·8
90	104	60	86·6	−13·4	125·1	+ 25·1
75	104	50	86·6	−13·4	150·2	+ 50·2
66	104	40	78·8	−22·2	155·2	+ 55·2
43	104	20	60·5	−39·5	182·8	+ 82·8
28	104	10	46·4	−53·6	215·5	+115·5

From the first table it will be seen that the two burners gave equal light (intensity 100), when the batwing consumed 180 liters per hour, and the standard 103 liters. Correcting the first line of this table for these consumptions, by the old rule

we have for the batwing $211:180=120:l$
and for the standard $103:100=\ l:l'$;
the total correction for both
will be therefore $211\times 103:180\times 100=120\times l:l\times l'$;
the l cancels out and leaves
the proportion $211\times 103:180\times 100=120:l'$.
The second line of this table
becomes $189\times 103:180\times\ 96=110:l$
and so on.

Now applying Farmer's Theorem, the proportions become

$$(211)^2\times(103)^2:(180)^2\times(100)^2=120:l''$$
$$(189)^2\times(103)^2:(180)^2\times(\ 96)^2=110:l''.$$

The value of l' and l'' from these proportions will be found in the 4th and 6th columns of the above table.

It will be seen that in the second table the burners gave equal light for consumptions of 130 and 104; these numbers have been used therefore in the corrections for that table.

The experiments show that the last term of the proportions should be 100. The tables show that by the old formula, this term becomes about 100 for every experiment of the first table, and for the first seven experiments of the second table, the greatest difference being 7·2; and for the remaining five experiments, the old formula gives the best approximate results except for the last one, and here the old law gives a result which must be *multiplied* by 2·15 to make it correct, and Farmer's Theorem gives a result that must be *divided* by 2·15 to make it correct.

From a perusal of these various results, I am led to disagree with Prof. Silliman, and say that 'Farmer's Theorem' is not proven, and that the law of the squares does *not in general* give any closer results than the old law of the direct ratio; though I entirely and heartily concur with him in the conclusion, *that every photometric observer should recognize the importance of bringing the consumption of gas and sperm to the agreed standard*, when attempting to give the true candle power of any gas.

It is much to be desired that experimenters should turn their attention to the matter of the relation of consumption of gas to illuminating power, and I sincerely hope that Mr. Farmer will not let the matter rest here, but will make and publish further observations upon the same subject.

Messrs. Audoin and Berard in their valuable experiments, to determine the best burner for the city of Paris, proved that for every consumption of gas there is a burner which is best suited for that consumption. Now by the proper selection of a burner for small consumptions, some simple relation may yet be found between consumption and illuminating power.

In the above, reference has been made only to experiments detailed in Prof. Silliman's communication. I have, however, made a number of experiments myself upon the same subject, besides collating the results of some sixty or more independent observations, which have been published during the last fifteen years, and the results are curious, instructive and unexpected. There are a few among them to which Farmer's Theorem might be applied, quite a number to which the old law will apply; though many of them require a *modification* of the old law.

I hope in due time to prepare a paper giving some of these results.

April, 1870.

ART. XLI.—*Note on Mr. Stimpson's Paper on Farmer's Theorem;* by B. SILLIMAN.

MR. STIMPSON'S criticism on the first experiment of my paper was induced by an obvious numerical error in the statement of the data, which Mr. S. has himself corrected in a foot note on p. 373. The experiment properly stated exactly sustains the theorem.

I admit that in Experiment No. 2, the exponent comes nearer to the third power than to the square, being 2·689, 2·638 and 2·669; in the three cases average 2·661. But it also indicates that the illuminating power of the standard 3·30 cubic feet was very imperfect. The experiments, however, appear to me to demonstrate clearly the radical inaccuracy of the old rule for photometric calculations, and that some ratio near to or greater than the square gives often more trustworthy results than the old rule.

In Experiment No. 3, the exponents are respectively 2·552, 2·297 and 2·092, coming in the last very near the square. It shows that 4·88 cubic feet consumption gave the best results, and also that 3·72 and 7·219 cubic feet consumption gave very nearly the same degree of intensity of combustion.

In Experiment No. 4, the exponent 1·85 power for a consumption of 9·519 consumption, shows an imperfect combustion of the fish-tail burner employed.

In Experiment No. 5, the experimental conditions were wholly unfavorable to accuracy, owing to an inequality of pressure unavoidable in the experimental method adopted, there being one inch pressure on the 10·06 c. f. consumption, and only half an inch on the 5·16 c. f. It is well known to all photometric observers how important a low pressure and an equal pressure is to the results obtained. It was hardly fair to Mr. Farmer to have quoted this trial, but I was desirous of exhibiting the entire range of observation, bad as well as good.

As in Experiment No. 6, the consumption of sperm was in the two tests very nearly uniform; the difference would be very trifling if the correction had been applied. Nor is it by any means so certain that Farmer's Theorem applies to the candle as I supposed when the remarks quoted by Mr. Stimpson were made, since, if a candle burns much over 120 grains it smokes ("tails off"), and then there is an end of all accuracy, and the observation must be rejected; since there is an imperfect combustion giving an increase in consumption but not in intensity.

Mr. Stimpson rejects all the data given by me which are founded upon the determination of the intensity of a rich gas by the 'method of mixtures,' because, as he says, the accuracy of this method has not yet been demonstrated by experiment. But this objection ceases to have force now that experiments, made by Mr. Farmer and myself lately, prove that this method is worthy of confidence. Inasmuch as the most important case for the use of Farmer's Theorem is that of gas too rich to be burned in the standard burner on a basis of 5 cubic feet per hour, it is striking off by all means the most valuable portion of my contribution to photometrical methods if it could be shown that the 'method of mixtures' was untrustworthy. I am, therefore, glad of this occasion to reiterate my confidence in this method, and to refer the reader curious in such matters to a brief paper of mine upon this subject, which will be found on page 379.

I care very little whether the results of experiment shall show, when they are sufficiently accumulated, that the ratio of consumption of gas is to the intensity produced as the squares of consumption in a given case, or in some other ratio greater than a simple ratio. I have desired chiefly to call attention to the general untrustworthiness of all photometric observations which are made with volumes of gas much less than the normal standard adopted, when these results are calculated on the simple ratio of the consumptions. It is only by the accumulation of carefully conducted experimental data that a law can be established, and these data are now pretty rapidly accumulating.*

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* In the proceedings of the American Association for Advancement of Science, Salem meeting, I presented the matter referring to *Sugg's manipulation* in a different form, giving the results of the observations with his Argand burner in a tabular form as follows:

No. obs.	Cubic feet.	Observations corrected by old rule candles.	Differences.	Uncorrected observations.	Observations corrected by Farmer's Theorem.	Differences.
1.	5·	14·00		14·00	14·00	
2.	4·9	13·78	0·22	13·504	14·060	0·556
3.	4·8	13·74	0·26	13·190	14·312	1·132
4.	4·7	13·30	0·70	12·502	14·148	1·646
5.	4·6	13·04	0·96	11·996	14·191	2·105
6.	4·5	11·93	2·07	10·738	13·255	2·517

The mean candle power of the 6th column is 13·99 candles; difference 0·01 candle.

The following will show the fractional power required to bring the uncorrected observations (column 5) to 14·00 candles.

2. $4{\cdot}9^{1{\cdot}83}$: 13·504 : : $5^{1{\cdot}83}$: 14·00 candles.
3. $4{\cdot}8^{1{\cdot}47}$: 13· 90 : : $5^{1{\cdot}47}$: 14·00 "
4. $4{\cdot}7^{1{\cdot}85}$: 12·502 : : $5^{1{\cdot}85}$: 14·00 "
5. $4{\cdot}6^{1{\cdot}86}$: 11·996 : : $5^{1{\cdot}86}$: 14·00 "
6. $4{\cdot}5^{2{\cdot}52}$: 10·737 : : $5^{2{\cdot}52}$: 14·00 "

This table shows that the 3rd and 6th tests have not been good ones, while the 2d, 3d, 4th and 5th tests fall a little below the square or 2d power, and the 6th test is considerably more.

New Haven, July, 1870.

ART. XLII.—*On the Determination of the Photometric Power of a rich gas by dilution with a poor gas of known value: the "method of mixtures;"* by B. SILLIMAN.

IN a paper on 'Farmer's Theorem,'* I have given several examples of the method of determining the intensity or photometric power of a rich gas by diluting it with several times its own volume of a poorer gas of known intensity, and then calculating its value from the increment of intensity. Having demonstrated in the paper before mentioned the worthlessness of all determinations of the intensity of gases of high illuminating power made by burning them in volumes less than five cubic feet, and then calculating their intensity by the rule of three up to that volume, I have shown how much more exact results were obtained when the results were calculated upon the theorem of Mr. Farmer; this greater exactness being predicated largely upon the confirmation drawn from parallel observations upon the same gases when measured by the method of mixtures. The results thus obtained having, however, been questioned by Mr. Stimpson,† on the ground that the method itself had not been experimentally demonstrated, I have undertaken lately, in connection with Mr. Farmer, to make some experiments calculated to test its accuracy.

The results which go to support the accuracy of the method were obtained with the use of a new photometric apparatus, constructed for the Manhattan Gas Co., under my direction, by Sugg of London, and which was designed to embrace all the best approved features which recent experience has indicated in photometry. A discussion of these details would be out of place in this connection. Before detailing our results, it will be proper to present the method of determination of intensity for gas of high illuminating power as practiced by Mr. Farmer at the Manhattan Gas Works in New York, and which I have called *the method of mixtures.*

To find the candle power of a gas having, for example, an intensity greater than 20 candles, mix the rich gas of unknown power with a poorer gas of known power in such proportions that the intensity of the mixture shall not be greater than 20 candles power, when consumed at the agreed rate of not over five cubic feet per hour. Then to compute the candle power (intensity) of the rich gas,—

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* This Journal, II, xlix, 17; also Proceedings of American Association for Advancement of Science, Salem meeting, 1869, p. 149.

† See page 272, this volume.

Let a=the percentage or volume of gas of low intensity.
" b=the intensity in candles of the gas of low intensity.
" c=the percentage or volume of rich gas used in the mixture.
" d=the intensity in candles of the mixture as observed.
" x=the intensity in candles of the rich gas required.

Hence, $\frac{ab+cx}{a+c}=d$ $\qquad$ $\frac{(d-b)a}{c}+d=x$

$$ab+cx=ad+cd$$
$$cx=ad+cd+ab.$$

And this expression is stated arithmetically in the following

Rule:—Subtract the intensity of the poor gas from the intensity of the mixture; multiply the remainder by the volume of poor gas; divide the product by the volume of rich gas; add to the quotient the intensity of the mixed gas, and the sum is the intensity of the rich gas sought.

Now when we reflect that in any given illuminating gas we have always a certain volume of non-luminous combustible gas, as the substratum to which is added, according to its source and mode of treatment, a variable volume of illuminants, it is no unwarranted assumption to say that the illuminants (chiefly olefines) are *diluted* by the non-illuminants. It is agreed, on all hands, that hydrogen, marsh gas and carbonic oxyd, which together form the mass of the non-luminous substratum of all illuminating gas, have of themselves, when pure, no luminosity, and when burned at ordinary atmospheric pressures and corresponding temperatures, that they may in fact be called, as to luminosity, neutral. It can hardly be questioned that the intensity which these neutral gases may assume in a given mixture must depend, under the same ordinary conditions before mentioned, upon the amount and kind of olefines they may take up in the destructive distillation of the coal or other hydrocarbons used in making gas. If these assumptions are true, we ought to be able to demonstrate them by experiment, by commingling certain volumes of a gas of known intensity with a neutral gas of no intensity. These experimental conditions would be met by using carbonic oxyd as the neutral gas, or better still, the mixture of carbonic oxyd and hydrogen resulting from the decomposition of vapor of water at a high temperature in contact with highly ignited carbon, as in the hydrocarbon gas process. But in default of any convenient means of obtaining these gases, we had recourse to hydrogen gas evolved by the action of diluted sulphuric acid upon zinc in a large self-regulating generator of hydrogen. It is well known that hydrogen thus made is not absolutely non-luminous, but it is sufficiently so for photometric purposes. The avidity of hydrogen for all the olefines, however, renders it difficult to obtain entirely satisfactory results with the use of this gas, provided it is passed through gas pipes and holders which have been previously used for the transmission of coal gas, since however carefully one may rinse out these tubes by hydrogen, there may yet cling, probably, some small trace of the condensed illuminants to the walls of the tubes, which imparts a trifling intensity to the hydrogen passing through them.

To obtain a supply of rich gas of uniform intensity, 100 lbs. of Albertite were coked until 810 cubic feet of gas had been taken from it of a density of ·498. This gas was purified, collected and preserved in a separate gas holder. Its intensity was determined,

1st. By Farmer's Theorem = 30·49 candles.
2d. " mixing with poor gas = 30·95 "
3d. " simple ratio = 26·20 "

In determining the intensity of this gas by the method of mixtures, 20 per cent by volume of Albert gas was mingled with 80 per cent of 10·6 candle gas obtained from a poor coal. The mixture had an intensity of 14·67 candles. Hence,

$$(14{\cdot}67-10{\cdot}60)\times 80\div 20+14{\cdot}67=30{\cdot}95 \text{ candles}$$

for the intensity of the Albert Gas by the method of mixtures.

1st Experiment.

Taken 75 volumes of coal gas =14·75 candles, obs'd.
" 25 " hydrogen = ...
" 100 " mixture =10·22 " "
If hydrogen=0 this mixture should have given 11·06 " calc.
Hence there is an error of observation of ·84 of a candle, which makes the value of the hydrogen apparently below zero.

2d Experiment.

Taken 75 volumes of coal gas =14·25 candles, obs'd.
" 25 " hydrogen = ...
" 100 " mixture =10·35 " "
If hydrogen=0 this mixture should have given 10·69 " calc.
Hence there is an error of observation = ·25 " which again makes the value of the hydrogen, apparently a little below zero.

The want of sufficient storage room for hydrogen and the necessity of many repetitions, and much care in manipulation to avoid errors of quantity in the synthesis of mixtures make experiments of this sort tedious, and my other avocations have prevented my multiplying them as much as is desirable. I think, however, that most photometric observers will agree with me that it is safe to conclude, from these two experiments, that the action of hydrogen in gaseous mixtures is simply that of a diluant. We might make a thousand observations by the means now at command, and not obtain one with an exact 0 for hydrogen. A very trifling error in the admeasurement greatly influences the result.

We may therefore safely conclude, as it appears to me—

1st. That in all illuminating gas we have a certain substratum of non-luminous gas, holding in solution a variable volume of luminous gas (olefines).

2d. That when a gas is too rich in illuminants to permit of accurate photometric admeasurement by the usual standards of intensity, it may be diluted with a poor gas of known value and volume to such a standard as is consistent with the accurate employment of the usual photometric apparatus, its true value being then calculated from the known values employed.

P. S. I find in the manuscript records of the Manhattan Gas Company, mention of four experiments made many years since by Mr. Schultz, chemist of that company, in which he mixed 5 per cent and 10 per cent of hydrogen with gas of very high illuminating power. The results are less satisfactory than they would have been, had the volume of hydrogen employed been much larger and the intensity of the coal gas not over 15 candles. Moreover at that time the means of admeasurement at the command of the observer in the laboratory of the Manhattan Company of small volumes of gas were much less exact than they now are. The results obtained are as follows:

1*st Experiment.*

Taken	90	volumes of coal gas	=22·53	candles,	observed.
“	10	“ hydrogen	=....		
“	100	“ mixture	=19·11	“	“
If the hydrogen is taken as 0		“	=20·27	“	calculated.
Showing apparent error of			=— 1·11	“	“

2*d Experiment.*

Taken	95	volumes of coal gas	=22·53	candles,	observed.
“	5	“ hydrogen	=		
“	100	“ mixture	=20·35	“	“
If the hydrogen is taken as 0		“	=21·39	“	calculated.
Apparent error			=—1·04	“	“

3*d Experiment.*

Taken	90	volumes coal gas	=20·83	candles,	observed.
“	10	“ hydrogen	=		
“	100	“ mixture	=19·44	“	“
If hydrogen is taken= 0		“	=18·29	“	calculated.
Apparent error,			+ ·15	“	“

4th *Experiment.*

Taken	95	volumes of	coal gas	=20·83	candles,	observed.
"	5	"	hydrogen	= ····		
"	100	"	mixture	=20·52	"	"
If hydrogen is taken	=0	"		=19·79	"	calculated.
Apparent error,				+ ·73	"	"

New Haven, Sept. 1870.

ART. XLIII.—*Address of Thomas Henry Huxley, at the meeting of the British Association at Liverpool, on the* 14*th of Sept.*, 1870.[*]

It has long been the custom for the newly installed President of the British Association for the Advancement of Science to take advantage of the elevation of the position in which the suffrages of his colleagues had, for the time, placed him, and, casting his eyes around the horizon of the scientific world, to report to them what could be seen from his watch-tower; in what directions the multitudinous divisions of the noble army of the improvers of natural knowledge were marching; what important strongholds of the great enemy of us all, ignorance, had been recently captured; and, also, with due impartiality, to mark where the advanced posts of science had been driven in, or a long-continued siege had made no progress.

I propose to endeavor to follow this ancient precedent, in a manner suited to the limitations of my knowledge and of my capacity. I shall not presume to attempt a panoramic survey of the world of science, nor even to give a sketch of what is doing in the one great province of biology, with some portions of which my ordinary occupations render me familiar. But I shall endeavor to put before you the history of the rise and progress of a single biological doctrine; and I shall try to give some notion of the fruits, both intellectual and practical, which we owe, directly or indirectly, to the working out, by seven generations of patient and laborious investigators, of the thought which arose, more than two centuries ago, in the mind of a sagacious and observant Italian naturalist.

It is a matter of every-day experience that it is difficult to prevent many articles of food from becoming covered with mould; that fruit, sound enough to all appearance, often contains grubs at the core; that meat, left to itself in the air, is apt to putrefy and swarm with maggots. Even ordinary water, if allowed to stand in an open vessel sooner or later becomes turbid and full of living matter.

The philosophers of antiquity, interrogated as to the cause of these phenomena, were provided with a ready and a plausible answer. It did not enter their minds even to doubt that these low forms of life were generated in the matters in which they made their appearance. Lucretius, who had drunk deeper of the scientific spirit than any poet of ancient or modern times except Goethe, intends to speak as a philosopher, rather than as a poet, when he writes that "with good reason the earth has gotten the name of mother, since all things are produced out of the earth. And many living creatures, even now, spring out of the earth, taking form by the rains and the heat of the sun." The axiom of ancient science, "that the corruption of one thing is the birth of another," had its popular embodiment in the notion that a seed dies before the young plant springs from it; a belief so wide spread and so fixed, that Saint Paul appeals to it in one of the most splendid outbursts of his fervid eloquence:—"Thou fool, that which thou sowest is not quickened, except it die." *

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[*] From "Nature," of Sept. 15.

The proposition that life may, and does, proceed from that which has no life, then, was held alike by the philosophers, the poets, and the people, of the most enlightened nations, eighteen hundred years ago; and it remained the accepted doctrine of learned and unlearned Europe, through the middle ages, down even to the seventeenth century.

It is commonly counted among the many merits of our great countryman, Harvey, that he was the first to declare the opposition of fact to venerable authority in this, as in other matters; but I can discover no justification for this wide-spread notion. After careful search through the "Exercitationes de Generatione," the most that appears clear to me is, that Harvey believed all animals and plants to spring from what he terms a "*primordium vegetale*," a phrase which may nowadays be rendered "a vegetative germ;" and this, he says, is "*oviforme*," or "egg-like;" not, he is careful to add, that it necessarily has the shape of an egg, but because it has the constitution and nature of one. That this "*primordium oviforme*" must needs, in all cases, proceed from a living parent is nowhere expressly maintained by Harvey, though such an opinion may be thought to be implied in one or two passages; while, on the other hand, he does, more than once, use language which is consistent only with a full belief in spontaneous or equivocal generation. In fact, the main concern of Harvey's wonderful little treatise is not with generation, in the physiological sense, at all, but with development; and his great object is the establishment of the doctrine of epigenesis.

The first distinct enunciation of the hypothesis that all living matter has sprung from pre-existing living matter, came from a contemporary, though a junior, of Harvey, a native of that country fertile in men great in all departments of human activity, which was to intellectual Europe, in the sixteenth and seventeenth centuries, what Germany is in the nineteenth. It was in Italy, and from Italian teachers, that Harvey received the most important part of his scientific education. And it was a student trained in the same schools, Francesco Redi—a man of the widest knowledge and most versatile abilities, distinguished alike as scholar, poet, physician, and naturalist—who, just two hundred and two years ago, published his "Esperienza intorno alla Generazione degl' Insetti," and gave to the world the idea, the growth of which it is my purpose to trace. Redi's book went through five editions in twenty years; and the extreme simplicity of his experiments, and the clearness of his arguments gained for his views, and for their consequences, almost universal acceptance.

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* 1 Corinthians, xv, 36.

Redi did not trouble himself much with speculative considerations, but attacked particular cases of what was supposed to be "spontaneous generation" experimentally. Here are dead animals, or pieces of meat, says he; I expose them to the air in hot weather, and in a few days they swarm with maggots. You tell me that these are generated in the dead flesh; but if I put similar bodies, while quite fresh, into a jar, and tie some fine gauze over the top of the jar, not a maggot makes its appearance, while the dead substances, nevertheless, putrefy just in the same way as before. It is obvious, therefore, that the maggots are not generated by the corruption of the meat; and that the cause of their formation must be a something which is kept away by gauze. But gauze will not keep away aëriform bodies, or fluid. This something must, therefore, exist in the form of solid particles too big to get through the gauze. Nor is one long left in doubt what these solid particles are: for the blowflies, attracted by the odor of the meat, swarm round the vessel, and, urged by a powerful but in this case misleading instinct, lay eggs out of which maggots are immediately hatched upon the gauze. The conclusion, therefore, is unavoidable; the maggots are not generated by the meat, but the eggs which give rise to them are brought through the air by the flies.

These experiments seem almost childishly simple, and one wonders how it was that no one ever thought of them before. Simple as they are, however, they are worthy of the most careful study, for every piece of experimental work since done, in regard to this subject, has been shaped upon the model furnished by the Italian philosopher. As the results of his experiments were the same, however varied the nature of the materials he used, it is not wonderful that there arose in Redi's mind a presumption, that in all such cases of the seeming production of life from dead matter, the real explanation was the introduction of living germs from without into that dead matter. And thus the hypothesis that living matter always arises by the agency of pre-existing living matter, took definite shape; and had, henceforward, a right to be considered and a claim to be refuted, in each particular case, before the production of living matter in any other way could be admitted by careful reasoners. It will be necessary for me to refer to this hypothesis so frequently, that, to save circumlocution, I shall call it the hypothesis of *Biogenesis;* and I shall term the contrary doctrine—that living matter may be produced by not living matter—the hypothesis of *Abiogenesis.*

In the seventeenth century, as I have said, the latter was the dominant view, sanctioned alike by antiquity and by authority; and it is interesting to observe that Redi did not escape the customary tax upon a discoverer of having to defend himself against the charge of impugning the authority of the Scriptures; for his adversaries declared that the generation of bees from the carcase of a dead lion is affirmed, in the Book of Judges, to have been the origin of the famous riddle with which Samson perplexed the Philistines:—

> "Out of the eater came forth meat,
> And out of the strong came forth sweetness."

Against all odds, however, Redi, strong with the strength of demonstrable fact, did splendid battle for Biogenesis; but it is remarkable that he held the doctrine in a sense which, if he had lived in these times, would have infallibly caused him to be classed among the defenders of "spontaneous generation." "Omne vivum ex vivo," "no life without antecedent life," aphoristically sums up Redi's doctrine; but he went no further. It is most remarkable evidence of the philosophic caution and impartiality of his mind, that although he had speculatively anticipated the manner in which grubs really are deposited in fruits and in the galls of plants, he deliberately admits that the evidence is insufficient to bear him out; and he therefore prefers the supposition that they are generated by a modification of the living substance of the plants themselves. Indeed, he regards these vegetable growths as organs, by means of which the plant gives rise to an animal, and looks upon this production of specific animals as the final cause of the galls and of at any rate some fruit. And he proposes to explain the occurrence of parasites within the animal body in the same way.

It is of great importance to apprehend Redi's position rightly; for the lines of thought he laid down for us are those upon which naturalists have been working ever since. Clearly, he held *Biogenesis* as against *Abiogenesis;* and I shall immediately proceed, in the first place, to inquire how far subsequent investigation has borne him out in so doing.

But Redi also thought that there were two modes of Biogenesis. By the one method, which is that of common and ordinary occurrence, the living parent gives rise to offspring which passes through the same cycle of changes as itself—like gives rise to like; and this has been termed *Homogenesis.* By the other mode the living parent was supposed to give rise to offspring which passed through a totally different series of states from those exhibited by the parent, and did not return into the cycle of the parent; this is what ought to be called *Heterogenesis*, the offspring being altogether, and permanently unlike the parent. The term Heterogenesis, however, has unfortunately been used in a different sense, and M. Milne-Edwards has therefore substituted for it *Xenogenesis*, which means the generation of something foreign. After discussing Redi's hypothesis of universal Biogenesis, then, I shall go on to ask how far the growth of science justifies his other hypothesis of Xenogenesis.

The progress of the hypothesis of Biogenesis was triumphant and unchecked for nearly a century. The application of the microscope to anatomy in the hands of Grew, Leeuwenhoek, Swammerdam, Lyonet, Vallisnieri, Reaumur, and other illustrious investigators of nature of that day, displayed such a complexity of organization in the lowest and minutest forms, and everywhere revealed such a prodigality of provision for their multiplication by germs of one sort or another, that the hypothesis of Abiogenesis began to appear not only untrue, but absurd; and, in the middle of the eighteenth century, when Needham and Buffon took up the question, it was almost universally discredited.

But the skill of the microscope-makers of the eighteenth century soon reached its limit. A microscope magnifying 400 diameters was a *chef d'œuvre* of the opticians of that day; and at the same time, by no means trustworthy. But a magnifying power of 400 diameters, even when definition reaches the exquisite perfection of our modern achromatic lenses, hardly suffices for the mere discernment of the smallest forms of life. A speck, only $\frac{1}{25}$th of an inch in diameter, has, at 10 inches from the eye, the same apparent size as an object $\frac{1}{10000}$th of an inch in diameter, when magnified 400 times; but forms of living matter abound, the diameter of which is not more than $\frac{1}{40000}$th of an inch. A filtered infusion of hay, allowed to stand for two days, will swarm with living things, among which, any which reaches the diameter of a human red blood-corpuscle, or about $\frac{1}{3200}$th of an inch, is a giant. It is only by bearing these facts in mind, that we can deal fairly with the remarkable statements and speculations put forward by Buffon and Needham in the middle of the eighteenth century.

When a portion of any animal or vegetable body is infused in water, it gradually softens and disintegrates; and, as it does so, the water is found to swarm with minute active creatures, the so-called Infusorial Animalcules, none of which can be seen, except by the aid of the microscope; while a large proportion belong to the category of smallest things of which I have spoken, and which must have all looked like mere dots and lines under the ordinary microscopes of the eighteenth century.

Led by various theoretical considerations which I cannot now discuss, but which looked promising enough in the lights of that day, Buffon and Needham doubted the applicability of Redi's hypothesis to the infusorial animalcules, and Needham very properly endeavored to put the question to an experimental test. He said to himself, if these infusorial animalcules come from germs, their germs must exist either in the substance infused, or in the water in which the infusion is made, or in the superjacent air. Now the vitality of all germs is destroyed by heat. Therefore, if I boil the infusion, cork it up carefully, cementing the cork over with mastic, and then heat the whole vessel by heaping hot ashes over it, I must needs kill whatever germs are present. Consequently, if Redi's hypothesis hold good, when the fusion is taken away and allowed to cool, no animalcules ought to be developed in it; whereas, if the animalcules are not dependent on pre-existing germs, but are generated from the infused substance, they ought, by-and-by, to make their appearance. Needham found that, under the circumstances in which he made his experiments, animalcules always did arise in the infusions, when a sufficient time had elapsed to allow for their development.

In much of his work Needham was associated with Buffon, and the results of their experiments fitted in admirably with the great French naturalist's hypothesis of "organic molecules," according to which, life is the indefeasible property of certain indestructible molecules of matter, which exist in all living things, and have inherent activities by which they are distinguished from not living matter. Each individual living organism is formed by their temporary combination. They stand to it in the relation of the particles of water to a cascade, or a whirlpool; or to a mould, into which the water is poured. The form of the organism is thus determined by the reaction between external conditions and the inherent activities of the organic molecules of which it is composed; and, as the stoppage of the whirlpool destroys nothing but a form, and leaves the molecules of the water, with all their inherent activities intact, so what we call the death and putrefraction of an animal, or of a plant, is merely the breaking up of the form, or manner of association, of its constituent organic molecules, which are then set free as infusorial animalcules.

It will be perceived that this doctrine is by no means identical with *Abiogenesis*, with which it is often confounded. On this hypothesis, a piece of beef, or a handful of hay, is dead only in a limited sense. The beef is dead ox, and the hay is dead grass; but the "organic molecules" of the beef or the hay are not dead, but are ready to manifest their vitality as soon as the bovine or herbaceous shrouds in which they are imprisoned are rent by the macerating action of water. The hypothesis therefore must be classified under Xenogenesis, rather than under Abiogenesis. Such as it was, I think it will appear, to those who will be just enough to remember that it was propounded before the birth of modern chemistry, and of the modern optical arts, to be a most ingenious and suggestive speculation.

But the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers, was played, almost immediately, for the benefit of Buffon and Needham.

Once more, an Italian, the Abbé Spallanzani, a worthy successor and representative of Redi in his acuteness, his ingenuity, and his learning, subjected the experiments and the conclusions of Needham to a searching criticism. It might be true that Needham's experiments yielded results such as he had described, but did they bear out his arguments? Was it not possible, in the first place, that he had not completely excluded the air by his corks and mastic? And was it not possible, in the second place, that he had not sufficiently heated his infusions and the superjacent air? Spallanzani joined issue with the English naturalist on both these pleas, and he showed that if, in the first place, the glass vessels in which the infusions were contained were hermetically sealed by fusing their necks, and if, in the second place, they were exposed to the temperature of boiling water for three-quarters of an hour,* no animalcules ever made their appearance within them. It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham. But we all too often forget that it is one thing to refute a proposition, and another to prove the truth of a doctrine which, implicitly or explicitly, contradicts that proposition, and the advance of science soon showed that though Needham might be quite wrong, it did not follow that Spallanzani was quite right.

Modern chemistry, the birth of the latter half of the eighteenth century, grew apace, and soon found herself face to face with the great problems which biology had vainly tried to attack without her help. The discovery of oxygen led to the laying of the foundations of a scientific theory of respiration, and to an examination of the marvellous interactions of organic substances with oxygen. The presence of free oxygen appeared to be one of the conditions of the existence of life, and of those singular changes in organic matters which are known as fermentation and putrefaction. The question of the generation of the infusory animalcules thus passed into a new phase. For what might not have happened to the organic matter of the infusions, or to the oxygen of the air, in Spallanzani's experiments? What security was there that the development of life which ought to have taken place had not been checked or prevented by these changes?

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* See Spallanzani, "Opere," vi, pp. 42 and 61.

The battle had to be fought again. It was needful to repeat the experiments under conditions which would make sure that neither the oxygen of the air, nor the composition of the organic matter, was altered in such a manner as to interfere with the existence of life.

Schulze and Schwann took up the question from this point of view in 1836 and 1837. The passage of air through red-hot glass tubes, or through strong sulphuric acid, does not alter the proportion of its oxygen, while it must needs arrest or destroy any organic matter which may be contained in the air. These experimenters, therefore, contrived arrangements by which the only air which should come into contact with a boiled infusion should be such as had either passed through red-hot tubes or through strong sulphuric acid. The result which they obtained was that an infusion so treated developed no living things, while if the same infusion was afterwards exposed to the air such things appeared rapidly and abundantly. The accuracy of these experiments has been alternately denied and affirmed. Supposing them to be accepted, however, all that they really proved was that the treatment to which the air was subjected destroyed, *something* that was essential to the development of life in the infusion. This "something" might be gaseous, fluid, or solid; that it consisted of germs remained only an hypothesis of greater or less probability.

Contemporaneously with these investigations a remarkable discovery was made by Cagniard de la Tour. He found that common yeast is composed of a vast accumulation of minute plants. The fermentation of must or wort in the fabrication of wine and of beer is always accompanied by the rapid growth and multiplication of these *Torulæ*. Thus fermentation, in so far as it was accompanied by the development of microscopical organisms in enormous numbers, became assimilated to the decomposition of an infusion of ordinary animal or vegetable matter; and it was an obvious suggestion that the organisms were, in some way or other, the causes both of fermentation and of putrefaction. The chemists, with Berzelius and Liebig at their head, at first laughed this idea to scorn; but in 1843, a man then very young, who has since performed the unexampled feat of attaining to high eminence alike in Mathematics, Physics, and Physiology—I speak of the illustrious Helmholtz—reduced the matter to the test of experiment by a method alike elegant and conclusive. Helmholtz separated a putrefying or a fermenting liquid from one which was simply putrescible or fermentable by a membrane which allowed the fluids to pass through and become intermixed, but stopped the passage of solids. The result was, that while the putrescible or the fermentable liquids became impregnated with the results of the putrescence or fermentation which was going on on the other side of the membrane, they neither putrefied (in the ordinary way) nor fermented; nor were any of the organisms which abounded in the fermenting or putrefying liquid generated in them. Therefore the cause of the development of these organisms must lie in something which cannot pass through membranes; and as Helmholtz's investigations were long antecedent to Graham's researches upon colloids, his natural conclusion was that the agent thus intercepted must be a solid material. In point of fact, Helmholtz's experiments narrowed the issue to this: that which excites fermentation and putrefaction, and at the same time gives rise to living forms in a fermentable or putrescible fluid, is not a gas and is not a diffusible fluid; therefore it is either a colloid, or it is a matter divided into very minute solid particles.

The researches of Schroeder and Dusch in 1854, and of Schroeder alone, in 1859, cleared up this point by experiments which are simply refinements upon those of Redi. A lump of cotton-wool is, physically speaking, a pile of many thicknesses of a very fine gauze, the fineness of the meshes of which depends upon the closeness of the compression of the wool. Now, Schroeder and Dusch found, that, in the case of all the putrefiable materials which they used (except milk and yolk of egg), an infusion boiled, and then allowed to come into contact with no air but such as had been filtered through cotton-wool, neither putrefied nor fermented, nor developed living forms. It is hard to imagine what the fine sieve formed by the cotton-wool could have stopped except minute solid particles. Still the evidence was incomplete until it had been positively shown, first, that ordinary air does contain such particles; and, secondly, that filtration through cotton-wool arrests these particles and allows only physically pure air to pass. This demonstration has been furnished within the last year by the remarkable experiments of Professor Tyndall. It has been a common objection of Abiogenists that, if the doctrine of Biogeny is true, the air must be thick with germs; and they regard this as the height of absurdity. But Nature occassionally is exceedingly unreasonable, and Professor Tyndall has proved that this particular absurdity may nevertheless be a reality. He has demonstrated that ordinary air is no better than a sort of stir-about of excessively minute solid particles; that these particles are almost wholly destructible by heat; and that they are strained off, and the air rendered optically pure by being passed through cotton-wool.

But it remains yet in the order of logic, though not of history, to show that among these solid destructible particles there really do exist germs capable of giving rise to the development of living forms in suitable menstrua. This piece of work was done by M. Pasteur, in those beautiful researches which will ever render his name famous; and which, in spite of all attacks upon them, appear to me now, as they did seven years ago,* to be models of accurate experimentation and logical reasoning. He strained air through cotton-wool, and found, as Schroeder and Dusch had done, that it contained nothing competent to give rise to the development of life in fluids highly fitted for that purpose. But the important further links in the chain of evidence added by Pasteur are three. In the first place he subjected to microscopic examination the cotton-wool which had served as strainer, and found that sundry bodies clearly recognizable as germs, were among the solid particles strained off. Secondly, he proved that these germs were competent to give rise to living forms by simply sowing them in a solution fitted for their development. And, thirdly, he showed that the incapacity of air strained through cotton-wool to give rise to life, was not due to any occult change effected in constituents of the air by the wool, by proving that the cotton-wool might be dispensed with altogether, and perfectly free access left between the exterior air and that in the experimental flask. If the neck of the flask is drawn out into a tube and bent downward; and if, after the contained fluid has been carefully boiled, the tube is heated sufficiently to destroy any germs which may be present in the air which enters as the fluid cools, the apparatus may be left to itself for any time, and no life will appear in the fluid. The reason is plain. Although there is free communication between the atmosphere laden with germs and the germless air in the flask, contact between the two takes place only in the tube; and as the germs cannot fall upward, and there are no currents, they never reach the interior of the flask. But if the tube be broken short off where it proceeds from the flask, and free access be thus given to germs falling vertically out of the air, the fluid which has remained clear and desert for months, becomes, in a few days turbid and full of life.

These experiments have been repeated over and over again by independent observers with entire success; and there is one very simple mode of seeing the facts for oneself, which I may as well describe.

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* "Lectures to Working Men on the Causes of the Phenomena of Organic Nature." 1863.

Prepare a solution (much used by M. Pasteur, and often called "Pasteur's solution") composed of water with tartrate of ammonia, sugar, and yeast-ash dissolved therein.* Divide it into three portions in as many flasks; boil all three for a quarter of an hour; and, while the steam is passing out, stop the neck of one with a large plug of cotton-wool, so that this also may be thoroughly steamed. Now set the flasks aside to cool, and when their contents are cold, add to one of the open ones a drop of filtered infusion of hay which has stood for twenty-four hours, and is consequently full of the active and excessively minute organisms known as *Bacteria.* In a couple of days of ordinary warm weather the contents of this flask will be milky from the enormous multiplication of *Bacteria.* The other flask, open and exposed to the air, will, sooner or later, become milky with *Bacteria*, and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton-wool, will remain clear for an indefinite time. I have sought in vain for any explanation of these facts, except the obvious one, that the air contains germs competent to give rise to *Bacteria*, such as those with which the first solution has been knowingly and purposely inoculated, and to the mould-*Fungi.* And I have not yet been able to meet with any advocate of Abiogenesis who seriously maintains that the atoms of sugar, tartrate of ammonia, yeast-ash, and water, under no influence but that of free access of air and the ordinary temperature, rearrange themselves and give rise to the protoplasm of *Bacterium.* But the alternative is to admit that these *Bacteria* arise from germs in the air; and if they are thus propagated, the burden of proof that other like forms are generated in a different manner, must rest with the assertor of that proposition.

To sum up the effect of this long chain of evidence:—

It is demonstrable that a fluid eminently fit for the development of the lowest forms of life, but which contains neither germs, nor any protein compound, gives rise to living things in great abundance if it is exposed to ordinary air, while no such development takes place if the air with which it is in contact is mechanically freed from the solid particles which ordinarily float in it, and which may be made visible by appropriate means.

It is demonstrable that the great majority of these particles are destructible by heat, and that some of them are germs or living particles, capable of giving rise to the same forms of life as those which appear when the fluid is exposed to unpurified air.

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* Infusion of hay treated in the same way yields similar results; but as it contains organic matter, the argument which follows cannot be based upon it.

It is demonstrable that inoculation of the experimental fluid with a drop of liquid known to contain living particles, gives rise to the same phenomena as exposure to unpurified air.

And it is further certain that these living particles are so minute that the assumption of their suspension in ordinary air, presents not the slightest difficulty. On the contrary, considering their lightness and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.

Thus the evidence, direct and indirect, in favor of *Biogenesis* for all known forms of life must, I think, be admitted to be of great weight.

On the other side, the sole assertions worthy of attention are that hermetically sealed fluids, which have been exposed to great and long-continued heat, have sometimes exhibited living forms of low organization, when they have been opened.*

The first reply that suggests itself is the probability that there must be some error about these experiments, because they are performed on an enormous scale every day with quite contrary results. Meat, fruits, vegetables, the very materials of the most fermentable and putrescible infusions are preserved to the extent, I suppose I may say, of thousands of tons every year, by a method which is a mere application of Spallanzani's experiment. The matters to be preserved are well boiled in a tin case provided with a small hole, and this hole is soldered up when all the air in the case has been replaced by steam. By this method they may be kept for years without putrefying, fermenting, or getting mouldy. Now this is not because oxygen is excluded, inasmuch as it is now proved that free oxygen is not necessary for either fermentation or putrefaction. It is not because the tins are exhausted of air, for *Vibriones* and *Bacteria* live, as Pasteur has shown, without air or free oxygen. It is not because the boiled meats or vegetables are not putrescible or fermentable, as those who have had the misfortune to be in a ship supplied with unskillfully closed tins well know. What is it, therefore, but the exclusion of germs? I think that Abiogenists are bound to answer this question before they ask us to consider new experiments of precisely the same order.

And in the next place, if the results of the experiments I refer to are really trust-worthy, it by no means follows that Abiogenesis has taken place. The resistance of living matter to heat is known to vary within considerable limits, and to depend, to some extent, upon the chemical and physical qualities of the surrounding medium. But if, in the present state of science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful.

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* For a full account of the most recent series of experiments of this description, see Dr. H. C. Bastian's paper in NATURE, No. xxxv, p. 170; No. xxxvi, p. 193; and No. xxxvii, p. 219.

But though I cannot express this conviction of mine too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as Abiogenesis ever has taken place in the past or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together. All I feel justified in affirming is that I see no reason for believing that the feat has been performed yet.

And looking back through the prodigious vista of the past, I find no record of the commencement of life, and therefore I am devoid of any means of forming a definite conclusion as to the conditions of its appearance. Belief, in the scientific sense of the word, is a serious matter, and needs strong foundations. To say, therefore, in the admitted absence of evidence, that I have any belief as to the mode in which the existing forms of life have originated, would be using words in a wrong sense. But expectation is permissible where belief is not; and if it were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter. I should expect to see it appear under forms of great simplicity, endowed, like existing fungi, with the power of determining the formation of new protoplasm, from such matters as ammonium carbonates, oxalates and tartrates, alkaline and earthy phosphates, and water, without the aid of light. That is the expectation to which analogical reasoning leads me; but I beg you once more to recollect that I have no right to call my opinion anything but an act of philosophical faith.

So much for the history of the progress of Redi's great doctrine of Biogenesis, which appears to me, with the limitations I have expressed, to be victorious along the whole line at the present day.

As regards the second problem offered to us by Redi, whether Xenogenesis obtains, side by side with Homogenesis; whether, that is, there exist not only the ordinary living things, giving rise to offspring which run through the same cycle as themselves, but also others, producing offspring which are of a totally different character from themselves, the researches of two centuries have led to a different result. That the grubs found in galls are no product of the plants on which the galls grow, but are the result of the introduction of the eggs of insects into the substance of these plants, was made out by Vallisnieri, Reaumur, and others, before the end of the first half of the eighteenth century. The tapeworms, bladderworms, and flukes continued to be a stronghold of the advocates of Xenogenesis for a much longer period. Indeed, it is only within the last thirty years that the splendid patience of Von Siebold, Van Beneden, Leuckart, Küchenmeister, and other helminthologists, has succeeded in tracing every such parasite, often through the strangest wanderings and metamorphoses, to an egg derived from a parent, actually or potentially like itself; and the tendency of inquiries elsewhere has all been in the same direction. A plant may throw off bulbs, but these, sooner or later, give rise to seeds or spores, which develop into the original form. A polyp may give rise to Medusæ, or a pluteus to an Echinoderm, but the Medusa and the Echinoderm give rise to eggs which produce polyps or plutei, and they are therefore only stages in the cycle of life of the species.

But if we turn to pathology, it offers us some remarkable approximations to true Xenogenesis.

As I have already mentioned, it has been known since the time of Vallisnieri and of Reaumur, that galls in plants, and tumors in cattle, are caused by insects, which lay their eggs in those parts of the animal or vegetable frame of which these morbid structures are outgrowths. Again, it is a matter of familiar experience to everybody that mere pressure on the skin will give rise to a corn. Now the gall, the tumor, and the corn are parts of the living body, which have become, to a certain degree, independent and distinct organisms. Under the influence of certain external conditions, elements of the body, which should have developed in due subordination to its general plan, set up for themselves and apply the nourishment which they receive to their own purposes.

From such innocent productions as corns and warts, there are all gradations to the serious tumors which, by their mere size and the mechanical obstruction they cause, destroy the organism out of which they are developed; while, finally, in those terrible structures known as cancers, the abnormal growth has acquired powers of reproduction and multiplication, and is only morphologically distinguishable from the parasite worm, the life of which is neither more nor less closely bound up with that of the infested organism.

If there were a kind of diseased structure, the histological elements of which were capable of maintaining a separate and independent existence out of the body, it seems to me that the shadowy boundary between morbid growth and Xenogenesis would be effaced. And I am inclined to think that the progress of discovery has almost brought us to this point already. I have been favored by Mr. Simon with an early copy of the last published of the valuable "Reports on the Public Health," which, in his capacity of their medical officer, he annually presents to the Lords of the Privy Council. The appendix to this report contains an introductory essay "On the Intimate Pathology of Contagion," by Dr. Burdon Sanderson, which is one of the clearest, most comprehensive, and well-reasoned discussions of a great question which has come under my notice for a long time. I refer you to it for details and for the authorities for the statements I am about to make.

You are familiar with what happens in vaccination. A minute cut is made in the skin, and an infinitesimal quantity of vaccine matter is inserted into the wound. Within a certain time a vesicle appears in the place of the wound, and the fluid which distends this vesicle is vaccine matter, in quantity a hundred or a thousandfold that which was originally inserted. Now what has taken place in the course of this operation? Has the vaccine matter, by its irritative property, produced a mere blister, the fluid of which has the same irritative property? Or does the vaccine matter contain living particles, which have grown and multiplied where they have been planted? The observations of M. Chauveau, extended and confirmed by Dr. Sanderson himself, appear to leave no doubt upon this head. Experiments, similar in principle to those of Helmholtz on fermentation and putrefaction, have proved that the active element in the vaccine lymph is non-diffusible, and consists of minute particles not exceeding $\frac{1}{20000}$ of an inch in diameter, which are made visible in the lymph by the microscope. Similar experiments have proved that two of the most destructive of epizootic diseases, sheep-pox and glanders, are also dependent for their existence and their propagation upon extremely small living solid particles, to which the title of *microzymes* is applied. An animal suffering under either of these terrible diseases is a source of infection and contagion to others, for precisely the same reason as a tub of fermenting beer is capable of propagating its fermentation by "infection," or "contagion," to fresh wort. In both cases it is the solid living particles which are efficient; the liquid in which they float, and at the expense of which they live, being altogether passive.

Now arises the question, are these microzymes the results of *Homogenesis*, or of *Xenogenesis;* are they capable, like the *To-*

rulæ of yeast, of arising only by the development of preëxisting germs; or may they be, like the constituents of a nutgall, the results of a modification and individualization of the tissues of the body in which they are found, resulting from the operation of certain conditions? Are they parasites in the zoological sense, or are they merely what Virchow has called "heterologous growth?" It is obvious that this question has the most profound importance, whether we look at it from a practical or from a theoretical point of view. A parasite may be stamped out by destroying its germs, but a pathological product can only be annihilated by removing the conditions which give rise to it.

It appears to me that this great problem will have to be solved for each zymotic disease separately, for analogy cuts two ways. I have dwelt upon the analogy of pathological modification, which is in favor of the xenogenetic origin of microzymes; but I must now speak of the equally strong analogies in favor of the origin of such pestiferous particles by the ordinary process of the generation of like from like.

It is, at present, a well-established fact that certain diseases, both of plants and of animals, which have all the characters of contagious and infectious epidemics, are caused by minute organisms. The smut of wheat is a well-known instance of such a disease, and it cannot be doubted that the grape-disease and the potato-disease fall under the same category. Among animals, insects are wonderfully liable to the ravages of contagious and infectious diseases caused by microscopic *Fungi*.

In autumn, it is not uncommon to see flies, motionless upon a window-pane, with a sort of magic circle, in white, drawn round them. On microscopic examination, the magic circle is found to consist of innumerable spores, which have been thrown off in all directions by a minute fungus called *Empusa muscæ*, the spore-forming filaments of which stand out like a pile of velvet from the body of the fly. These spore-forming filaments are connected with others which fill the interior of the fly's body like so much fine wool, having eaten away and destroyed the creature's viscera. This is the full-grown condition of the *Empusa*. If traced back to its earlier stages, in flies which are still active, and to all appearance healthy, it is found to exist in the form of minute corpuscles which float in the blood of the fly. These multiply and lengthen into filaments, at the expense of the fly's substance; and when they have at last killed the patient, they grow out of its body and give off spores. Healthy flies shut up with diseased ones catch this mortal disease and perish like the others. A most competent observer, M. Cohn, who studied the development of the *Empusa* in the fly very carefully, was utterly unable to discover in what manner the smallest germs of the *Empusa* got into the fly. The spores could not be made to give rise to such germs by cultivation; nor were such germs discoverable in the air, or in the food of the fly. It looked exceedingly like a case of Abiogenesis, or, at any rate, of Xenogenesis; and it is only quite recently that the real course of events has been made out. It has been ascertained, that when one of the spores falls upon the body of a fly, it begins to germinate and sends out a process which bores its way through the fly's skin; this, having reached the interior cavities of its body, gives off the minute floating corpuscles which are the earliest stage of the *Empusa*. The disease is "contagious," because a healthy fly coming in contact with a diseased one, from which the spore-bearing filaments protrude, is pretty sure to carry off a spore or two. It is "infectious" because the spores become scattered about all sorts of matter in the neighborhood of the slain flies.

The silkworm has long been known to be subject to a very fatal and infectious disease called the *Muscardine*. Audouin transmitted it by inoculation. This disease is entirely due to the development of a fungus, *Botrytis Bassiana*, in the body of the caterpiller; and its contagiousness and infectiousness are accounted for in the same way as those of the fly-disease. But of late years a still more serious epizoötic has appeared among the silkworms; and I may mention a few facts which will give you some conception of the gravity of the injury which it has inflicted on France alone.

The production of silk has been for centuries an important branch of industry in Southern France, and in the year 1853 it had attained such a magnitude that the annual produce of the French sericulture was estimated to amount to a tenth of that of the whole world, and represented a money-value of 117,000,000 of francs, or nearly five million sterling. What may be the sum which would represent the money-value of all the industries connected with the working up of the raw silk thus produced is more than I can pretend to estimate. Suffice it to say that the city of Lyons is built upon French silk as much as Manchester was upon American cotton before the civil war.

Silkworms are liable to many diseases; and even before 1853 a peculiar epizoötic, frequently accompanied by the appearance of dark spots upon the skin (whence the name of "Pébrine" which it has received), had been noted for its mortality. But in the years following 1853 this malady broke out with such extreme violence, that, in 1858, the silk-crop was reduced to a third of the amount which it had reached in 1853; and, up till within the last year or two, it has never attained half the yield of 1853. This means not only that the great number of people engaged in silk growing are some thirty millions sterling poorer than they might have been; it means not only that high prices have had to be paid for importing silkworm eggs, and that, after investing his money in them, in paying for mulberry-leaves and for attendance, the cultivator has constantly seen his silkworms perish and himself plunged in ruin; but it means that the looms of Lyons have lacked employment, and that for years enforced idleness and misery have been the portion of a vast population which, in former days, was industrious and well to do.

In 1858 the gravity of the situation caused the French Academy of Sciences to appoint Commissioners, of whom a distinguished naturalist, M. de Quatrefages, was one to inquire into the nature of this disease, and, if possible, to devise some means of staying the plague. In reading the report* made by M. de Quatrefages in 1859, it is exceedingly interesting to observe that his elaborate study of the Pébrine forced the conviction upon his mind that, in its mode of occurrence and propagation, the disease of the silkworm is, in every respect, comparable to the cholera among mankind. But it differs from the cholera, and so far is a more formidable disease, in being hereditary, and in being under some circumstances, contagious as well as infectious.

The Italian naturalist, Filippi, discovered in the blood of the silkworms affected by the strange disease a multitude of cylindrical corpuscles, each about $\frac{1}{6000}$ of an inch long. These have been carefully studied by Lebert, and named by him *Pansistophyton;* for the reason that in subjects in which the disease is strongly developed, the corpuscles swarm in every tissue and organ of the body, and even pass into the undeveloped eggs of the female moth. But are these corpuscles causes, or mere concomitants, of the disease? Some naturalists took one view and some another; and it was not until the French Government, alarmed by the continued ravages of the malady, and the inefficiency of the remedies which had been suggested, dispatched M. Pasteur to study it, that the question received its final settlement; at a great sacrifice, not only of the time and peace of mind of that eminent philosopher, but, I regret to have to add, of his health.†

But the sacrifice has not been in vain. It is now certain that this devastating, cholera-like Pébrine is the effect of the growth and mutiplication of the *Panhistophyton* in the silkworm. It is contagious and infections because the corpuscles of the *Panhistophyton* pass away from the bodies of the diseased caterpillars, directly or indirectly, to the alimentary canal of healthy silkworms in their neighborhood; it is hereditary, because the corpuscles enter into the eggs while they are being formed, and consequently are carried within them when they are laid; and for this reason, also, it presents the very singular peculiarity of being inherited only on the mother's side. There is not a single one of all the apparently capricious and unaccountable phenomena presented by the Pébrine, but has received its explanation from the fact that the disease is the result of the presence of the microscopic organism, *Panhistophyton*.

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* Etudes sur les Maladies Actuelles des Vers à Soie, p. 53.

† In Nature. No. xxxvi, p. 181, will be found a *résumé*, by Prof. Tyndall, of Pasteur's investigations of the silkworm disease.

Such being the facts with respect to the Pébrine, what are the indications as to the method of preventing it? It is obvious that this depends upon the way in which the *Panhistophyton* is generated. If it may be generated by Abiogenesis, or by Xenogenesis, within the silkworm or its moth, the extirpation of the disease must depend upon the prevention of the occurrence of the conditions under which this generation takes place. But if, on the other hand, the *Panhistophyton* is an independent organism, which is no more generated by the silkworm than the mistletoe is generated by the oak or the apple-tree on which it grows, though it may need the silkworm for its development in the same way as the mistletoe needs the tree, then the indications are totally different. The sole thing to be done is to get rid of and keep away the germs of the *Panhistophyton*. As might be imagined, from the course of his previous investigations, M. Pasteur was led to believe that the latter was the right theory; and, guided by that theory, he has devised a method of extirpating the disease, which has proved to be completely successful wherever it has been properly carried out.

There can be no reason, then, for doubting that, among insects, contagious and infectious diseases, of great malignity, are caused by minute organisms which are produced from preëxisting germs, or by homogenesis; and there is no reason, that I know of, for believing that what happens in insects may not take place in the highest animals. Indeed, there is already strong evidence that some diseases of an extremely malignant and fatal character to which man is subject, are as much the work of minute organisms as is the Pébrine. I refer for evidence of this to the very striking facts adduced by Professor Lister in his various well-known publications on the antiseptic method of treatment. It seems to me impossible to rise from the perusal of those publications without a strong conviction that the lamentable mortality which so frequently dogs the footsteps of the most skillful operator, and those deadly consequences of wounds and injuries which seem to haunt the very walls of great hospitals, and are, even now, destroying more men than die of bullet or bayonet, are due to the importation of minute organisms into wounds, and their increase and multiplication; and that the surgeon who saves most lives will be he who best works out the practical consequences of the hypothesis of Redi.

I commenced this Address by asking you to follow me in an attempt to trace the path which has been followed by a scientific idea, in its long and slow progress from the position of a probable hypothesis to that of an established law of nature. Our survey has not taken us into very attractive regions; it has lain, chiefly, in a land flowing with the abominable, and peopled with mere grubs and mouldiness. And it may be imagined with what smiles and shrugs, practical and serious contemporaries of Redi and of Spallanzani may have commented on the waste of their high abilities in toiling at the solution of problems which, though curious enough in themselves, could be of no conceivable utility to mankind. Nevertheless you will have observed that before we had traveled very far upon our road there appeared, on the right hand and on the left, fields laden with a harvest of golden grain, immediately convertible into those things which the most sordidly practical of men will admit to have value, viz: money and life.

The direct loss to France caused by the Pébrine in seventeen years cannot be estimated at less than fifty millions sterling; and if we add to this what Redi's idea, in Pasteur's hands, has done for the wine-grower and for the vinegar-maker, and try to capitalise its value, we shall find that it will go a long way towards repairing the money losses caused by the frightful and calamitous war of this autumn. And as to the equivalent of Redi's thought in life, how can we over-estimate the value of that knowledge of the nature of epidemic and epizoötic diseases, and consequently of the means of checking, or eradicating, them, the dawn of which has assuredly commenced?

Looking back no further than ten years, it is possible to select three (1863, 1864, and 1869) in which the total number of deaths from scarlet-fever alone amounted to ninety thousand. That is the return of killed, the maimed and disabled being left out of sight. Why, it is to be hoped that the list of killed in the present bloodiest of all wars will not amount to more than this! But the facts which I have placed before you must leave the least sanguine without a doubt that the nature and the causes of this scourge will, one day, be as well understood as those of the Pébrine are now; and that the long-suffered massacre of our innocents will come to an end.

And thus mankind will have one more admonition that "the people perish for lack of knowledge;" and that the alleviation of the miseries, and the promotion of the welfare, of men must be sought, by those who will not lose their pains, in that diligent, patient, loving study of all the multitudinous aspects of Nature, the results of which constitute exact knowledge, or Science. It is the justification and the glory of this great meeting that it is gathered together for no other object than the advancement of the moiety of science which deals with those phenomena of nature which we call physical. May its endeavors be crowned with a full measure of success.

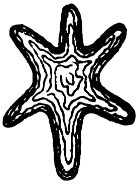
ART. XLIV.—*The Hail-storm of June 20th*, 1870; by Rev. HORACE C. HOVEY, M.A.

THIS remarkable storm swept along a path about thirty miles wide, and extending from Troy, N. Y., to Bangor, Me., though it was not everywhere accompanied by hail.

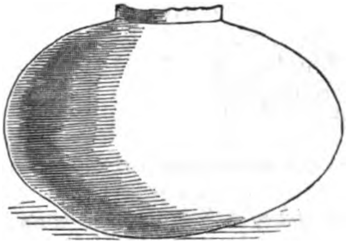
My point of observation was in Northampton, Mass., which was in the central line of the storm.

At sunrise the atmosphere was obscured by fog, which was partially dispersed at a later hour. The day was sultry. At noon the thermometer indicated 88° in the shade. At 3 P. M. a vast mass of dark-green cloud rolled up from the N. W., while lateral currents seemed to set in, forcing the clouds at first into confusion, but afterwards into a well-defined vortex, or spout. The electrical detonations were frequent and sharp. No rain preceded the hail, though it fell copiously after a few minutes. The first hail-stones were about one inch in diameter, and seemed to fall from a greater height, and with more force, than those that fell subsequently. The latter were probably nearer the center of the vortex, and so had their downward motion restrained by that which was lateral. The first that fell were, most of them, on striking the ground, instantly buried out of sight. If they struck on a rocky surface they were dashed in pieces, or else rebounded to a considerable height in in the air. Had their larger successors been driven by a corresponding force, nothing could have survived their assault. The smaller hail-stones were generally flattened spheres, though sometimes in rude stellar forms, (fig. 1). But the largest ones were symmetrical ovoids; each being surmounted, however, by a roughened crown, (fig. 2). The dimensions and weight of three specimens are given, with such accuracy as could be secured by the means at hand. These are but samples of thousands that fell till the earth was covered with ice. The first was, in long diameter, 3⅝ inches; short diameter, 2⅛ inches; weight 7 ounces. The second was 3½ inches by 2¼; weight 8 ounces. The third was 4 inches by 2⅝; weight 10 ounces.

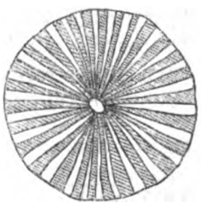
1.



2. 3½″ × 2¼″.

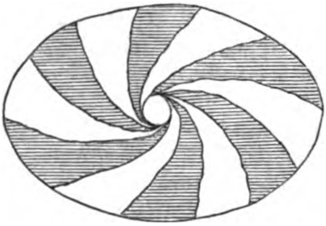


3. 2″ diam.

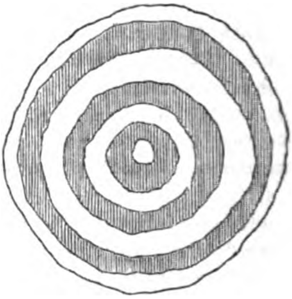


This monster, a foot in circumference, did not entirely melt away for six hours after it fell! The ice in all the hail-stones was peculiarly hard and compact. Interesting structural peculiarities were noted. Hail-stones of stellar form were always transparent and homogeneous. The spheroids were covered with an opaque coating, and had likewise an opaque center. On being bisected some of them showed a radiated structure, the alternate rays being white and clear, (fig. 3). The largest hail-stones had an axis of white ice, half an inch in diameter, around which the alternate layers were arranged in spiral convolutions, (fig. 4). The most common form was in concentric layers, like the coats of an onion, still alternating opaque and transparent; but the edges were finely serrated, like the stripes in some species of agate, (fig. 5). In one hailstone I counted thirteen of these layers, indicating that it had passed through as many strata of snowy and vaporous cloud.

4. 3½″ × 2¼″.



5. 3″ diam.



After a lull in the storm, for half an hour, there was a second fall of hail, but much lighter than the first.

The damage done by such a war of the elements cannot easily be ascertained. Vegetation suffered greatly. In some cases men and animals were wounded. The icy missles not only broke thousands of pains of glass, but also in many instances the window-blinds and sash. In a few cases weather-worn house-roofs were pierced.

Peoria, Ill., July 25, 1870.

Art. XLV.—*Photograph of a Solar Prominence;* by Prof. C. A. Young, of Dartmouth College.

The following is from a letter to the editors dated Sept. 28th, 1870:—

I have just succeeded, with the help of our skillful artist Mr. H. O. Bly, in obtaining a photograph of one of the solar prominences, a copy of which I enclose. It was taken through the hydrogen line, near G, by opening the slit of the spectroscope and attaching a small camera to its eye-piece. As a *picture* of course it amounts to very little. It required an exposure of three minutes and a half, and the polar axis of the telescope being imperfectly adjusted, the clock-work failed to follow perfectly, so that no detail is visible, and the picture will not bear much magnifying. I am convinced, however, that by using a more sensitive collodion, and taking proper pains with the adjustment of the instrument, satisfactory photographs of these curious objects may be obtained.

I may add that the spectroscope employed has the dispersive power of 13 prisms of flint, each with an angle of 55°.

With it I observed this afternoon in the spectrum of a spot, the reversal of the following lines, viz: C, D_1, D_2, D_3, 1474 K, (very faint), b_1, b_2, b_3, b_4, F, 2796 K, (Hγ), and *h* (Hδ). b_3 was most conspicuous after C_1, D_3, and F.

ART. XLVI.—*Contributions to Zoölogy from the Museum of Yale College.* No. 8.—*Descriptions of some New England Nudibranchiata;* by A. E. VERRILL.

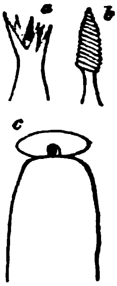
DURING a dredging expedition to Eastport, Me., and Grand Menan, the past season, in company with Mr. Oscar Harger and C. H. Dwinelle, students in the Sheffield Scientific School, the following very interesting species was obtained. Many other Nudibranchs were also observed, most of which are well known species.

Dendronotus robustus, sp. nov. Figure 1.

Body stout, about 2 inches long; ·5 broad, and about the same in height, somewhat quadrangular, tapering posteriorly, but much less acute than in *D. arborescens*, as well as much stouter throughout. Branchiæ in about six pairs, those of the three first pairs with a supplementary one of nearly the same size arising separately outside of, but close to their bases; on the fourth pair these originate from the base as large branches, and on the following ones they are more distinctly branches, arising from the sides near the bases of the branchiæ. The branchiæ are diffusely arborescent and very much subdivided, the divisions taking place very rapidly, the branches being more equal in length and more spreading than in *D. arborescens*, and do not have the long, slender and acute main branches seen in that species. The sheaths of the tentacles (figure 1, *a*) are round and stout, about ·4 of an inch long and ·12 in diameter, and are destitute of any lateral branches; they divide at top into five simple, round, smooth, tapering, acute divisions, of which the two posterior ones are longest. The tentacles (figure

1, *b*) are about equal in length to the lobes of the sheath, the pedicle forming about half of the visible part; the terminal portion suddenly enlarges at first, becomes somewhat conical, and tapers to an obtuse point; it has ten or twelve oblique plications. Front of head with numerous (about thirty) sparingly branched appendages arranged in two series. In the upper series there are about ten, the outer ones being largest; these have stout stems with a few conical, tapering branches, mostly on the lower side, which are tipped with sulphur-yellow. Below these are numerous unequal, smaller, and more simple appendages, about ten on each side, part of which are forked at the end, while others are simple and papilliform and surround the expanded oral disk; all are tipped with yellow. The oral disk (figure 1, *c*) is tranversely elliptical. The foot is nearly as broad as the body (·4 of an inch), and can be adapted for clasping by infolding the edges.

1.



Color pale grayish, thickly sprinkled with small yellow spots, which become less numerous on the oral appendages and sheaths of the tentacles.

Whale Cove, Grand Menan, on sea-weeds in a pool near low-water mark. One specimen only, found by Mr. Oscar Harger.

Dendronotus arborescens Alder and Hancock (*D. Reynoldsii* Couthouy) differs widely from this species in having a very narrow foot; an elevated compressed body, which is more slender and more acute behind; a much smaller number (about ten or twelve) appendages in front of the head, of which the six upper ones are larger and much more branched, and the four lower ones very small; the gills longer and the branches more unequal, while the lowest branch on the outside arises from the side, above the base, even on the front pairs; and in having more clavate tentacles, with longer and branched lobes to their sheaths, while the sheaths also have a large, arborescent, gill-like branch originating from the outer side toward the base. By the last character alcoholic specimens can easily be distinguished. Both species occurred together in the same pool.

Doris bifida, sp. nov.

Outline broad oval, widest anteriorly, 1 inch long by ·5 broad, in extension, back very convex, mantle covered with numerous, scattered, prominent, pointed papillæ. Tentacles rather long, thickest in the middle, the outer half strongly plicated, but with a smooth tip, the base surrounded by small papillæ. Gills retractile into a single cavity, united together by a partial web,

Figure 1.—*Dendronotus robustus* Verrill; *a*, tentacle sheath, natural size; *b*, tentacle, enlarged; *c*, oral disk and anterior part of the foot, natural size; from living specimen by A. E. Verrill.

deeply frilled, much subdivided, bipinnate, the subdivisions fine and slender. Foot very broad, in extension projecting back beyond the mantle about a quarter of an inch, slightly tapering, rounded and slightly notched at the end. Oral disk or veil crescent-shaped, the front a little prominent, the sides extended backward and forming a curve continuous with that of the foot.

Color dark purplish brown, sprinkled with white specks; tentacles deep brown, specked with white, tips yellowish; gills purplish at base, the edges and tips yellow; foot similar in color to mantle, but lighter.

Eastport, Me., at low-water mark, under stones, Aug. 19, 1868.

Onchidoris tenella.

Doris tenella Ag., in Gould, Invert. of Mass., Ed. 2, p. 229, Pl. xx, figures 289, 290, 293, 1870.

Specimens of this rare and imperfectly known species were obtained under stones in a large pool at low-water mark, near Eastport, Me.

The largest one was ·35 of an inch long, and ·20 to ·25 broad, according to the position. The outline is oval, elliptical, or oblong, in different states of extension, and the edges of the mantle are often rolled inward. The back is strongly convex, the surface thickly covered with small conical papillæ, which are strengthened by numerous minute white spicula. The tentacles are rather long, oblong, scarcely tapering, with numerous transverse laminæ, which cover nearly the whole length, the tip with a small obtuse papilla; the base is surrounded by a short sheath, with the edge divided into five small conical papillæ or teeth, the two anterior ones largest. Branchiæ nine, seven principal ones with two very small ones posteriorly; the larger ones are short, thick, lanceolate, with short lateral lobes. In the center of the branchial circle there is a small brownish papilla. The foot is long-oval, tapering behind and rounded in front, about half as wide as the mantle, and very much shorter. The oral disk is short and broad, subtriangular, with a very obtuse angle in front.

Color of the upper surface yellowish white, the papillæ mostly tipped with yellow, but some with flake-white; tentacles lemon-yellow with lighter tips; branchiæ yellowish white, edged and tipped with lemon-yellow, the yellow tint conspicuous in partial contraction; foot yellowish white; mouth and edge of oral disk bright yellow.

Onchidoris grisea.

Doris grisea Stimpson MS., in Gould, op. cit., p. 232, Pl. xx, figures 292, 295.

This species occurred under the same circumstances as the preceding, and more commonly. The color was generally clear white, sometimes tinged with pale sulphur-yellow, in some parts.

Onchidoris pallida.

Doris pallida Ag., in Gould, op. cit., p. 229, Pl. xx, figs. 284, 287, 288, 291.

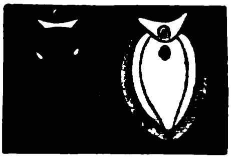
This species was dredged in 20 fathoms in Eastport harbor. It has much larger tubercles than either of the preceding.

Doridella, gen. nov.

Body covered with an ample, smooth mantle, oval, convex. Dorsal tentacles retractile, without sheaths. Head prominent, the lateral angles prolonged anteriorly as short oral palpi or tentacles. Foot broad, cordate. Branchiæ posterior, in the groove between the mantle and foot.

Doridella obscura, sp. nov. Figures 2 and 3.

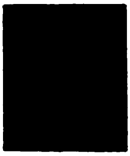
Form broad oval, ·3 of an inch long and ·2 broad; back convex, smooth. Foot broad, cordate in front. Oral disk broad, emarginate or with concave outline in front; The angles somewhat produced, forming short, tentacle-like organs, which in extension project beyond the front edge of the mantle. Dorsal tentacles small, stout, retractile. Color of body blackish, lighter toward the edge, as if covered with nearly confluent black spots, the whitish ground color showing between them; foot, oral disk, and dorsal tentacles white; the central part of body, beneath, bright yellow.



Savin Rock, near New Haven, Oct. 28, 1868,—E. T. Nelson.

The eggs, laid in confinement, were very small, pale yellow, numerous, arranged in an open coil, (figure 3).

This is the only Nudibranch hitherto discovered in the vicinity of New Haven. It appears to be allied to *Phyllidia* and *Fryeria*, which are usually referred to the Tectibranchs.



Art. XLVII.—*On a recent Earthquake at Bogota;* by the Hon. S. A. Hurlbut, U. S. Minister to Columbia.

[The following communication respecting an earthquake recently felt at Bogota, observed by the Hon. S. A. Hurlbut, U. S. Minister to Columbia, has been kindly furnished to us by the Secretary of the Smithsonian Institution, to whom it was addressed.]

We have had rather an unusual phenomenon at this place in a remarkably well developed earthquake. At about 10 minutes before 10 P. M. of the evening of the 4th of April (Saturday), and without any previous warning that we had noticed, there

Figure 2.—*Doridella obscura* Verrill, enlarged two diameters; *a*, upper surface; *b*, lower surface.

Figure 3.—Eggs of *D. obscura*, enlarged two diameters.

occurred first, a moderate shock not of any peculiar force and consisting of a single vibration; this was momentary. In about two minutes afterward a very sharp movement took place, giving the impression of a lateral motion from north to south. The table on which I had my elbow at the time seemed to recede about 1½ to 2 inches, quiver an instant and return to its place: the beams of the houses creaked like the timbers of a ship in heavy weather. Doors and windows flew open. Those who were in bed at the time seemed to feel it much more, and the effect of the vibrations was to make many "sea sick." This shock, they tell me, was the sharpest known here since 1826. I cannot learn of any damage done to buildings in the city.

The unquietness of the earth continued from the time mentioned until nearly 11 P. M., with a species of shuddering motion scarcely perceptible unless one were lying down. There was heard with each shock, a peculiar muffled rushing sound, not as clear and distinct as the movement of wind, but something like it. At the moment of the principal shock I looked at my watch and found the time to be ten minutes of ten—Bogota time. Time however, here, is not well regulated, as the observatory possesses no instruments and is neglected. The direction of the movement was very distinct from the north to the south. As earthquakes rarely have their centers in Columbia and are generally the result of action in Ecuador, it may be advisable to connect this observation with notices from that country. I believe there is but one volcano in action in Colombia-Puracè.

Some nights since we noticed for two hours after sunset in the west, and nearly in the range of Tolima, a well defined column or line of light, on the Cordillera. This bore about due west. The character of the light I could not determine.

Legation of the United States, Bogota, June 6, 1870.

ART. XLVIII.—*Discovery of a new Planet, the* 112*th, named Iphigenia;* by Dr. C. H. F. PETERS, of the Litchfield Observatory of Hamilton College. Letter to the Editors, dated Clinton, Oneida Co., N. Y., Sept. 22, 1870.

I HAVE the pleasure to communicate the following observations upon an asteroid discovered on the night of the 19th inst.

1870	Ham. Coll. m.t.	App. A.R.	App. Decl.	
	h m	h m s	° ′	
Sept 19.	15 30 —	1 2 35	+10 16 —	(approx. by estimation).
" 20.	14 57 28	1 1 51·54	+10 13 30·8	10 comp. W. 0^h 1079.
" 21.	10 48 22	1 1 12·24	+10 10 58·9	10 comp. Schj. 374.

The planet is about 11th magnitude, receives the number (112), and I have already given a name to it, *Iphigenia*, while that found on Aug. 14, (111), has been called *Ate*, with regard to the simultaneous events in Europe.

ART. XLIX.—*Geological Explorations in China;* by Baron von RICHTHOFEN. In a letter to Prof. J. D. WHITNEY, dated Peking, Aug. 20th, 1869, and communicated by him for this Journal.

I PROMISED you, a few days ago, a more detailed account of the geological results of my travels in Manchuria and the province of Chi-li than I wished to give before having visited at least some of the localities near Peking examined before by Pumpelly. I have done this and am now acquainted with the most important formations occurring in the neighborhood of this capital.

The southern province of Manchuria has the name Shing-King and is divided by the Liao river into Liao-tung and Liao-hsi (meaning East and West of the Liao). The course of my travels from May to July was as follows: from Chifu by sea to Nin-chang, at the mouth of the Liao; thence by land down the western and up the southeastern coast of Liao-tung, to the frontier of Corea, then to the northeast, along this frontier, and to Mukden; from Mukden I went to Peking by Kin-chan and Yung-ping.

There are certain circumstances which render the geology of northern China difficult. In the first place the extent of the country is very great, and the character of the formations changes no less in the different provinces of it than it does in other regions of the globe. One is easily inclined, on a hasty tour of reconnoissance, to compare the strata in various regions on lithological grounds. But if I bear in mind the erroneous conclusions arrived at in the European Alps, by geologists who endeavored to determine the age of the sedimentary formations on the strength of their lithological resemblance to the formations of other parts of Europe, I think I cannot be cautious enough in this new country. Lithological analogy can here be used as a safe guide only when carefully traced from province to province and so on to remote regions. Then there is the apparent absence of any great geological events creating disturbances simultaneously over the whole region. Ancient deposition continued in one place while it was interrupted in another by the dislocation of those strata previously deposited. Another difficulty is the scarcity of fossils. I dare say that, with the exception of a few plants of the Coal-measures, I have discovered all localities of fossils now known to exist. Yet, if it is considered that I never received any knowledge of their existence (excepting Lake Tai-hu) not even the slightest hint, from either native or foreigner, but that I had to discover every fossil myself in hurrying through the country, it will still be found surprising, that the number of known fossiliferous localities is so great. I believe that China will, on a closer examination, contribute largely to the knowledge of the most ancient animal life on the globe. There is another difficulty caused by the recurrence, at different levels, of strata which bear a close similarity to each other. This relates chiefly to certain quartzose sandstones of a reddish or yellow color when converted into quartzite. They have often a thickness of thousands of feet, and, as they are less affected by denudation than many other stratified rocks, they frequently compose mountain ranges for themselves, without offering any clue for determining their stratigraphical position. This is also true, though in a far less degree, of the limestones. In regard to these, as also to the knowledge of the occurrence of fossils, the analogy with the history of the exploration of the European Alps is striking. One cannot be astonished that the first explorer of Chinese geology, though a most admirable observer, should have distinguished only one great limestone formation in China, and referred it all to that age (namely, the Devonian) to which all the fossils then known (namely, those which are sold in drug stores) were believed to belong; just as "the Alpine limestone" was until not very long ago a comprehensive term used to designate all the limestones occurring at different geological levels in the Alps. You may recollect that already my observations on the Yang-tse induced me to distinguish there at least three limestone formations. Since then I have come to the conclusion that it will not be difficult, on detailed examination, to establish in other regions a greater number at distinct levels.

It is for the various reasons mentioned, that I examined every mountainous country independently from what I had seen before, applying new terms for the different formations observed, and tried to establish the analogy between different regions only after having completed the exploration of each. The series of formations, as established on the Yang-tse, has found thereby a great deal of additional support and, I think, will prove to be a near approximation to the true order of succession.

In my two previous letters (March 1st and May 8th) I mentioned my having found Carboniferous fossils on the Yang-tse and in Shantung, in certain limestones and shales which occur there in close connection with the Coal-measures, or indeed form part of them. They mark there a very distinct and remarkable horizon. I did not find any corresponding fossils in Manchuria or Chili. There, however, I was fortunate enough to collect sufficient material for determining the age of a formation which corresponds, stratigraphically and lithologically, with the lowest ("Matsu") limestone on the Yang-tse and the formation mentioned as No. 2 of Shantung in my letter of May 8th. It has a thickness of many thousand feet, and is, lithologically, exceedingly varied. It consists of endless alternations of red micaceous argyllite and brown sandstone, with strata of limestone, the former two prevailing in the lower half, the latter in the upper portion. The limestone itself presents a large number of varieties, and one can distinguish, on purely lithological grounds, several horizons, the order of succession of which is similar in wide regions. One of these horizons is marked by black oölitic limestone, which, together with some other conspicuous varieties of limestone, extends from the Yang-tse to Liao-tung and to Peking (twelve degrees of latitude and ten of longitude).

On the frontier of Corea, this oolitic limestone abounds in the remains of Trilobites and small brachiopods, chiefly of the Orthis and Lingula families. The sediments were there deposited in inlets of the ancient sea between ridges consisting chiefly of gneiss, granite and quartzite. The accumulation of the shells of trilobites which took place in these protected abodes is astonishing. I collected quite a number of species, but nothing adequate to the material which I saw, as several circumstances, chiefly the danger of being cut off by torrents swelled by the copious rains of the season, made it necessary to hurry over the most prolific localities. Yet, I hope that it will now be possible to determine the age of one of the most important formations of Northern China. It predominates, in bulk, over all other sedimentary formations of Lio-tung and extends thence into Corea. In Liao-tsi, it appears to have been removed by denudation to a great extent. It appears only in places, and even in these it is often covered by porphyries. Again it takes a dominent part in the structure of the country in the province of Chi-li, chiefly between Yung-ping-pu and Peking, becoming, however, more and more metamorphosed with the approach to the latter city. Its limestones (that is, its upper portion) form the narrows which lead to the Nankan pass. They are intensely altered, and traversed by innumerable dykes of intrusive rocks. What Pumpelly describes as the Hwaingan beds, are probably the lower strata of this same great formation.

In Liao-tung, as in Shan-tung, the oolitic limestones carrying the trilobites, are overlain by a great thickness of limestone, which is immediately followed by the Coal-measures. In Liao-tung, I found no fossils in this limestone, excepting numerous Ammonites and Orthoceratites, which cannot be determined. They occur immediately below the coal-bearing strata, the conformable superposition of which on the limestone I observed in a number of places. There are localities, such as northwestern Shan-tung and southern Liao-tung, where there is an apparently uninterrupted series of sedimentary deposits, commencing with sandstones and shales thousands of feet lower than the oolitic limestone and ending with the Coal-measures. Every layer appears to be conformable to that which it overlies, but no such parallelism exists between the lower and upper portions of the series, a gradual change of inclination marking the former gradual changes of level. It is not improbable that in these localities the entire series of formations, from the Silurian to the Coal-measures, is represented. It is in these regions that the order of succession will have to be studied. There are, however, more numerous instances where the outbreaks first of granitic and then of porphyric rocks, which extended evidently over long periods, have created repeated disturbances. The vicinity of Peking has been among the theaters of the most intense eruptive action. But even here the Coal-measures are conformable to the underlying limestones, in all instances which came under my observation.

I am unable to pronounce at present an opinion on the relation of the northern Coal-measures to those of Shan-tung and the lower Yang-tse. The remains of plants which I collected in nearly every one of the northern coal-fields will probably help to elucidate this question. But, judging on mere stratigraphical grounds, I cannot help thinking, that the Coal-measures of Liao-tung and Liao-hsi, and the lower portion of those in the neighborhood of Peking, will not differ much in age, either among themselves, or as compared with the Coal-measures of Middle China, It is, however, a noteworthy fact, that the coal formation of Peking has an extraordinary development. I had only occasion to see those coal-bearing strata which lie immediately on the limestone. West of Peking, these strata, together with the limestone and a few thousand feet of superincumbent deposits, have undergone an intense metamorphism, the coal being converted into anthracite. It appears, from Mr. Pumpelly's description, that the Chaitung Coal-measures (which I did not visit) occupy a, geologically, much higher level. Altogether it may be safe to conclude, that in China, as in other countries, the deposition of coal and intervening sediments continued during a considerable period, in which it shifted to different regions. Yet, I must confess, that comparison on stratigraphical grounds makes it difficult to believe that any portion of the Coal-measures of Northern China should be of so recent age as Dr. Newberry was inclined to conclude on the strength of the vegetable remains which he determined.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the influence of electricity on air and oxygen as a means of producing ozone.*—Houzeau has drawn the following conclusions from a great number of estimations of ozone obtained by means of Ruhmkorff's apparatus.

(1.) The production of ozone is greater in air renewed from time to time than in confined air.

(2.) It is greater at the negative than at the positive pole.

(3.) The production of ozone increases only up to a certain point with the duration of the electric action.

(4.) The ozone increases with the electric intensity.

(5.) The ozone diminishes when the distance which separates the electrodes increases.

(6.) The production of ozone varies with the length or surface of the electrodes.

(7.) Other conditions being equal, the production of ozone is greater by utilizing the effect of the two electrodes.

(8.) The production of ozone is equally manifested, out of direct contact with the air, with metallic electrodes, when these last are surrounded for their whole lengths with tubes of thin glass play-ing the part of insulating sheaths, whether the extremities of these tubes are closed or not.

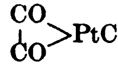
(9.) Still the production of ozone resulting from the passage of air over the naked metallic electrodes (direct contact with the platinum wires) is greater than that which arises from the passage of the air round the same electrodes, when sheathed and closed, (no direct contact of the air with the naked metallic electrodes).

(10.) With closed sheathed electrodes the production of ozone varies equally with the length or surface of the metallic electrodes.

(11.) The production of ozone increases considerably with a diminution of the temperature at which the electrification of the air is effected.

(12.) All conditions being equal, the quantity of ozone produced with a definite volume of oxygen, is always much more considerable (about eight or ten times) than that furnished by the same volume of air.

(13.) The ozone produced by the obscure electrification of air, is accompanied by small quantities of nitrous compounds, while that which is furnished by pure oxygen under the same circumstances, contains only traces. By attention to the conditions above described, the author has been able to construct a new apparatus, which he calls an *ozonizer*, and with which, according to his statement, quantities of ozone hitherto unknown, may be prepared. The apparatus is not described in the paper from which our extract is taken.—*Comptes Rendus*, lxx, 1286. W. G.

2. *Researches on Platinum.*—SCHÜTZENBERGER has communicated to the Academy of Sciences further investigations of the remarkable compounds of platinum with carbonic oxyd, already noticed in this Journal, and has in addition described some new series of great interest and theoretical importance. Setting out with the view that the two compounds already described, $CO = PtCl_2$, and  $\begin{matrix}CO\\ |\\ CO\end{matrix}\!>PtCl_2$,* may be regarded as the chlorids of two diatomic radicals, the author found that ammonia unites with each compound, forming the two new chlorids represented respectively by the formulas,

$$N_2H_6 . CO . PtCl_2, \text{ and } N_2H_6 . (CO)_2 . PtCl_2.$$

When heated these compounds fuse, yielding metallic platinum, nitrogen, hydrogen and sal-ammoniac, together with a volatile liquid having a penetrating odor which appears to be chlorid of formyl, $COHCl$. The compound $CO . PtCl_2$ absorbs dry ethylene and forms a yellow crystalline body, which has probably the formula $C_2H_4 . CO . PtCl_2$, and may be regarded as corresponding to the dicarboxyl compound $(CO)_2 . PtCl_2$, C_2H_4 replacing CO. When heated above 95° C. this body gives off chlorhydric acid, while a dark colored body remains insoluble in water and having the formula $C_2H_3Cl . CO . PtCl_2$, so that it contains monochlorinated ethylene in place of ethylene.

Phosphorous chlorid, PCl_3, like carbonic oxyd, unites readily with platinous chlorid, forming a solid reddish-yellow substance, volatile with difficulty, fusing at about 200°, and soluble in phosphorous chlorid, from which it crystallizes in cooling. The composition of this body is represented by the formula $PCl_3 . PtCl_2$. The same substance is formed by heating one atom of platinum with one molecule of phosphoric chlorid, $PCl_5 + Pt = PCl_3 . PtCl_2$, and also by the action of PCl_3 upon $CO . PtCl_2$, CO being given off. The compound $PCl_3 . PtCl_2$ dissolves in water with a yellow color, forming a new complex acid, $P(HO)_3 PtCl_2$, and differs essentially from the body described by Baudrimont, which according to that chemist, has the formula $PCl_5 . PtCl_2$, ($Pt = 98{\cdot}5$). In a later communication to the Academy, Schützenberger gives the formulas and names of a series of very remarkable substances which he has obtained from the primitive, $PCl_3 . PtCl_2$; these are as follows:

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* $Pt=197$ $C=12$ $O=16$.

$PCl_3 . PtCl_2$ chlorid of trichloro-phosphoplatinum.
$P(HO)_3 . PtCl_2$ phospho chloro-platinous acid.
$P(A_gO)_3 . PtCl_2$ silver salt of the same.
$P(C_2H_5O)_3 . PtCl_2$ chlorid of trioxethyl-phospho platinum.
$P(C_2H_5O)_3PtO . H_2O$ hydrate of oxyd of platinum.
$P(C_2H_5O)_3PtO . N_2O_3$ nitrate of oxyd " "
$P(C_2H_5O)_3 . PtN_2H_4 . 2HCl$ chlorhydrate of trioxethyl-phospho-platin-diamin.
$P_2Cl_6 . PtCl_2$ chlorid of hexachloro-diphosphoplatinum.
$P_2(HO)_6 . PtCl_2$ diphospho-chloroplatinous acid.
$P_2(AgO)_6 . PtCl_2$ silver salt.
$P_2(C_2H_5O)_6 . PtCl_2$ chlorid of hexoxethyl-diphospho platinum.
$P_2(C_2H_5O)_6 . PtN_2H_4 . 2HCl$ " " " diamine.
$P_2(C_2H_5O)_3Cl_3PtCl_2$ chlorid of trichloro-trioxethyl-diphospho platinum.
$P_2(C_2H_5O)_3(HO)_3PtCl_2$ diphospho-trioxethyl-chloro-platinous acid.

Schützenberger points out the analogy between these compounds and the ammonia-platinum bases. In a third note he gives a method of isolating the radicals contained in the above described compounds. By treating the alcoholic solutions of the two chlorids $P(C_2H_5O)_3PtCl_2$, and $P_2(C_2H_5O)_6PtCl_2$, with zinc, viscid black masses are obtained, having respectively the formulas $P(C_2H_5O)_3Pt$ and $P_2(C_2H_5O)_6Pt$, and combining directly with chlorine to reproduce the original chlorids.—*Comptes Rendus*, lxx, 1287, 1414; lxxi, 69. W. G.

3. *On new derivatives of triethylphosphine.*—Cahours and Gal have studied the product of the action of platinic chlorid upon triethylphosphine, and have obtained a compound having the formula,* $P(C_4H_5)_3PtCl$, which they regard as the equivalent of the green salt of Magnus in the ammonia series. This body crystallizes from its solution in ether in voluminous transparent prisms of an amber yellow color. An alcoholic solution of this salt heated to 100° for several hours in sealed tubes, yields crystals of an iso-

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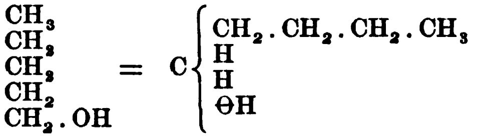
* $C=6$ $O=8$ $Pt=98{\cdot}7$.

meric white salt. When the yellow salt is boiled with water and triethylphosphine, a colorless salt is formed, which has the formula, $[P(C_4H_5)_3]$ PtCl and which the authors consider to be the phosphorous analogue of the salt of Reiset. It loses an atom of triethylphosphine when long kept, and leaves the white modification of the chlorid first described. With argentic oxyd and water it yields a strongly alkaline liquid; with the chlorids of gold and platinum well defined salts. Palladium forms a compound similar to the first described salt of platinum, and also forming amber yellow prisms. This does not, however, unite with another atom of triethylphosphine. When triethylphosphine is added to a solution of auric chlorid, $AuCl_3$, in alcohol a colorless crystalline salt is obtained which has the formula, $P(C_4H_5)_3AuCl$. Compounds of an exactly analogous composition were obtained with the chlorid of platinum, palladium and gold, and triethylarsine.—*Comptes Rendus*, lxx, 1380, lxxi, 208. W. G.

4. *On silico-propionic acid.*—By the simultaneous action of zinc-ethyl and sodium upon ethyl-silicic monochlorhydrine, $SiCl(OC_2H_5)_3$, **Friedel** and **Ladenburg** have obtained a liquid boiling at 158°·5 C. and having the formula, $Si(C_2H_5)(OC_2H_5)_3$, which they term tribasic silico-propionic ether. A concentrated solution of caustic potash does not set free in this compound the silicon in the form of silicic oxyd $Si\Theta_2$, but gives a product having the formula, $SiC_2H_5O_2H$, which however cannot be obtained in this way in a state of purity. By heating silico propionic ether at 180° C. in a closed tube with chlorid of acetyl, the authors obtained a mixture of acetic ether and a body having the formula $SiC_2H_5Cl_3$. By treating with water the part of this liquid which boils at 90°–110°, chlorhydric acid and a white gelatinous body are formed; this last is the hydrate of silico-propionic acid. When dried at 100° the acid forms a white amorphous powder greatly resembling silicic oxyd, but easily distinguished from it by its combustibility. When heated it burns like tinder, disengaging combustible gases. The acid is insoluble in water, but dissolves in hot concentrated caustic potash, and is not precipitated from this solution by HCl, but only by NH_4Cl, like SiO_2, the residue after evaporation being unchanged silico-propionic acid. The new substance appears therefore to be a weak acid analogous to silicic acid, and presents the first known case of a silicic acid containing carbon. Its formula shows that it contains the group, SiO_2H, which may be termed silicoxyl, and which is the analogue of carboxyl, CO_2H. It is easy to see also that it forms one term of a group of homologous acids.—*Comptes Rendus*, lxx, 1407. W. G.

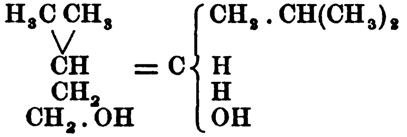
5. *On normal amylic alcohol.*—**Lieben** and **Rossi** have succeeded in obtaining synthetically the normal amylic alcohol, which bears the same relation to the alcohol already known which normal butylic alcohol bears to that obtained by fermentation. Normal cyanid of butyl yields normal valeric acid, which greatly resembles the acid already known, but which has an odor more closely resembling that of butyric acid. It boils at 184°–185° at 736^{mm}. When normal calcic valerate is mixed with normal calcic formate,

and the mixture is distilled in small portions at a time, valeric aldehyd is obtained, boiling at 102° C. This aldehyd, by the action of nascent hydrogen, yields the normal amylic alcohol. The alcohol much resembles that obtained by fermentation, but has a higher boiling point, 137° C. under a pressure of 740^{mm}. The authors have prepared from it the chlorid, bromid, iodid and acetate of amyl, all of which possess higher boiling points than the corresponding ordinary amylic ethers. The constitution of the normal alcohol must be expressed by the formula,



$$\begin{array}{l} \mathrm{CH_3} \\ \mathrm{CH_2} \\ \mathrm{CH_2} \\ \mathrm{CH_2} \\ \mathrm{CH_2 \, . \, OH} \end{array} = \mathrm{C}\left\{\begin{array}{l} \mathrm{CH_2 \, . \, CH_2 \, . \, CH_2 \, . \, CH_3} \\ \mathrm{H} \\ \mathrm{H} \\ \Theta\mathrm{H} \end{array}\right.$$

while common amylic alcohol has probably the formula attributed to it by Erlenmeyer,



$$\begin{array}{c} \mathrm{H_3C \ CH_3} \\ \vee \\ \mathrm{CH} \\ \mathrm{CH_2} \\ \mathrm{CH_2 \, . \, OH} \end{array} = \mathrm{C}\left\{\begin{array}{l} \mathrm{CH_2 \, . \, CH(CH_3)_2} \\ \mathrm{H} \\ \mathrm{H} \\ \mathrm{OH} \end{array}\right.$$

The authors promise a detailed description of the normal valeric acid and its salts.—*Comptes Rendus*, lxxi, 369. W. G.

6. *Transformation of the fatty acids into the corresponding alcohols.*—SAYTZEFF has given a method of passing from the fatty acids to the corresponding alcohols, which possesses much interest. An amalgam of sodium of 3 per cent is to be introduced into a flask, and a mixture of one molecule of the chlorid and two of the acid introduced, the mixture being cooled. After 12 hours water is added, the liquid distilled, and the distillate saturated with potassic carbonate. The product separated is the ether formed by the fatty acid with the corresponding alcohol. This may then be saponified by potash. In this manner the author prepared propylic and butylic alcohols.—*Bull. de la Société Chimique*, xiii, p. 51. W. G.

II. GEOLOGY AND MINERALOGY.

1. *Laurentian Rocks of Nova Scotia;* by Rev. Dr. HONEYMAN, F.G.S., &c.—In this communication I propose to make a few observations bearing upon remarks made by Dr. Hunt on the above subject, in the July number of the American Journal of Science.

While I was engaged in the service of the Canadian Survey, ascertaining the extent of the distribution of the Upper and Middle Silurian rocks of Arisaig, I unexpectedly came upon a band of crystalline rocks, of considerable thickness and of great lithological variety. There was a succession of different kinds of diorites and hornblendic rocks, traversed with veins of quartz and many granular limestones. These extended about two miles along the shore, stretching at the same time both into the sea and land. After these came a thick band of ophites and ophio-calcites. Succeeding these were bands of diorites and hornblendic slates, with quartz veins containing mica in crystals, and then white feldspar rock of considerable thickness with green veins, and a thick band of rose-colored feldspar. I was convinced that I had discovered a series of Laurentian rocks. In geological position it is unquestionably inferior to the Arisaig series, which ranges from the Medina and Oneida through the Clinton, Niagara (?), Lower Helderberg, and possibly the Devonian, into the Lower Carboniferous. There appeared to me to be a sufficient lithological resemblance, between these rocks and those of Canada and Newfoundland Laurentian, to warrant the conclusion. Dr. Hunt gives me the credit of having profited by his suggestions, and the descriptions of Laurentian rocks given by the Canadian Survey, and assures the readers of the Journal, that these first brightened my ideas in regard to the geological age of the rocks in question. Dr. Hunt, however, knows right well that I had other and much better means of acquiring the requisite knowledge. I spent the greater part of six months in the Paris Exhibition, in the immediate vicinity of the Canadian Survey collection of rock specimens, from the Laurentian rocks of Canada and Newfoundland, and those who know me know that I am not in the habit of allowing such good opportunities to pass unimproved. After Mr. Richardson had finished the arrangement of these rocks in the Canadian department of the Exhibition, when there was no geologist in the Canadian and Newfoundland courts, my duties as Executive Commissioner in the Nova Scotia department requiring my constant attendance, I had frequent occasion to direct the attention of English and Continental geologists to the rock specimens referred to; so that I was not altogether ignorant of their character and appearance.

Thus schooled I considered the rocks discovered to be *Laurentian*. The first to whom I communicated the discovery was H. R. Hill, Esq., High Sheriff of Antigonish, who had often accompanied me in my Arisaig trips, and whose knowledge of the Silurian rocks of Arisaig and the places where fossils occur is somewhat intimate. To him I declared positively that I had found the Laurentian rocks in the neighborhood of our old geological field; at the same time I gave him specimens of these rocks. Shortly after I left the field on account of the inclemency of the weather. On my way home I had to pass through New Glasgow; I called upon Sir W. Logan at his hotel and showed him specimens of the ophite and ophiocalcite. He appeared to hail them as an important discovery, declared them to be of Quebec age, and recommended a search for chromic iron in the locality. When Prof. Hind inspected the specimens which I had in the Museum, he appeared then to regard them as Sir W. Logan had done, and in his report on the Waverly Gold Fields, he mentions the discovery as a discovery of Quebec rocks. In the month of December following, I received a letter from Sir W. Logan, addressed to the care of W. A. Hendry, Esq., Deputy Commissioner of Nova Scotia Crown Lands, a gentleman who has had considerable field experience as a geologist, to whom I communicated the contents of the letter. In this letter Sir W. Logan says "after all it is not serpentine you have discovered. Dr. Hunt says it is agalmatolite or dysyntribite and it proves nothing; the rocks may be of Upper Silurian or Devonian age." I was not prepared to maintain that the ophite was not agalmatolite or dysyntribite; simple test proved that the ophiocalcite was undoubtedly a calcite. I could not for a moment admit that the strata in question were Upper Silurian or Devonian. Any one knowing the locality could not fail to be convinced that they were underlying the Upper and Middle Silurian of Arisaig, and therefore might be Lower Silurian, Huronian, or Laurentian, but certainly nothing more recent I had a specimen of the ophiocalcite—ophiocalcite it is now acknowledged to be—polished on two opposite sides by Mr. Wesley, marble worker. In returning it to me he said he had partly polished it by means of acid. It turned out to be a very beautiful and peculiar specimen. I examined its surface by a combination of lenses having a considerable magnifying power, and observed what appeared to me to be structure. I had often examined the magnificent specimens of Eozonal serpentine which were exhibited in the Canadian department of the Paris Exhibition of 1867, which were afterward deposited in the Ecole des Mines of Paris. I also had received from a Bohemian geologist in Paris a specimen of ophiocalcite with *Eozoon Bohemicum.* I compared this with the Nova Scotian specimen; I became impressed with the conviction that if the Bohemian was Eozonal so was the Nova Scotian, and that this was another evidence of the Laurentian age of the Nova Scotian rocks. In my simplicity and ignorance of facts advanced in the Eozonal controversy, I did not know that serpentines of *all ages* might contain *species* of the genus Eozoon. I pointed out the peculiar structure of the Nova Scotian specimen and its resemblance to that of the Bohemian to Mr. Hendry, already referred to. He expressed doubts on the matter of structure in both, as an unbeliever in the Eozoon might do. About the same time Prof. Lawson of Dalhousie College and University, Halifax, came to the Museum and asked if I had a specimen of the *Eozoon Canadense;* I told him that I had not, but that I had a specimen of the *Eozoon Bohemicum* and what might be called the *Eozoon Nova-Scoticum.* He examined them, but replied that he wanted to see the real *Eozoon Canadense.* Mr. Hendry confirms the above statements as far as he is referred to, in the presence of Dr. How of Kings College and University, Windsor, to whose friendship and kindness I am indebted for the loan of a copy of the number of the American Journal of Science, containing Dr. Hunt's article already referred to.

In the spring of 1869, I visited Montreal and took with me the polished specimen of ophiocalcite. I gave it to Dr. Hunt in the lapidary's workshop in Gabriel street. A day or two after, when I was engaged in the same place talking with Sir W. Logan, Dr. Hunt entered with the specimen referred to in his article, and stated to Sir W. Logan in my presence to the effect that Dr. Honeyman had discovered a very interesting series of rocks; that the specimens tempted him to go to see them, and that he believed them to be the Laurentian. Sir W. objected, saying that the specimen was not serpentine. Dr. Hunt assured him that it was ophiocalcite; that he had examined it; that the white effervesced with acid. He also named some of the constituents; I do not recollect what they were. Sir W. and Dr. Hunt left. I heard nothing more about the matter until my very kind host, Dr. Dawson, on returning from a visit to Gabriel street, told me, in his library, that Dr. Hunt had said to him that certain rocks that I had discovered were of Laurentian age; but that he had advised him to consider them first of all Devonian. All that I replied was, I heard him say so. For certain private reasons I expressed no opinion on the subject. When I returned to Halifax I arranged my representative collection of Nova Scotia rocks in the Provincial Museum under my charge, thus: Arisaig ophite, ophiocalcite and porphyritic diorite, at the bottom of the series; granites, new gneissoid rocks of Prof. Hind, next; andalusite rock and slates next; and thus they had remained until the present time. Many a time have I pointed out these rocks to visitors who take an interest in such matters, and told them the lowest ones were Laurentian. I have certainly not been silent, as Dr. Hunt informs the readers of the American Journal of Science, although the sound of my voice may not have reached Gabriel street, Montreal. I could name many witnesses to attest this.

I would now observe that, from one cause or other, I had never met with Prof. Hind since the time already referred to, until a day or two after he had read a paper on his discovery of the Laurentian rocks, before the Nova Scotia Institute of Natural Science. He came to meet Dr. Lawson in the Museum. He commenced discussing the subject of his discovery. I then turned his attention to my collection of rocks; showed him the Laurentian of Arisaig, and gave him a detailed account of the opinions expressed in regard to them. He informed me that he had announced his discovery of the Laurentian gneissoid in Sherbrooke and elsewhere, to Sir Roderick Murchison, Sir W. Logan, &c. Not long after a letter was inserted in the Halifax Morning Chronicle, addressed by Prof. Hind to the Hon. Robert Robertson, M.E.C., Commissioner of Mines and Public Works, dated Windsor, 10th Feb., 1870, from which I give the following extracts: "Under date Montreal, Feb. 3, 1870, Dr. Hunt informs me that recent microscopic examination of some of the specimens sent by Dr. Honeyman, has revealed *well-defined* '*Eozoon Canadense.*'" "This," continued Dr. Hunt, "must, I conceive, be *conclusive evidence of their Laurentian age.*" "Dr. Dawson, in a separate communication, confirms the identification of the Eozoon."

I find, however, that in the discussion that followed the reading of the paper, "On the Laurentian rocks of Arisaig, N. S.," addressed to the Geological Society of London, Dr. Dawson remarked that the Arisaig Eozoon was different from that of Canada; that the *Eozoon Bohemicum* belonged to a formation more recent than the Laurentian, and that the Arisaig Eozoon did not prove the rocks to be Laurentian. *Vide* printed abstract of the proceedings of the Geological Society of London.

I do not regard the discovery of Eozoon as the only evidence of the Laurentian age of the Arisaig rocks. I consider that *relative* position, in connection with *lithological* character, must have something to do with the determination of their age. Besides, is it not possible that, as there appear to be *new* different species of Eozoon belonging to different countries and different ages, that there may be different species in different countries, but still of the same age, and still that the Nova Scotian species is still Laurentian.

Another extract from Prof. Hind's letter: Dr. Hunt says, in his letter already referred to, "A line from the Laurentian of Malignant Cove (Arisaig) to that in Newfoundland, will pass through Cape Breton, and we can now look for limestone and Eozoon there." Is it not possible that the thought may have occurred to some one else besides Dr. Hunt? Perhaps Dr. Hunt has forgotten something that happened during the Exhibition in Paris of 1867. He came into the Nova Scotian Court, accompanied by Prof. Lesley of Philadelphia; he found among the polished specimens of marble a green one which arrested his attention; he asked where it came from, and was told that it came from Cape Breton, (having been furnished by Mr. Hendry already referred to.) This specimen also attracted the attention of Prof. Wyville Thompson, an Eozoonal unbeliever, who was a member of the International Jury; he said that it had Eozoonal structure; he wished to have it and got it; he took it to London where it made no small stir among the believers in the Eozoon, and gave occasion for a little ingenious explanation on the part of Dr. Hunt; so I was informed.

Another extract from Prof. Hind's letter: "It is thus, that Dr. Honeyman's opinions have been beautifully verified, but it would have greatly enhanced the gratification which Dr. Honeyman must feel if the announcement had been made a year and a half ago."

As might be expected this observation caused some excitement in Gabriel street. In order to rebut the above implied charge, doubtless, Dr. Hunt finds fault with me for not expressing my views, relative to the age of the rocks in question, in my official report. I answer, I studiously avoided expressing any opinion on controverted points in my report, and made it a simple record of facts. As far as I recollect, I mentioned the discovery of the rocks *simpliciter*. It appears that my alleged silence is considered by Dr. Hunt as "*simply incomprehensible.*" I think I can bring the matter to the level of the comprehension of even a less acute person than Dr. Hunt. I have already shown that the term *silence*, if meant literally, is not a proper term to use in the case, as I have not been *silent* on the subject. If by silence he means that I had not addressed a communication on the subject to any scientific Society or Journal, until the publication of Prof. Hind's letter, Dr. Hunt has hit the mark. My defense is, I had not been able on account of the inclemency of the weather and lateness of the season, to examine the rocks in a manner so thorough and satisfactory as was desirable; I did not think there was any great hurry in the matter, as I did anticipate the *great discoveries* of my excellent friend Prof. Hind. I have not yet had the desired opportunity, and yet I have had to write on the subject: first, to the Geological Society of London at the request of Prof. Hind; second, to the American Journal of Science, at the instigation of Dr. Hunt.

2. *Descriptions of Fossils collected by the U. S. Geological Survey under the charge of Clarence King, Esq.* (Proc. Acad. Nat. Sci., Philad., No. 1, 1870).—Mr. MEEK prefaces his numerous descriptions of new species by the following observations, addressed to Prof. Joseph Leidy.

I send herewith, to be presented for publication in the Proceedings of the Academy, descriptions of a few of the fossils brought in by the United States Geological Survey under the direction of Clarence King, Esq. You will please state, in presenting the paper, that the Trilobites described in it from Eastern Nevada, are decidedly Primordial types, and, so far as I know, the first fossils of that age yet brought in from any locality west of the Black Hills. Mr. King's collections also establish the fact that the rich silver mines of the White Pine district occur in Devonian rocks, though the Carboniferous is also well developed there. The Devonian beds of that district yet known by their fossils, seem mainly to belong to the upper part of the system. Mr. King, however, has a few fossils from Pinon Station, Central Nevada, that appear to belong to the horizon of the Upper Helderberg limestone of the New York series.

The Tertiary fossils described in this paper, from the region of Hot Spring Mountains, Idaho, came from an extensive and interesting fresh-water Lacustrine deposit, and are all distinct specifically, and some generically, from all the other Tertiary fossils yet brought from the far west. Two of the species belong to the existing California genus Carinifex, or some closely allied group, while another beautifully sculptured species was thought, by Mr. Tryon, to whom I sent a specimen of it, to be possibly a true *Melania*, and allied to existing Asiatic forms.

It is an interesting fact, that among all of our fresh-water Tertiary shells from this distant internal part of the Continent, neither the beaks of the bivalves, nor the apices of the spire in the univalves, is ever in the slightest degree eroded; even the most delicate markings on these parts being perfectly preserved, if not broken by some accident. From this fact it may be inferred that the waters of the lakes and streams of this region, during the Tertiary epoch, were more or less alkaline, as is the case with many of those there at the present day.

These descriptions, as well as others that I expect to send you soon, are merely preliminary and will be re-written, and presented with full illustrations, now in course of preparation, in Mr. King's report of his survey.

3. *Discovery of a Mastodon.*—On Friday last, Mr. Fletcher Correll, a farmer residing one and one-half miles southeast of Illiopolis, in this county, was digging a well upon his place, when, at the depth of about four feet from the surface, he struck a hard sub-

stance, at first supposed to be a stone or piece of wood imbedded in the earth; but upon digging farther, it was discovered to be the remains of a mastodon.

The bones were in a fair state of preservation, and exhibited a dark, spongy, porous appearance. One of the tusks, which was broken in removing it from its long resting place, proved to be, when measured, nearly ten feet in length, and twenty nine inches in circumference three feet from the lower end. The other tusk, and the main portion of the skeleton, are now being lifted from the earth, and will probably be added to the collection of fossils now being made by our state Geologist, Professor Worthen.

The part of this huge creature which was exhibited in our office in the presence of Professor Worthen, was a piece of the lower left jaw, about two feet in length, and at least the same in circumference. It contained, in a fine state of preservation, one of the great jaw-teeth.—*Daily State Journal, of Springfield, Illinois, Sept. 7. Received from C. L. Conkling.*

4. *Description of the Cavern of Bruniquel, and its organic Contents;* by Professor OWEN. Part I, Human and Equine Remains. 55 pp. 4to, with six plates. (Phil. Trans., 1870; communicated to the Royal Soc., June, 1864).—Professor Owen, after describing the human remains, draws from them the following conclusions:

"They exemplify the distinctive characteristics of the human genus and species, as decidedly as do the corresponding parts of the present races; they show most affinity with the oldest Celtic types, the cranium being oval, and rather dolicho- than brachy-cephalic in its general proportions; the cranial capacity or brain corresponds with that of uneducated Europeans of Celtic origin, and exceeds that of the average Australian aboriginals."

Some of the bones accompanying the human remains—those of the deer especially—are covered with drawings, representing the heads of horses and showing much artistic skill; and from these draughtsmen of the cave-dwelling people, we thus learn that the horse of the era, *Equus spelæus*, had short pointed ears, and that the stallions had beard-like hairs; and from the antler of a reindeer, found in another cavern, that of La Madelaine, in Dordogne, we have the additional fact that the tails of these early horses were short and furnished with long hairs to their base, having "cauda undique setosa," instead of "cauda extremitate setosa," a fact repeated seven times on the antler. The horse is of the same species that occurs in certain quaternary beds in France, for example, those near the Tour de Juvillac, Püy-de-Dome, and belongs to the restricted genus *Equus*, and not to that including the zebra and ass (*Asinus*). Prof. Owen concludes with the following paragraph:

"No satisfactory evidence of an aboriginal feral *Equus caballus* has yet been obtained by the Naturalist. No specimen of such exists in any Museum. The doubts expressed by Forster and Pallas as to the alleged wild horses of the Ukraine, viz: that they might be descendants from strayed domestic horses, have not yet been cleared up. I believe the illustrations contained in the present paper to be the best, if not sole, evidences of the wild originals of some of our domesticated breeds. Like the alleged wild horses of Prussia, those of Aquitaine, in the time of the flint-armed hunters and cave-dwellers, were doubtless "shy and difficult of capture, but very good venison."

The above memoir is followed by another, under the same cover, entitled *Supplementary Remains of Equines from Central and South America referable to* Equus conversidens *Ow.*, Equus tau *Ow.*, and Equus arcidens *Ow.* In this paper Professor Owen, besides discussing the characters of the different kinds of fossil horse in the regions referred to, treats of their relations to the extinct genera *Macrauchenia*, *Nesodon*, and *Toxodon*, and illustrates the subject with numerous figures on six lithographic plates. He observes that according to the facts made out, the most ancient Equines of the South American continent present modifications of the upper molars, and more especially of the lower molars, which supply an additional link between the Equines with other perissodactyle Ungulates, and the group including *Toxodon* and the other genera above mentioned.

5. *The North American Lakes as Chronometers of Post-glacial time.*—Dr. E. Andrews, the author of this paper, noticed at p. 264 of this volume, states, in a recent letter, that his method of calculation eliminates the error alluded to in the notice, due to the wearing away of the sands. He says that in the calculation, "the first element is the age of the lower beach, which is obtained by ascertaining the age of its cotemporary terrace of erosion; this settles the approximate age of the lower beach without liability to error from attrition of sand. The age of the upper beaches is deduced by comparison of their relative masses with the lower. Now as the attrition was going on at about the same rate at all ages, the relative loss by attrition would be strictly proportioned in all the beaches, and then so far as time is concerned would be unchanged."

6. *Descriptions of new Fossil Shells of the Upper Amazon;* by T. A. Conrad, (Am. J. of Conch., Oct. 10, 1870).—Mr. Conrad describes several species of shells from the deposit at Pebas, on the Amazon, visited by Prof. Orton, (see p. 294 of this volume). They include a number of species of *Pachydon*, Gabb, and a bivalve near *Mulleria*, besides gasteropods of the genera *Isaa* Conrad, *Hemisinus*, *Dyris* Conrad, *Neritina*, *Bulimus*. The Bulimus is the only land shell in the collection. The species "may have lived either in fresh or brackish water, but are certainly not of marine origin." They appear to be all extinct, and if so, they "cannot be later than the Tertiary."

7. *Restoration of a Flying Dragon* (Dimorphodon macronyx *O.*) *from fossil remains in the Lias-cliffs of Dorsetshire;* by Prof. Owen, F.R.S. (Pages 463–502 of Pal. Soc. volume for 1869). With two plates. London, 1870.—Prof. Owen's memoir contains a full and detailed account of this remarkable Pterosaur, and illustrates it by admirable lithographs. He does not agree with Mr. H. G. Seeley with regard to the relations of Pterosaurs to Birds.

8. *Remarks on Prof. Owen's Monograph on Dimorphodon;* by H. G. Seeley, F.G.S., (from the Ann. Mag. Nat. Hist., Aug., 1870. 24 pp.)—Mr. Seeley in this paper presents his objections to the views of Prof. Owen on the Dimorphodon.

9. *Geological Charts.*—The second edition of von Dechen's general geological chart of Germany, France and England and the adjoining countries, was published at Berlin in 1869, and a geological chart of Germany by the same able author in 1870.

The Royal "Comitato Geologico," of Italy have announced that they will publish, the coming year, a small geological map of Italy.

10. *Carte Géologique du versant Occidental de L'Oural;* by Valérien de Möller. 1869. Scale $\frac{1}{840.000}$. A beautiful colored geological chart of the Western or European slope of the Urals.

11. *Second Annual Report upon the Geology and Mineralogy of the State of New Hampshire;* by C. H. Hitchcock, Prof. Geol. and Min. in Dartmouth College. 38 pp. 8vo. Manchester, N. H. 1870.

III. BOTANY AND ZOOLOGY.

1. *Martius, Flora Brasiliensis:* fasc. xlix, *Cyatheaceæ et Polypodiaceæ.* Exposuit J. G. Baker.—The other suborders of Filices were worked up by J. W. Sturm, and were published in 1859. To illustrate the different ways in which these two fern-authors regard species, one may look at the genus *Aneimia*, of which Mr. Baker in the "Synopsis Filicum" recognizes but twenty-six species for the whole world, while Sturm describes not less than thirty-one species peculiar to Brazil, besides thirteen common to Brazil and some other countries, and five more not yet reported from Brazil, but likely to be found there. Mr. Baker seems to diminish the number of species at every step; many that were recognized in the "Synopsis" being reduced to synonyms in the work now under consideration. For instance, *Lomaria L'Herminieri* and *L. lanceolata*, placed in different groups in the Synopsis, are now made into one species. So, too, *Blechnum Fendleri* is joined to *B. longifolium*, and *Polypodium* (*Phegopteris*) *rigidum* to *Aspidium aculeatum*. *Gymnogramme calomelanos* swallows up *G. tartarea*, having long ago digested *G. chrysophylla*, *Massoni*, *Peruviana*, &c., and many other such instances might easily be pointed out. But all this reduction seems to have been done only after patient study over many specimens, and it is gratifying to see this emphatic protest against the flood of new species with which we are deluged by continental authors.* The author's industry and re-

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* The publication in Linnæa by Dr. Kuhn of manuscript names left by the late Dr. Mettenius. many of which the maturer judgment of the latter would doubtless have withheld, gives to Botany several hundred "new species;"—a few examples of them from one genus, by no means a large one. are subjoined.—*Pellæa intermedia*, founded on Mr. Wright's New Mexican No. 825, is said to be "perhaps a hybrid between *P. sagittata* and *P. flexuosa*," forms which in all probability both belong to *P. cordata; Pellæa myrtillifolia*, is the Chilian form of *P. andromedæfolia; Pellæa microphylla* is the new name for the New Mexican specimens of *P. pulchella*, and *Pellæa glabella* rests upon St. Louis and Wisconsin specimens of *P. atropurpurea*, with an unusually glabrous stipe and rhachis.

search have unearthed a few long buried and forgotten names for well known ferns:—*Pteris geraniæfolia* Raddi (1825) was found by Dr. Kuhn to be the same as *Pteris Pohliana* of Presl. (1822), and now we must go back twelve years more to *Pteris concolor* Langsdorff and Fisher (1810). Can any one find a yet more ancient name for it? It is gratifying to see, in a note, that the African *Cheilanthes Kirkii* is now confessed to be probably an adiantopsoid variety of this same species, the only difference being in the interrupted instead of continuous involucre.

Synonymy given by Mr. Baker is ample without being profuse: the characters are well drawn up, and the descriptions clear. There are several pages of letter press devoted to the geographical distribution of Brazilian Cytheaceæ and Polypodiaceæ. If the orders elaborated by Dr. Sturm could have been included in the notes and tables relating to geographical distribution, it would have been very acceptable to Pteridologists, but, as Mr. Baker remarks, the former suborders were written under such a very different theory of species that they could not be joined with the present ones in any kind of synoptical table. There are thirty-eight plates of Nature-printed fragments of Fern-fronds, borrowed from Ettingshausen, and thirty-two more cut on stone, from drawings by Fitch, Hipsley and Kunze, with a single one done under the eye of Dr. Eichler at Munich. D. C. E.

Oxford Botanists is the theme of a discourse pronounced by Prof. Lawson,—the successor in the professorial chair of the worthies he commemorates,—at the recent Congress held at Oxford by the Royal Horticultural Society. The history begins with the foundation of the Botanic Garden by Henry Earl of Danby, in 1632, and with the installation of *Jacob Bobart*, the first curator. It gives a biography of *Dr. Morison* the first professor, appointed in 1669; of his successor, *Jacob Bobart*, the second; of *Dr. Sherard*, or "Sherwood" who re-endowed the chair, and of *Dillenius* whose appointment he provided for in his will; of *Dr. Humphrey Sibthorp*, the next incumbent, and his distinguished son, *Dr. John Sibthorp*, who succeeded his father in 1784, and died at the age of 38 in 1796, bequeathing new endowments and the foundation of the associated chair of Rural Economy, after providing for the publication of the sumptuous *Flora Græca*. His successor *Dr. Williams* is mentioned; and an interesting notice of the late incumbent and patron, *Dr. Daubeny*, brings down the history to the present time.

Dr. Hasskarl, who has long been studying the *Commelynaceæ*, has just issued a monograph of the Indian species, especially of the Archipelago, with occasional genera and species of other parts of the world; an octavo of 182 pages, published at Vienna. Even in this partial representation of the order over forty genera are admitted! Some valuable criticisms of Dr. Hasskarl's work in this order will be found in a paper by Mr. Clarke, mentioned below.

The Journal of the Linnean Society, Nos. 54, 55, issued together in September of this year. The first and the most important, as well as the longest, article is

A Revision of the Genera and Species of Herbaceous Capsular Gamophyllous Liliaceæ, by J. G. BAKER, F.L.S., &c. The adjectives here employed distinctly limit the portions of the great Liliaceous family, to which Mr. Baker here devotes himself. That he intends to go through the order in this way seems probable, and it is desirable. In treating incidentally of the limitation of *Liliaceæ*, he refers without qualification, to "characters which are universally regarded as of ordinal value, in the extrorse anthers, separated styles and septicidally dehiscent capsules of *Colchicaceæ*," apparently under the impression that these characters and their converse, run harmoniously parallel throughout. The exceptions, however, which have already been noted and placed upon record, and the way in which some of these opposed characters blend, may upon consideration show that so complete a separation is hardly practicable; and indeed the attempt has been abandoned by at least three botanists already, two of them authors of British Floras, and of whose views Mr. Baker would be likely to be particularly well-informed. *Helonias* and *Chamælirium*, on one hand, are loculicidal in dehiscence, while some near Japanese relatives of Helonias have united styles, and *Tofieldia* has introrse anthers. Then the two elements which constitute extrorsion or introrsion of the anthers may not coincide. The anthers of *Uvularia*, being adnate, are plainly extrorse, yet the lines of dehiscence are almost exactly lateral. If, then, we regard the fixation rather than the lines of dehiscence as the more essential, *Lilium* itself comes into the category of extrorse anthers (as first noticed, long ago, by Dr. Chapman), for its anthers are extrorsely attached although slightly introrsely dehiscent. *Medeola* is in the same case, while *Trillium* is nearly as in *Uvularia*. Mr. Baker brings the plants he deals with under 27 genera, including our *Hesperocallis*, which had escaped notice when the Clavis was printed. As to this very rare and little-known plant, he expresses a natural doubt as to its generic distinctness from *Hemerocallis*, which inhabits a widely distant part of the world. We are now able to state that, like *Hemerocallis*, the blossoms open in the morning and close at night. This is on the authority of Mr. Schott, who proves to have been the first discoverer of the plant. Moreover, which is of more consequence, he collected and has supplied us with the "bulb." It really is a *tunicated bulb*; and so the genus falls out of the tribe *Hemerocallideæ*, and seemingly into Mr. Baker's *Odontostemoneæ*, for which we would like to have Mr. Baker substitute the name of *Hesperocallideæ*. Our further interest in this revision, as regards N. American Botany, centers in *Brodiæa* and its allies. In this genus he (as well as Dr. Hooker, in Bot. Mag.) retains the remarkable *Brevoortia* of Mr. Wood, which, it appears, was first collected by the late Mr. Lobb; and he follows up the idea which this species suggested by reducing Torrey's *Stropholirion* likewise to *Brodiæa*. On the other hand he departs from Bentham's view, and refers all the hexandrous species, and we suppose justly, to *Milla*, which swallows up *Triteleia*, *Calliprora*, and even *Hesperoscordum*. The latter name is accidentally printed *Hesperocordium* in the conspectus; and further on, *Hesperoscordium.* The latter is perhaps an intended (and a laudable) innovation, as it is hard to pronounce the original name, as must needs be, with the accent on the penult.

On the Commelynaceæ of Bengal, by C. B. Clarke; another paper in this fasciculus of no small importance for the correct understanding of the characters of these difficult plants.

Notes on some Algæ found in the North Atlantic Ocean, by Dr. Dickie. The results of the examination of the contents of a bottle which was filled in the midst of the Atlantic, lat. 12° N., long. 21° 40′ W. There Capt. Mitchell, on the 24th Nov. 1867, found a patch of sea, 12 or 14 miles across, closely studded with a green substance, some of which he gathered. It is thought to have been wafted there from the American side by the Gulf-stream and the African current. The principal interest to us is in the following statement:—

"The substance thus picked up by Capt. Mitchell was in excellent condition, and, besides three *Algæ*, contained numerous fragments, more or less decayed, of wood, both endogenous and exogenous; *seedling plants several inches long, all with a pair of cotyledons, roots, and terminal bud, quite fresh;* small fruits partially decayed, evidently one-seeded legumes; intermixed were various microscopical crustacea, and a common oceanic insect, one of the *Hydrodromideæ*, genus *Halobates;* on some of the pieces of drift wood were numerous elliptical ova, of a deep orange color, mixed with which was growing the smallest of the three Algæ now to be described."

Leaves do not absorb moisture from the atmosphere in the state of vapor, according to a series of elaborate experiments by Unger, and afterward by Duchartre,—some of which were published several years ago,—confirmed recently by those of Prillieux.

The latter reaches this conclusion upon ascertaining that when a leafy branch, flaccid from evaporation, is suspended in moist air and recovers its freshness—as every one knows it promptly does—it does not gain in weight, but even loses. The same occurs with leaves furnished with petiole, the cut end sealed with wax. The conclusion is that the moisture by which the foliage is freshened comes not from the surrounding air but from the lower part of the branch, or from the leaf-stalk, the upper and foliaceous parts gaining at the expense of the lower. To test this, let the experiment be tried with single leaves destitute of leaf-stalk. Duchartre made a portion of his experiments with epiphytes, both Orchids and *Bromeliaceæ*, and concluded that the same applied to the aerial roots as well as the foliage. There was no gain in weight except when liquid water reached the leaves, roots, or other portion of the surface, when absorption was promptly indicated by increase in weight: and it appeared that this absorption of liquid water hardly took place by the leaves unless these were immersed or kept long and thoroughly wet. This again corresponds with some older experiments which went to show that syringed foliage did not absorb the liquid. As to aqueous vapor, there being free communication by the stomata between the air outside of the leaf and that within its intercellular spaces and passages, and the thin walls of the parenchyma-cells freely allowing vapor of water to pass out, it is very difficult to believe that it will not equally pass inward under appropriate conditions. Hence we cannot yet regard the question as concluded. The editor of the Gardeners' Chronicle (Sept. 17) pertinently suggests that "it does not appear to have been remembered that the density of aqueous vapor is very much less than that of dry air. If the latter be estimated at 1000, that of aqueous vapor is 625 only,—a circumstance that ought to be taken into consideration in such experiments." But if leaves continue so to absorb aqueous vapor, they ought to condense or appropriate it, along with carbonic acid gas, and so increase manifestly in weight.

An Elementary Course of Botany, Structural, Physiological and Systematic, by the late Prof. Henfrey, which was originally published (by Van Voorst) in 1857, is this year reproduced in a second edition, "revised and in part rewritten" by Dr. Maxwell T. Masters, F.R.S., &c.—The death of Prof. Henfrey, while still a young man, was a sad loss to physiological botany in Great Britain. As a vegetable anatomist he was becoming truly eminent. Our estimate of the value of this, his last work—on the whole very favorable—was recorded at the time in a somewhat extended review of it in the number of this Journal for November, 1857 (vol. xxiv, II, p. 434–440.)

The preparation of a second edition could hardly have been consigned to better hands than those of Dr. Masters, who has evinced special aptitude for morphological work, which was not Prof. Henfrey's *forte*. The alterations and additions by the present editor are not specially indicated: those which we have discovered are well adapted to bring the work up to the time, and to render it a valuable text-book. But we are bound to maintain that it might have been more thoroughly "revised and rewritten" to great advantage. It could hardly be expected that criticisms made a dozen years ago, and on this side of the Atlantic, would come under the notice of a recent editor, yet it is curious to observe that, out of a series of more than twenty misapprehensions or errors in detail (individually of small moment, no doubt), which were indicated in this Journal, chiefly in the morphological part, all but two (and one of these a grammatical slip) stand unaltered in the new edition. The sole alteration is in the statement which attributed axillary tendrils to the Vine. We do not include some criticisms of another order, relating to mere theoretical points, upon which views which have long prevailed are retained;—such, for instance, as that which unquestioningly regards the so-called radicle of the embryo as root, although in one part of the work its nature as hypocotyledonary stem is implied in a particular instance. That this part of the embryo represents the first joint of stem is a doctrine which seems to be ignored in England, but generally accepted out of it. A. G.

2. *Anatomisch-Systematische Beschreibung der Alcyonarien. Erste Abtheilung; die Pennatuliden, erste Hälfte*, von A. Kölliker. Quarto, with ten plates. Frankfort, 1870.—There are very few groups in the animal kingdom which have been more carefully studied, during several years past, than the Halcyonoid Polyps, or concerning which our information has more rapidly increased, both in respect to their structure and the number and variety of their forms. The Pennatulacea, especially, have been carefully studied and described by several able naturalists, and the number of species has been increased to an extent that is truly surprising, considering the very few species known a dozen years ago, and their comparative rarity. Numerous species and genera were made known in the works of Herklots, Bleeker, Richiardi, and others. But in this work Prof. Kölliker has undertaken a complete revision, both anatomical and descriptive, of the entire group, and in the part already published has added a great number of new and interesting forms. His work is of greater interest from the fact that he has been able to examine the collections in nearly all the principal museums of Europe, including the types of most previous writers.

This first part includes an anatomical description of the whole group, with an analytical table of the classification adopted, while most of the volume is devoted to very careful and elaborate anatomical descriptions of the genera: *Pteroeides*, *Godeffroyia* (nov.), *Sarcophyllum*, *Pennatula*, *Leioptilum*, *Ptilosarcus* and *Halisceptrum*, and their included species, of which a large proportion are admirably illustrated. In the genus *Pteroeides* 43 species are described, of which 29 are regarded as new, although a few of them are probably identical with some of those described in the nearly contemporary work of Richiardi, (we have not learned the precise date of either). Numerous varieties of some of the species are also named and carefully described. The polymorphic forms of the polyps in all the genera and species are thoroughly discussed

The following is the classification adopted:

First Tribe, Pennatuleæ; Second Tribe, Renillaceæ; Third Tribe, Veretillidæ.

Tribe, Pennatuleæ.

1st Family, Penniformes.

1st Subfamily, Pteroidinæ, including the genera *Pteroeides* Herkl., *Godeffroyia* Köll., *Sarcophyllum* Köll.

2nd Sub-family, Pennatulinæ, including *Pennatula* L., *Leioptillum* Verrill, *Ptilosarcus* Gray, *Halisceptrum* Herkl.

2nd Family, Virgularieæ, including *Virgularia* Lam., *Stylatula* Verrill, *Pavonaria* Köll., *Scytalium* Herkl., *Funiculina* Lam., *Halipteris* Köll. v.

3. *The External and Internal Parasites of Man and Domestic Animals*; by A. E. Verrill. 140 pp. 8vo, 85 wood-cuts. (From the Report of the Connecticut Board of Agriculture), Hartford, 1870.—The first part of this work is devoted to the external parasites and contains descriptions of the various species of fleas, lice,

ticks, bot-flies, bed-bugs, mites, and acari, often troublesome to our domestic animals, as well as to man himself, together with accounts of their habits, effects, and remedies. Several pages are also devoted to introductory remarks on the structure of insects and their classification.

The second part is devoted to the internal parasites, and contains a general account of the parasitic worms, and their classification, a series of lists of those found respectively in man, the dog, cat, sheep, cattle, horse, hog, and poultry; and detailed descriptions of the forms, anatomy, metamorphoses, habits, effects, and remedies of some of the most important species, many of which are illustrated by wood-cuts. Among those most fully discussed are the *Trichina spiralis;* the pork and beef tape-worms of man; the *Tœnia cœnurus* or water-brain of sheep; the *T. echinococcus* or hydatids of man, sheep, etc.; the *Fasciola hepatica* or liver-fluke of sheep and cattle; the *Syngamus trachealis* or gape-worm of chickens, etc., and many others of importance.

4. *On the Eared Seals* (*Otariadæ*), *with detailed descriptions of the North Pacific species;* by J. A. ALLEN. *Together with an account of the Habits of the Northern Fur Seal* (*Callorhinus ursinus*); by CHARLES BRYANT. 108 pp. 8vo, with three plates. From the Bulletin of the Museum of Comparative Zoology, vol. ii, no. 1. Cambridge, Mass., August, 1870.—This is a very interesting and important contribution to our knowledge of the seals of the Pacific and Southern oceans. It includes a résumé of the recent literature of the subject; a chapter on the affinities, distinctive characters, variations due to age and sex, etc., and a conspectus of the genera and species; detailed descriptions of *Eumetopias Stelleri* Peters, based on two perfect skins and two ligamentary skeletons; of *Zalophus Gillespii* Gill, based on two skulls: and of *Callorhinus ursinus* Gray, based on six skins and four complete and two incomplete skeletons, representing both sexes and the young and old. Mr. Bryant contributes a very full and interesting account of the habits of the last species, with descriptions of the islands that they inhabit, the modes of capture, etc. Much information, some of it new, is also given from various other sources, concerning the habits of this and the other species. The geographical distribution of the family is also discussed on page 42.*

The plates and several wood-cuts are devoted chiefly to the skulls and teeth. V.

5. *Record of American Entomoloy for the year* 1869. Edited by A. S. PACKARD, Jr., M.D. Naturalist's Book Agency, Salem, Mass.—This, like the Record for 1868, is a work that should be in the hands of every entomologist who hopes to keep up with the

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* It is here stated that "the habitat of no species, so far as certainly known, quite reaches the tropics," but a skull of *Otaria jubata* in the Harvard Anatomical Museum labeled as from "Arica, Peru." is mentioned, with a doubt as to the correctness of the locality. I may add that in the Museum of Yale College there is a large adult skull of the same species, collected by Prof. F. H. Bradley much farther north, at Zorritos, Peru (about lat. 5° S.) where the marine fauna is eminently tropical and nearly identical with that of Panama.—A. E. V.

progress of the science. It contains references to the writings of fifty-two authors, with indications of the species referred to, including 335 described during the year as new, in American works.

6. *Antero-posterior Symmetry, with special reference to the Muscles of the Limbs;* by Elliott Coues, A.M., M.D., Ph.D.—Dr. Coues takes up the idea, well illustrated by Dr. Jeffries Wyman, of antero-posterior polarity in the animal system, and points out the facts sustaining it in the Muscles of the Limbs. His valuable discussion of the subject runs through the several recent numbers of "The Medical Record," (published in New York), from June 1 to September 1; Article v, in the series, is contained in the last mentioned number.

7. *On the Hypothesis of Evolution;* by Edward D. Cope. (From Lippincott's Magazine, for July, Aug. and Sept., 1870.)—Prof. Cope has brought an extensive knowledge of science to bear on the question of evolution in animal life, and presents some original views. The subject is too large a one for discussion in this place.

IV. ASTRONOMY.

1. *Elements of the new planet Ate;* by Dr. C. H. F. Peters. (Communicated by the author in a letter dated, Litchfield Observatory of Hamilton College, Clinton, N. Y., October 23d, 1870.—The following elements of the planet *Ate* (111) were derived from intervals of 18 days, viz: from observations of Aug. 14, Sept. 1 and 19:—

Epoch: 1870. Sept. 0·0. Berlin mean time.

$$\begin{array}{l}
M_\circ = 205^\circ\ 17'\ 21''\cdot0\\
\left.\begin{array}{rrrr} \pi = & 122 & 53 & 7\cdot3\\ \Omega = & 306 & 26 & 28\cdot4\\ i = & 5 & 1 & 21\cdot4 \end{array}\right\}\text{Mean Equ. }1870\cdot0.\\
\varphi = \ \ 5\ \ 49\ \ 10\cdot6\\
\mu = 858''\cdot392\\
\log a = 0\cdot4108808.
\end{array}$$

They represent an observation obtained last night still within a few seconds.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On the Chemistry of the Bessemer process.*—The following is an abstract of the paper on the Chemistry of the Bessemer process, read before the American Association for the Advancement of Science, at its Troy meeting, by Lieut. C. E. Dutton, U. S. A.

After some general statements concerning the chemical composition of crude pig iron, Lieutenant Dutton passed to discuss the theoretical changes possible when air is brought in contact with it in a melted state. According to his view, the silicon is first oxydized, then the phosphorus, next the manganese and sulphur, and lastly, the carbon. Chemically, there is nothing new in the Bessemer process; it may be said to be "the employment of entirely new mechanical methods and appliances for effecting old and familiar reactions." The process itself was then minutely described. Since the sulphur is but partially removed and the phosphorus not at all, the iron selected must be free from these substances. It must also contain at least two per cent of silicon. This iron is melted in a cupola furnace, run into the converter—a charge being 12,000 pounds—and the blast turned on. "The first change is in the oxydation of iron and silicon. The silicon becomes silicic acid and enough of the iron oxydizes to satisfy the affinities of the acid and does not decompose during the remainder of the blast. It is during this stage of the conversion that the remarkable heat of the conversion is developed. It will be remembered that when silicon oxydizes, it takes up three equivalents of oxygen. Carbon takes up only one in this process, becoming carbonic oxyd. It is a common error to suppose that any very great quantity of heat is generated by the combustion of the carbon,—that is, as compared with that derived from the silicon. * * * The heat generated by the silicon burning to silicic acid will be found by the application of the coefficients and formulæ of the mechanical theory of heat, to be from two and one-half to three times greater than that generated by the burning of the carbon to carbonic oxyd. Another circumstance of importance is that the silicic acid remains as a dense fluid in the converter, no part of its heat being lost, except such as is carried out of the converter by the atmospheric nitrogen; and none is rendered latent by converting it into vapor; while the carbon is vaporized, a physical change absorbing much heat, and the vapor thus formed is carried out of the converter at a very high temperature. Hence will be seen the necessity of employing irons containing high percentages of silicon. At least two per cent of this element is essential, any less quantity being insufficient to generate heat enough to keep the iron thoroughly liquid and fluent until the end of the casting process. It is often asserted that irons for Bessemer conversion must be 'gray irons,' as they are called; i. e., irons rich in carbon. Now, although this happens as a rule, to be true enough, it is apt to lead to misapprehension. The fact that Bessemer pig irons are carbonized varieties is an accident, and not an essential feature. What is essential is that it should contain a large quantity of silicon, and very little—indeed, the least possible—of sulphur, phosphorus, and manganese. Now, a pig-iron containing much silicon and no sulphur or manganese, is pretty sure to contain a high percentage of carbon, as all smelters are aware. This fact is a feature of the blast-furnace, and almost without exception. If an iron could be produced with much silicon, a little carbon, no phosphorus, it would I think, be not altogether unsuited to the Bessemer treatment. In a word, the quantity of carbon is approximately immaterial, except so far as it implies proper conditions with respect to other elements. The main element required is the silicon and not the carbon." Of phosphorus, "the arch-enemy of the iron-maker, but the very scourge and pestilence of the steel-maker,"—fifteen-thousandths of one per cent ruining Bessemer metal past all remedy,—Lieut. Dutton said: "I have already ventured the opinion that phosphorus increases its affinity for iron with every increase of heat; at least relatively if not absolutely. The fact seems to be, absolutely. If we accept it, we can instantly explain what seem, otherwise, to be many anomalies and contradictions. It will explain to us why, in the great heat of the blast-furnace, it leaves every other combination and enters the iron; why, in the much lower heat of the puddling furnace it seems to waver between staying with the iron, or forming a new alliance with oxygen, ready to choose either, at the influence of any third substance which may affect the question; why in the Bessemer process it clings to the iron with a desperate tenacity which nothing seems able to resolve. These three facts, then, are all of them formidable: 1st. That a minute quantity of phosphorus is capable of working terrible injury, and that it is omnipresent throughout nature; 2d. That whatever quantities of it are charged into the blast-furnace, as fuel, flux, or ore, are almost wholly concentrated in the resulting pig-iron; and 3d. That no portion is eliminated in the Bessemer converter." The sudden change of the flame at the close of the conversion Lieut. Dutton thus explained: "When two combustibles are intermixed, like oxygen and hydrogen, or hydrocarbon gas, it is well known that the relative proportion of the two elements in the mixture influences the readiness with which they combine. Thus oxygen and hydrogen cannot combine explosively unless their proportions lie within certain definite and rather narrow limits. May not the same law hold good in the present case? It is certain, or nearly certain, that the iron either does not oxydize in the bath during the blow, except in quantity sufficient to furnish a base for the acids present; or, if it oxydizes beyond that, it is immediately reduced again, leaving little or no free oxyd of iron in the bath. But after the change of flame all this is reversed and iron oxydizes rapidly and freely, and remains undecomposed, while the residual traces of the other elements as suddenly cease to oxydize rapidly. I freely grant that in referring this back to what is supposed to be a conceded, but unexplained law, we are merely putting the question in another, a more general, and more abstract shape:—still it is, in a qualified sense, an explanation." The theory of the action of the spiegeleisen, run in after the blast is stopped is next discussed, and also the quality of the metal produced, which Lieut. D. calls a "cast wrought-iron." The paper closed with some comparisons of Bessemer with other metal, and a discussion of the uses for which Bessemer metal is most valuable. G. F. B.

2. *Earthquake of October 20th*, 1870.—On Thursday morning, Oct. 20th, an earthquake vibration was felt throughout Canada, and the northern part of the United States from Maine to Iowa. It seems to have been more severe in Canada and in New England. In many places the shock was sufficient to throw down chimnies, crack the walls of buildings, and do other damage. It was remarkably severe for the region of country visited.

At New Haven, as well as in many other places, there were two distinct series of vibrations. Prof. Twining has carefully collected information from several persons as to the time of the occurrence and duration of the vibrations. The beginning of the first shock was at $11^h\ 19^m\ 45^s$ A. M. New Haven mean time. It lasted ten seconds, and its individual vibrations were about two-thirds of a second in duration, or one and one-third second for a complete double vibration. After an interval of five seconds there was a second series like the first, lasting eleven seconds.

The motion was not a simple oscillation but there was a rocking motion, indicating a vertical component in the movement of the earth. The vibrations were not severe enough to arrest universal attention, though multitudes felt a peculiar sensation without recognizing the cause. The direction of the vibration was N.N.E. and S.S.W.

At Cambridge, Mass., according to Prof. Winlock, the direction was about 10° north of east, as determined by the appearance of the sides of a vessel containing milk.

Mr. Farmer, at Boston, gives $11^h\ 25^m\ 37^s$ for the time of the ending of the vibrations, Cambridge mean time. This would imply that the shock reached Boston a minute and three-fourths earlier than New Haven.

At Cleveland, Ohio, several clocks were stopped by the earthquake, each indicating very nearly $10^h\ 45^m$ A. M. This is approximately the instant at which the shock reached New Haven.

It is reported that the shock reached Quebec 30 seconds before it did Montreal. the telegraph operator of the former city being in the act of inquiring of the operator in the latter one respecting the earthquake when it arrived at Montreal. These data seem to show that the general progress of the wave was from North to South.

Slight vibrations were felt as far south as Richmond, Va., and as far west as Dubuque, Iowa. H. A. N.

3. *Kansas Natural History Society.*—(From the Secretary of the Society.)—The third annual meeting of this Society was held in Lawrence, Kansas, the first week in September, in the University Building. Gen. John Fraser, President of the State University, delivered a lecture, the first evening of the session, "On the Aims, Organization and Advantages of Scientific Associations," in which he recommended an enlargement of the scope of the Society to cover the whole field of scientific observation and investigation. On the following evening Rev. John H. Barrows, of Arvonia, delivered an address in the Presbyterian church on "Hugh Miller, or The Workingman's Education." At the close of the lecture President Fraser, who spent his youth in Cromarty, gave some personal recollections of the great geologist.

The following papers were read before the Society: "A Catalogue of all the Plants of Kansas, as far as observed," by Prof. J. H. Carruth, of Lawrence. The catalogue contains about 500 species—30 being added by members of the association. "On the Fishes of the Kansas River, as observed at Lawrence," by Prof.

F. H. Snow, of Lawrence. This paper describes 27 species of fish. "On the Internal Heat of the Earth," by Rev. John D. Parker, of Burlington. "On a Comparison of the Coals of Kansas with other Western Coals," by Prof. Wm. H. Saunders, M.D., of Lawrence. "On the Saurian Formation and Moss Agate of Kansas," by Prof. B. F. Mudge, of Manhattan. Prof. Mudge exhibited to the Society some fine specimens gathered during his recent visit to the Rocky Mountains, among which was a fine, well-preserved Saurian.

Officers of the Society for the current year: Gen. John Fraser, President; B. F. Mudge, Vice President; John D. Parker, Secretary and Librarian; Frank H. Snow, Treasurer; B. F. Mudge and F. H. Snow, Curators. Curators of Departments: B. F. Mudge, Geology; F. H. Snow, Entomology; Wm. H. Saunders, Chemistry; John D. Parker, Meterology; J. H. Carruth, Botany.

4. *Corrections of errata in the "Notes on the structure of the Crinoidea, &c."*—In page 226, in the 20th line from the top, for "*Spœnonites*," read "*Sphœronites;*" page 228, in the 5th line, for "Miller," read Müller;" page 230, in the 17th line for "ovo-anal," read "oro-anal;" in page 232, for "Crinoidea," read "paleozoic Crinoidea;" and in page 233, 11th line, for "interradial," read "internal." I desire also to express my obligations to Prof. Alexander Winchell, State Geologist of Michigan, and to Mr. Charles Wachsmuth of Burlington, Iowa, for their kind assistance in lending me a large number of beautifully preserved specimens.

E. BILLINGS.

OBITUARY.

CAPT. JAMES PEDERSEN, whose name is associated with numerous new and rare species of corals, echinoderms, shells, etc., described in this Journal and elsewhere, as well as with the very extensive collections which he has contributed to the Museum of Yale College. and of which portions have been distributed to many other Museums, both in this country and in Europe, died in San Francisco, Aug. 19th. He was born at Christiania, Norway, February 24th, 1811. His father, Sars Pedersen, and his ancestors for several generations, were sea-faring men, and he seems to have early developed the same taste. In 1822, and for several years subsequently, he sailed in his father's vessels between Christiania and Copenhagen, and by his visits to the celebrated museums and galleries of the latter city, he acquired a taste for the rare and curious in nature and art, which in later years led him to become an enthusiastic and intelligent collector of objects of Natural History. In 1828–9 he studied navigation and took his diploma, and afterward sailed to St. Petersburg. In 1830 he came to this country, and subsequently made New York his home. He sailed for 19 years between New York and Havana, New Orleans, Vera Cruz, Honduras, etc., but in 1849 he took his vessel to California, and since then has spent most of his time in various parts of the Pacific and Indian Oceans, being for some time on the South American coast, and some years in China and Japan. The last years of his life were spent at La Paz, Lower California, where he engaged to a considerable extent, in the pearl fishery, and from whence he sent the extensive zoölogical collections which adorn the Museum of Yale College. These collections are no doubt the largest that have ever been made in the Gulf of California, and when fully described will add greatly to the knowledge of that rich fauna. He was obliged to leave La Paz last December, on account of failing health and the excessive heat, but after his arrival in San Francisco, was still mindful of the interests of science, and continued to send valuable collections as often as he was able. He was a thoroughly temperate, Christian man, who has done much in his humble way, to advance science. His collections will be an enduring monument to his memory.

Dr. W. A. **Miller**, the author of "Miller's Elements of Chemistry," and of various memoirs on chemical subjects published in the Philosophical Transactions and elsewhere, died at Liverpool, of apoplexy, on the 30th of September. He was born at Ipswich in December, 1817. In 1861 he was elected Treasurer of the Royal Society. He occupied for some years the office of President of the Chemical Society, and was one of the assayers to the Royal Mint.

Dr. A. **Mattheissen**, a chemist of high promise, and one of the examiners of the University of London, died recently, in his 39th year.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Transactions of the Connecticut Academy of Arts and Sciences*, Vol. II, Part I. New Haven, August, 1870. 210 pp. 8vo, 7 lithog. plates. Price $3.—This number includes: Notice of the Crustacea collected by Prof. C. F. Hartt on the coast of Brazil in 1867, by Sidney I. Smith; On the Geology of the New Haven Region, with special reference to the origin of some of its topographical features, by James D. Dana; Notes on American Crustacea, No. 1, Ocypodoidea, by S. I. Smith; On some alleged specimens of Indian Onomatopæia, by J. H. Trumbull; On the Molluscan fauna of the later Tertiary of Peru, by E. T. Nelson. Several of these papers have already been noticed when the author's copies have been distributed. The last, illustrated by two plates, is of special interest as throwing some light on the ancient marine fauna of the Pacific coast of Central America, and its relations to that of the existing faunæ of the same coast and of the Atlantic coast, thus aiding in solving the question of a connection, more or less ancient, between the two oceans across the Isthmus. In this paper numerous new species are described from a formation very rich in fossils near Zorritos, Peru. The formation is probably of Miocene or even earlier age, since only one or two species appear identical with living forms, but the genera are nearly all identical with the common genera now living on the same coast, and the species are often representative of or analogous to living forms of the same region, being apparently much more nearly related to the existing species of Panama, than to either the Miocene or living species of the West Indies. V.

2. *A Treatise on Elementary Geometry;* by Prof. WILLIAM CHAUVENET. Lippincott & Co., Philadelphia.—The reputation of Prof. Chauvenet in science, and the knowledge of his experience as a teacher, will draw peculiar attention to this work upon Elementary Geometry.

In the methods of demonstration Prof. Chauvenet resembles Legendre more than Euclid, though he differs widely from both. He uses the *reductio ad absurdum* rarely, and the various modes of superposition quite extensively. In his treatment of ratios he uses algebraic symbols. The difficulties of incommensurable ratios are met by the early introduction of the idea and theory of limits. He does not adopt the *direction* theory of parallels, believing that it does not meet all difficulties, especially in solid geometry. He introduces the idea of *loci* early in the first Book. To *symmetrical* figures he gives special place and importance. He adds two appendices, the first containing exercises upon the several books, and the second an introduction to modern geometry.

As a preparation for the study of the theory of transversals and other branches of modern Geometry, the introduction of algebraic symbols in elementary demonstrations is of great value. The modern Geometry has greater power than that of Euclid for two reasons: first, it uses the *plus* and *minus* of algebra; and second, it implicitly uses the roots of quadratic equations, real and imaginary.

On the other hand, the use of algebraic symbols in elementary geometry, has, we believe, a disadvantage. The statement of the steps of the syllogism is not so lucid.

The introduction of the idea of limits early in geometry is a good feature of the work. Students have in decimal fractions and their applications been accustomed to incommensurable ratios, and we believe that such early use is not premature.

For the sake of discipline in clear and distinct statement and close consecution of thought, we admit a special liking to the first six books of Euclid. But Prof. Chauvenet's work more nearly satisfies our idea of a good geometry than any other modern textbook. H. A. N.

3. *Grundzüge einer allgemeinen Theorie der Oberflächen in synthetischer Behandlung;* von LUDWIG CREMONA. Translated into German by M. Curtze. Berlin, 1870. (S. Calvary & Co.)—This volume is ostensibly a translation of the memoirs of the distinguished Italian Geometer, but it is in fact much more. Rather more than half of the volume is new matter, added by Prof. Cremona, or under his supervision.

The work is an extension to surfaces of Prof. Cremona's method of dealing with plane curves, as shown in his memoir published by the Bologna Academy, *Introduzione ad una teoria geometrica delle curve piane.* This important work had previously been translated by M. Curtze into German.

These German translations render Cremona's important contributions to pure Geometry much more accessible than they are in the original Italian. H. A. N.

4. ***A historical and descriptive narrative of the Mammoth Cave of Kentucky,*** including explanations of the causes concerned in its formation, its Atmospheric conditions, its Chemistry, Geology, Zoology, etc.; with full scientific details of the eyeless Fishes; by W. STUMP NORWOOD, M.D. 225 pp. 12mo, with illustrations. Philadelphia, 1870. (J. B. Lippincott & Co.)—All future visitors to the Mammoth Cave will have occasion to thank Dr. Norwood for the service he has rendered the tourist. The book contains several interior views of the cave, which give some idea of the wonders within, although a very inadequate one. They should be supplemented by a good map of the extensive windings of the cavern. The author estimates the extent of the various avenues and mazes of the Mammoth Cave at over one hundred and fifty miles. An instrumental survey would probably shorten this assumed distance, but we know from our own wanderings in it that its extent is immense. The chapter on the eyeless fishes is made up mostly from the writings of Agassiz and Wyman, and presents the facts on the subject in an attractive form, well adapted to instruct and interest the general reader.

5. *Tent life in Siberia, and Adventures among the Koraks and other Tribes in Northern Kamtchatka and Northern Asia;* by GEORGE KINNAN. 425 pp. 12mo, with a map. New York, 1870. (G. P. Putnam & Sons, London; S. Low, Son and Marston).—A very readable and instructive narrative of travel in a little known region of the globe, is this neat volume of Mr. Kinnan's. The costly and disastrous enterprise of the Western Union Telegraph Company, designed to establish electrical communication between America and Asia through Kamtchatka and Northern Asia, has been by no means fruitless in scientific interest. Already Messrs. Whymple and Dall have given us instructive and valuable contributions upon their wanderings in British Columbia and Alaska. Mr. Kinnan makes no claim to any special devotion to scientific investigation, but his narrative gives a clear and lively account of the physical and social conditions of a country rarely visited, and full of curious interest. His volume well repays perusal, and is obviously the work of a good observer and careful historian, who is never tedious, and has the agreeable art of carrying his reader along with him.

6. *Report of the Superintendent of the U. S. Coast Survey, showing the progress of the Survey during the year* 1867. 344 pp. 4to, with 28 maps.—This volume, besides the Report of the Superintendent, Prof. Peirce, contains, among the articles in its Appendix, a Report on Transatlantic determination of Longitude, by Dr. B. A. Gould; on comparison of Meters, by Dr. F. A. P. Barnard and M. Tresca; on a new form of Reflector for geodetic signals, by J. E. Hilgard; on the Tides and Currents of Hell Gate, by Henry Mitchell; on Soundings in the Gulf Stream, by H. Mitchell; on the Fauna of the Gulf Stream, by L. F. Pourtales; on Alaska Territory, by G. Davidson; obituary of Alexander Bache.

7. ***Treatise on the power of Water as applied to drive Flour Mills and to give motion to Turbines and other Hydrostatic***

Engines; by JOSEPH GLYNN, F.R.S., member of the Institute of Civil Engineers of London, etc. 3d ed., revised and enlarged. 162 pp. 12mo, with numerous illustrations. New York, 1869. (D. Van Nostrand).—This is a popular and practical treatise on water as a motive power. It is simple in its explanations and mathematics, and is well illustrated.

8. *Archives of Science and Transactions of the Orleans Co. Society of Natural Sciences.*—This first number of the Archives of a new Natural History Society, at Newport, Vermont, contains the following papers: On the characters and customs of the Pawnees, by Rev. T. E. Ranney; Qualitative Analysis of the Mineral Springs of Essex Co., Vt., by H. A. Cutting; on the Indian History of Northern Vermont, by Wm. W. Grout, Esq.; Meteorological register, by J. M. Currier; on a new mounting for microscopic objects, H. A. Cutting.

9. *The American Entomologist and Botanist.*—This St. Louis Journal will be suspended during the year 1871, to be resumed with the commencement of the following year.

10. *Geology and Revelation, or the Ancient History of the Earth, considered in the light of Geological facts and Revealed religion,* with illustrations; by the Rev. GERALD MOLLOY, D.D., Prof. Theol. Roy. Coll. St. Patrick, Maynooth. 380 pp. 12mo. New York, 1870. (G. P. Putnam & Sons.)—A handsome American edition of the excellent work noticed at page 151 of this volume.

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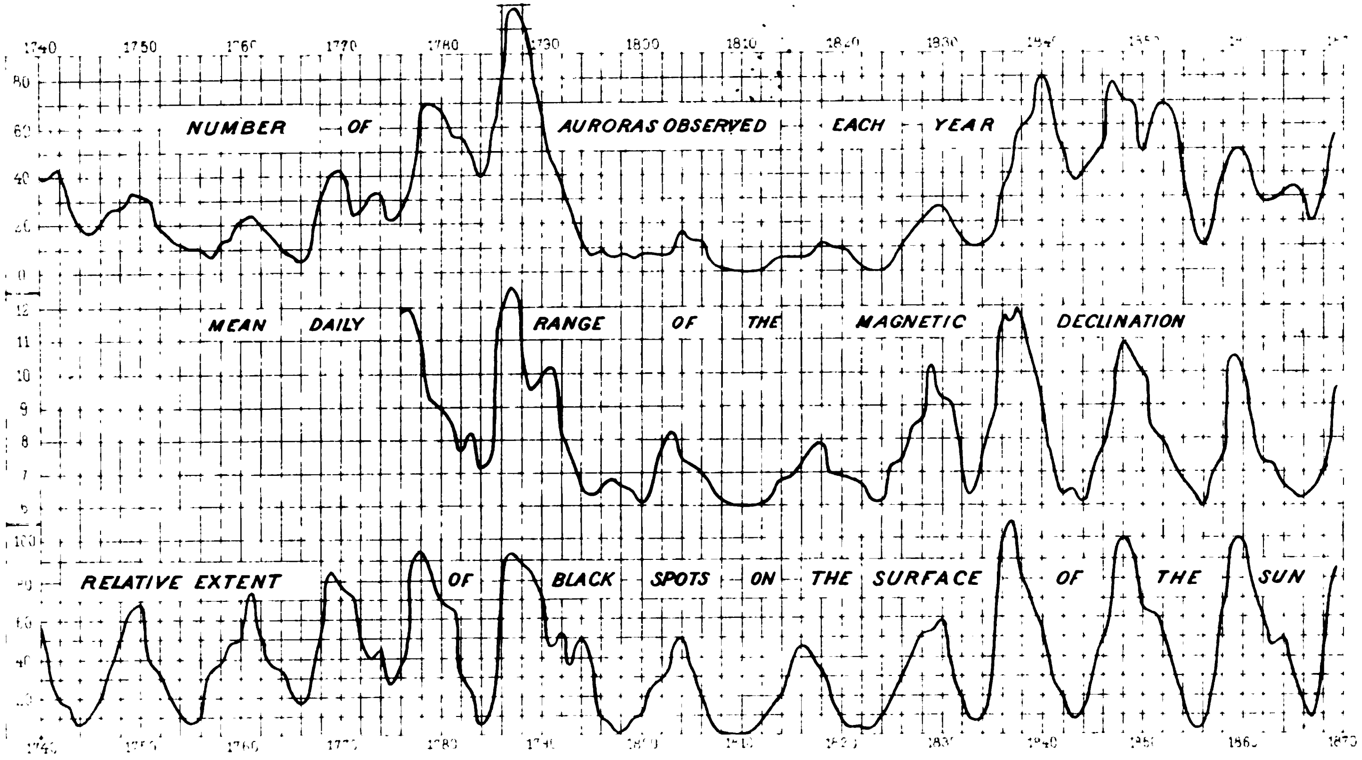
SOLAR SPOTS, MAGNETIC DECLINATION AND AURORAL DISPLAYS COMPARED.


NUMBER OF AURORAS OBSERVED EACH YEAR
MEAN DAILY RANGE OF THE MAGNETIC DECLINATION
RELATIVE EXTENT OF BLACK SPOTS ON THE SURFACE OF THE SUN
1740
1750
1760
1770
1780
1790
1800
1810
1820
1830
1840
1850
1860
1870
80
60
40
20
0
12
11
10
9
8
7
6
100
80
60
40
20